A Symposium Sponsored By
The Federal Reserve Bank of Kansas City

PRICE STABILITY AND PUBLIC POLICY

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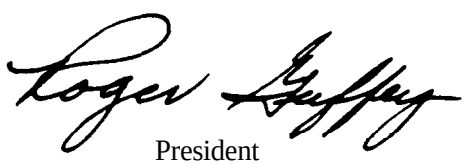
Contents

Foreword

Considerable progress has been made in recent years in bringing down the actual and expected rate of inflation. Despite this progress, inflation remains higher than has traditionally been thought of as consistent with the national goal of reasonable price stability. Moreover, lingering concerns about future acceleration of inflation continue to plague financial markets and the real economy.

In view of these concerns, a major public policy issue today is how to consolidate and extend past gains against inflation, while maintaining sustainable economic growth and a sound financial system. Accordingly, we decided to hold this, our seventh annual economic symposium, on the topic of 'Price Stability and Public Policy.'

To discuss this important public policy issue, we brought together leading authorities from academe and the private sector, as we have in our previous six symposia. We sincerely hope that these proceedings will be of interest and value to all who are concerned about the past and prospective consequences of inflation.



President
Federal Reserve Bank of Kansas City

The Contributors

Alan S. Blinder, Gordon S. Rentschler Memorial Professor of Economics, Princeton University A member of the Princeton economics faculty since 1971, Dr. Blinder has also served as deputy assistant director of the Congressional Budget Office's Fiscal Analysis Division and as a consultant to the Board of Governors of the Federal Reserve System and the Council of Economic Advisers. He also writes economics columns for the Boston Globe and the Washington Post. The author of five books, Dr. Blinder is associate editor of the Journal of Public Economics and has served on the board of other journals.

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and other positions for the Federal Reserve Board and the Federal Reserve Banks of Boston and New York. Dr. Friedman's recent research has focused on financial markets and their effect on the economy, and on the economic effects of financing government deficit. He is a senior adviser to the Brookings Panel on Economic Activity and associate editor of the Journal of Monetary Economics, and he is program director of the financial markets and monetary economics group of the National Bureau of Economic Research.

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Robert J. Gordon, professor of economics, Northwestern University. As a research associate at the National Bureau of Economic Research, Dr. Gordon was co-organizer of the NBER's annual International Seminar on Macroeconomics. Before joining the Northwestern faculty, he taught at Harvard and the University of Chicago. He is a senior adviser to the Brookings Panel on Economic Activity and a member of the executive committee of the American Economic Association. Dr. Gordon's published works include articles on inflation, unemployment, and productivity.

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Raymond Lombra, professor of economics, Pennsylvania State University. Dr. Lombra served as senior staff economist with the Board of Governors of the Federal Reserve System until 1977, when he joined the faculty of Pennsylvania State University. He has written or coauthored a number of articles, with his primary research interest in the demand for money, the term structure of interest rates, and other financial market issues.

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Allan H. Meltzer, John M. Olin Professor of Political Economy and Public Policy, Carnegie-Mellon University. A respected researcher and author in the field of money and capital markets, Dr. Meltzer has served as a consultant to the President's Council of Economic Advisers, the U.S. Treasury, and foreign governments and their central banks. He has written six books and more than 100 articles, emphasizing monetary theory and the transmission mechanism for monetary policy. Dr. Meltzer has held several visiting professorships here and abroad, and he is a founder and co-chairman of the Shadow Open Market Committee and the Shadow European Economic Policy Committee.

Frederic S. Mishkin, professor in the Graduate School of Business, Columbia University. Before coming to Columbia last year, Dr. Mishkin had been on the faculty of the University of Chicago since 1976. He has also served as an economist with the Federal Reserve Board, a member of the Brookings Panel on Economic Activity, and a faculty research fellow and research associate with the National Bureau of Economic Research. Dr. Mishkin' has published two books and numerous arrticles in professional journals.

William Nordhaus, professor of economics, Yale University. A member of the Yale faculty since 1967. Dr. Nordhaus has also served on the staff of the National Bureau of Economic Research and spent a year as a senior visitor at the University of Cambridge. He has been a member or senior adviser of the Brookings Panel on Economic Activity for more than a decade, and he serves in a variety of other positions with the National Academy of Science, the National Aeronautics and Space Administration, the Carnegie Endowment for International Peace, and the American Economic Association, among others. Dr. Nordhaus is the author, editor, or coauthor of five books and numerous articles in professional journals.

Helmut Schlesinger, deputy president, Deutsche Bundesbank. A member of the staff of the German central bank since 1952, Dr. Schlesinger has served as the Bundesbank's leading economist since 1980. In the 1950s and early '60s he headed the bank's economics and statistics section. After appointment to the directorate of the Bundesbank, he was in charge of credit policy at a time when European consumer credit services were being competitively sought by U.S. banks, and he was director of the domestic economy division in 1977, at a time when Germany had the most favorable financial conditions and lowest interest rates of any major industrial nation.

Robert J. **Shiller**, professor of economics and management, Yale University. A member of the Brookings Panel on Economic Activity and the National Science Foundation Panel on Economics, Dr. **Shiller** is also foreign editor of the Review of Economic Studies and associate editor of the ***Journal of Portfolio Management*** and the ***Journal of Econometrics***. Before coming to Yale, he taught—fulltime or as a visiting scholar—at the University of Minnesota, the University of Pennsylvania, the Wharton School at the University of Pennsylvania, and the Massachusetts Institute of Technology, where he earned his doctorate.

James **Tobin**, Sterling Professor of Economics, Yale University. Dr. **Tobin**, the 1981 Nobel laureate in economics for his work in portfolio theory, has been affiliated with Yale since 1950. He has served as a director of the Cowles Foundation and as chairman of the Economics Department. Dr. **Tobin** was a member of President **Kennedy's** Council of Economic Advisers and has served as president of the American Economic Association, the Econometric Society, and the Eastern Economics Association. He is also a member of the National Academy of Sciences, the American **Philo-**

sophical Society, the American Statistical Association, and the American Academy of Arts and Sciences. He has received several honorary degrees and published a number of books and journal articles.

The Moderators

Gardner Ackley, Henry Carter Adams Distinguished University Professor Emeritus of Economics, University of Michigan. Dr. Ackley has been a member of the University of Michigan economics faculty since 1941. But his career has also included 15 years in government, including chairman of the Council of Economic Advisers in 1964-67, and U.S. ambassador to Italy in 1968-69. Long active in the American Economic Association, he was elected its vice president in 1962 and its president in 1982. His honors include election to the American Philosophical Society and the American Academy of Arts and Sciences. His 1961 text on macroeconomics has been translated into many languages.

Paul W. McCracken, Edmund Ezra Day University Professor of Business Administration, University of Michigan. A member of the Michigan faculty since 1948, Dr. McCracken was a member of the Council of Economic Advisers during the Eisenhower administration, and he returned as chairman in 1969-72. Earlier, he had served as director of research at the Federal Reserve Bank of Minneapolis. Dr. McCracken is now chairman of the Council of Academic Advisers of the American Enterprise Institute for Public Policy Research, and he also serves on President Reagan's newly formed Economic Policy Advisory Board.

1
The Causes of Inflation

Frederic S. Mishkin

The problem of inflation has been of central concern to American policymakers since the mid 1960s. Of particular concern has been the rise in the core, or sustained, inflation rate from below the 2 percent level in the early 1960s to near the double-digit level by the late 1970s. Since 1981 a rapid disinflation has occurred, bringing the current inflation rate down to below 5 percent. The recent decline in inflation has not been achieved without substantial costs: In 1982, unemployment reached the highest level in the postwar period, peaking at 10.7 percent and is currently still above the 7 percent level. At the present time we are at a crucial juncture: The inflationary fire has abated, but there remains a persistent worry that it might reignite. What should be the stance of policymakers, and in particular the monetary authorities, in the current economic environment?

This paper attempts to provide some answers to this question by exploring why sustained inflations occur and the role of monetary policy in the inflation process.[1] The conclusion reached in this paper is that in the last ten years there has been a convergence of views in the economics profession on the causes of inflation. As long as inflation is appropriately defined to be a sustained inflation, macroeconomic analysis, whether of the monetarist or Keynesian persuasion, leads to agreement with Milton Friedman's famous dictum, "Inflation is always and everywhere a monetary phenomenon."[2] However, the conclusion that inflation is a monetary phenomenon does not settle the issue of what causes inflation because we also need to

I thank Bob Cumby and participants at the Symposium for their helpful comments. This research has been supported by the Sloan Foundation. The usual disclaimer applies.

1. Temporary movements of the inflation rate have been substantial in the 1970s because of the external supply shocks due to the increase in oil prices in 1973 and 1979. This paper does not focus on these temporary movements of inflation because they are strongly influenced by external factors that are not under the control of the monetary authorities. See Blinder (1979) for a discussion of how supply shocks temporarily raised inflation in the 1970s.

2. Friedman (1963).

understand why inflationary monetary policy occurs. This paper will also examine this issue and by so doing provide some suggestions as to how monetary policy should be conducted in order to prevent the resurgence of inflation at a minimum cost in terms of unemployment and output loss.

Inflation as a monetary phenomenon

The most persuasive evidence that Friedman cites to support his proposition is the fact that in every case where a country's inflation rate is high for any sustained period of time, its rate of money supply growth is also high. This evidence for the decade spanning 1972-82 is shown in the scatter diagram in Figure 1, which plots the average rate of inflation for 52 countries against the average rate of money growth in this period.[3] The well known relation between money growth and inflation is illustrated by the regression line plotted in the figure, and the correlation between inflation and money growth is found to be 0.96. The country with the highest rate of inflation in this period, Argentina, is also found to have the highest rate of money growth, while the country with the lowest rate of inflation, Switzerland, is also the country with the lowest rate of money growth.

An important feature of this evidence is that it focuses on sustained or core inflation, that is, a situation where the price level is continually rising. Friedman's sweeping statement that inflation is always and everywhere a monetary phenomenon thus focuses on the ***long-run*** phenomenon of inflation and is ***not*** concerned with temporary inflations in which the upward movement in the price level is not a continuing process. If Friedman's proposition did refer to temporary inflations, then it could easily be refuted by numerous counter examples.

The distinction between sustained and temporary inflations is an important one in evaluating Friedman's proposition. Although articles in the popular press seem to indicate that monetarists and Keynesians have a completely different view of the inflation process, this is not the case. Keynesian macro theory as it is currently practiced, as well as monetarist analysis (and its offshoot, the new classical macroeconomics advocated by Lucas and Sargent), all support Friedman's proposition that sustained inflations are monetary phenomena.

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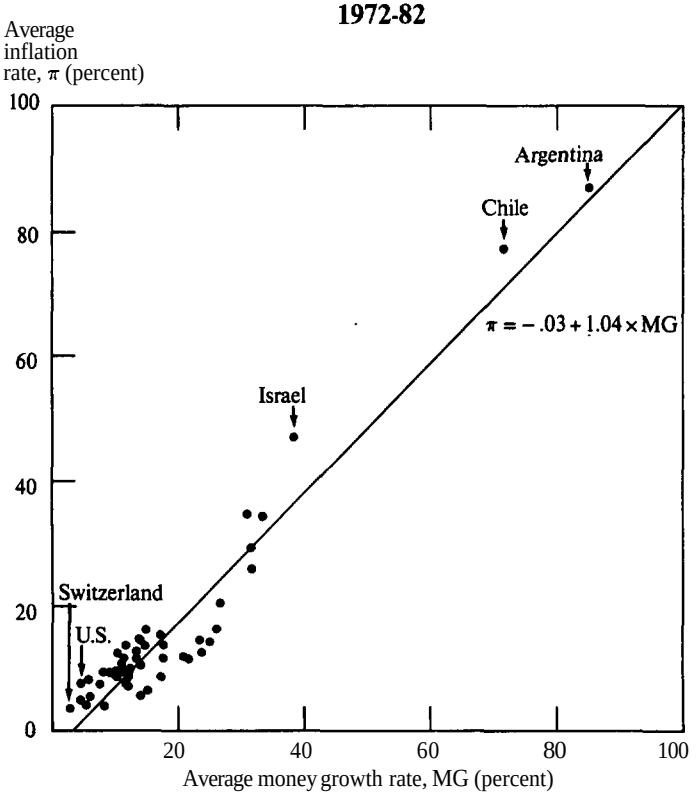
3. These are the 52 countries for which money supply, price level and real output data were available in the IMF's *International Financial Statistics*. A quantity theory view of money growth and inflation would make use of a money growth variable that is adjusted for real output growth by subtracting real output growth from money growth. As expected, the adjusted money growth measure is more highly correlated with inflation than is the unadjusted money growth variable used in the text: The correlation of the adjusted money growth variable with inflation for the 52 countries is .98.

FIGURE 1

Inflation and Money Growth in 52 Countries 1972-82

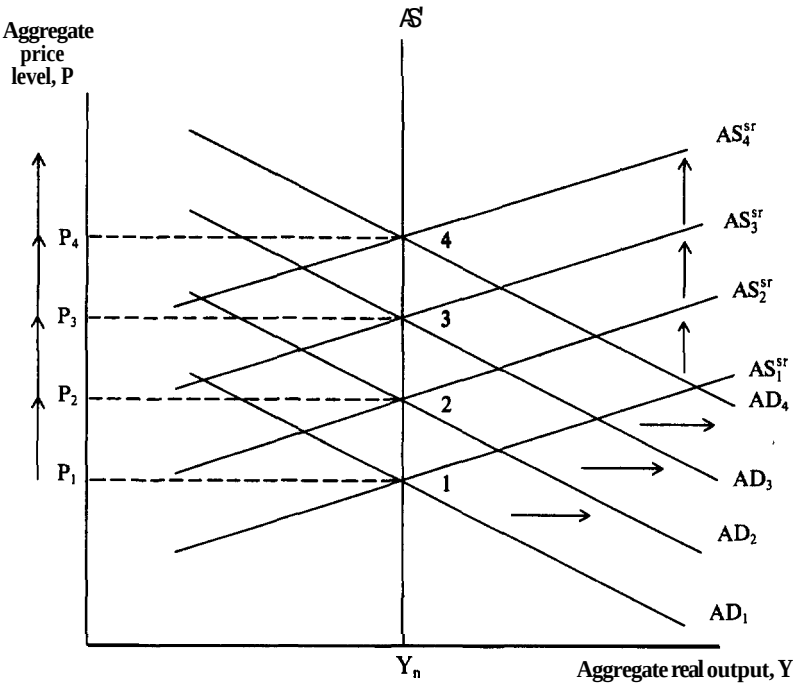


Source: The data used in constructing the inflation and money growth numbers were obtained from the IMF's *International Financial Statistics Annual Yearbook 1983.* Consumer price indices were used to calculate the inflation rates and narrowly defined money was used to construct the money growth rates. The average growth rates were calculated by taking the log of the 1982 value of the CPI or money supply, subtracting off the log of the 1972 value, and then dividing by 10. All data are at annual rates, continuously compounded.

The best way to see the wide theoretical support behind the Friedman proposition is to make use of the aggregate supply and demand framework to see how each of the three major paradigms in macroeconomic analysis view the inflationary process. Figure 2 contains the aggregate supply and demand diagram that shows the response of prices and output to a continually rising money supply,

FIGURE 2

The Response of Prices and Output to a Continually Rising Money Supply



Let us first consider how this diagram works in the context of the monetarist model. Suppose that initially we are at Point 1, where the price level is P_1 and real output is at the natural rate level of output, Y_n, which is the level of real output that corresponds to the natural rate of unemployment. The initial aggregate demand curve, AD_1, is downward sloping in the monetarist model because nominal income is fixed by the level of the money supply, and any decline in price level means that there must be a corresponding rise in output. The initial short-run aggregate supply curve, AS_1^{sr}, is upward sloping because a rise in nominal income yields a rise in both real output and the price level in the short-run. In the long run, however, real output will be at its natural rate level, Y_n; hence the long-run aggregate supply curve is the vertical line AS^{lr} at the real output level of Y_n. The diagram has been drawn so that initially the aggregate demand and short-run aggregate supply curves intersect at Point 1, which is also on the long-run aggregate supply curve.

When the money supply increases, the monetarist model predicts that nominal income will rise, thus shifting out the aggregate demand curve to AD_2. At first we might have an increase of real output above the natural rate level, but the resulting decline in unemployment below the natural rate will create upward pressure on wages and prices, thus leading to a continuing shift up in the short-run aggregate supply curve until it reaches AS_2^{sr}, where the economy is again back at the natural rate level of output. The price level has now increased to P_2 where the aggregate demand and supply curves intersect at Point 2. A further increase in the money supply next period shifts the aggregate demand curve out to AD_3, and the economy moves to Point 3 and a higher price level of P_3. Continuing increases in the money supply send the economy to Point 4 and beyond. The net result of this process is that a continuing rise in the price level, that is, a sustained inflation, results from a growing money supply. In the monetarist model, the aggregate demand curve shifts only as a result of changes in the money supply and so, in the absence of a high rate of money growth, sustained inflation cannot develop. Friedman's proposition that inflation is a monetary phenomenon then follows.

The Keynesian analysis of the response of output and prices to a continually rising money supply is almost identical to the scenario just described for the monetarist model, The Keynesian model also has a downward sloping aggregate demand curve because for a given money supply a decline in prices raises real money balances, lowers interest rates, and thereby raises aggregate demand. In addition, this downward slope in the aggregate demand curve can result from real balance effects in which the decline in the price level raises the real value of wealth, thereby increasing aggregate demand. The upward sloping short-run aggregate supply curve and the vertical long-run aggregate supply curve, AS^{lr}, are also features of the Keynesian model. The Keynesian model differs in its treatment of aggregate supply from the monetarist model in that it views the speed of adjustment of the short-run aggregate supply curve to its long-run position as being slower than in the monetarist model. While monetarists see the economy as inherently stable with a rapid adjustment to the natural rate level of output, Keynesians see the economy as inherently unstable, with a much slower adjustment to the natural rate level of output.

A rise in the money supply in the Keynesian model also leads to the aggregate demand curve shifting out to AD_2 because at a given price level real money balances rise, leading to both a decline in interest rates and a rise in the real value of wealth, thus causing aggregate demand to rise. The economy will again head to Point 2 because the short-run aggregate

supply curve will continue to rise until it reaches AS_2^{sr}, where output is at its natural rate level. Further increases in the money supply will move us to Point 3, 4, and so on. The Keynesian model thus also reaches the conclusion obtained from the monetarist model: A continuing rise in the price level, that is, a sustained inflation, will result from a rapid growth of the money supply.

The Keynesian model, in contrast to the monetarist model, does allow other factors besides the money supply to affect the aggregate demand curve, specifically fiscal policy. Thus, at first glance, it would seem that a sustained inflation might occur as a result of expansionary fiscal policy, such as increased real government spending or decreases in taxes, and that the Friedman proposition would be refuted. However, this is not the case. Even in the Keynesian model, a sustained inflation cannot result unless there is a rapid growth in the money supply.

Suppose that the economy is initially at Point 1 in Figure 2 and government spending is permanently increased, shifting out the aggregate demand curve to AD_2. Initially, output will rise above the natural rate level, leading to a rise in the short-run aggregate supply curve to AS_2^{sr}, where output is again at Y_n and the price level has risen to P_2. The net result from the permanent increase in government spending is a one-shot, permanent increase in the price level. While the economy is moving from Point 1 to Point 2, the inflation rate will be high. Once Point 2 is reached, however, the inflation rate will return to zero. Thus, the permanent increase in government expenditure leads to only a *temporary* increase in inflation.

In the absence of rapid money growth, a permanent increase in government expenditure cannot lead to a continually rising price level and hence to a sustained inflation. Only a continuing rise in government expenditure can lead to shifts in the aggregate demand curve to Points 3, 4, and so on, yielding a sustained inflation. Such a policy, however, is not a feasible one because there is a limit on the total amount of government expenditure possible: The government cannot spend more than 100% of GNP. In fact, well before this limit is reached, the political process would stop the increase in government expenditure. As is visible in recent congressional debates about the budget, the public and politicians have a particular target level of government spending that they think is appropriate for our society. Although small deviations from this level might be tolerated, large deviations will not be, imposing even tighter limits on the degree to which government expenditures can be increased.

By a similar argument, lowering taxes also cannot lead to sustained inflation in the absence of rapid money growth. A permanent decline in

taxes can shift the aggregate demand curve from AD_1 to AD_2. But further outward shifts in the aggregate demand curve can occur only if taxes are continually reduced. This process will obviously have to stop when tax collections are zero. The outward movements of the aggregate demand curve will thus eventually also have to come to a stop, and the resulting inflation will necessarily be temporary. The conclusion we have reached is the following. ***Even in a Keynesian model, fiscal policy cannot by itself be the source of sustained inflation.*** The Keynesian framework therefore also supports the Friedman proposition.

The new classical macroeconomics also can be cast in the aggregate demand and supply framework of Figure 2. The advocates of new classical macroeconomics lean to Milton Friedman's position that money is all that matters to changes in nominal income, although they are willing to entertain the possibility that other factors influence the aggregate demand curve. The principal difference between them and monetarist or Keynesian economists is in their views of aggregate supply. The new classical macroeconomics combines the assumption of market clearing (because wages and prices respond completely flexibly to the appearance of new information) with the assumption of rational expectations. Any changes in the aggregate demand curve that are anticipated will lead to changes in the short-run aggregate supply curve that leave real output unchanged. The resulting neutrality of anticipated policy does not affect any of the conclusions reached above. New classical macroeconomics is also consistent with the view that inflation is always and everywhere a monetary phenomenon.

The causes of inflationary monetary policy

To understand the process generating sustained inflation, it is not enough to know that a sustained inflation will not occur without a high rate of money growth. We also must understand why governments pursue inflationary monetary policies. Because politicians and government policymakers never advocate inflation as a desirable outcome, it must be that in trying to achieve other goals, governments end up with a high money growth rate and thus a higher inflation rate. There are two goals that may lead to inflationary monetary policy: high employment, and the desire to have high government spending with low taxes.

High employment targets and inflation

The U.S. government is required by law, in the Employment Act of 1946, as well as the more recent Humphrey-Hawkins Act of 1978, to promote high employment. It is true that both of these laws state that a high

employment level is to be achieved that is consistent with a stable price level, but in practice this has often meant that our government has pursued a full employment target with less concern about the inflationary consequences of its policies.

One result of pursuing a full employment target is that the government will engage in an activist stabilization policy to promote high employment, using monetary and fiscal policy to raise real output and employment when they fall below their natural rate levels. How this activist policy can lead to a high rate of money growth and inflation is again illustrated with the aggregate supply and demand apparatus in Figure 3. Consider a situation in which initially output in the economy is at the natural rate level at Point 1, where the aggregate demand curve, AD,, and the short-run aggregate supply curve AS_1^{sr}, intersect. If unions and firms decide that they want to obtain higher wages and prices and so raise them, the short-run aggregate supply curve will rise to a position such as AS_2^{sr}. With government monetary and fiscal policy unchanged, the economy would move to Point A and output would decline to below its natural rate level. When unemployment rises as a result, activist policymakers with a high employment target would accommodate the higher wages and prices by implementing expansionary monetary or fiscal policy that would raise the aggregate demand curve to AD_2, thus raising output back up to its natural rate level.

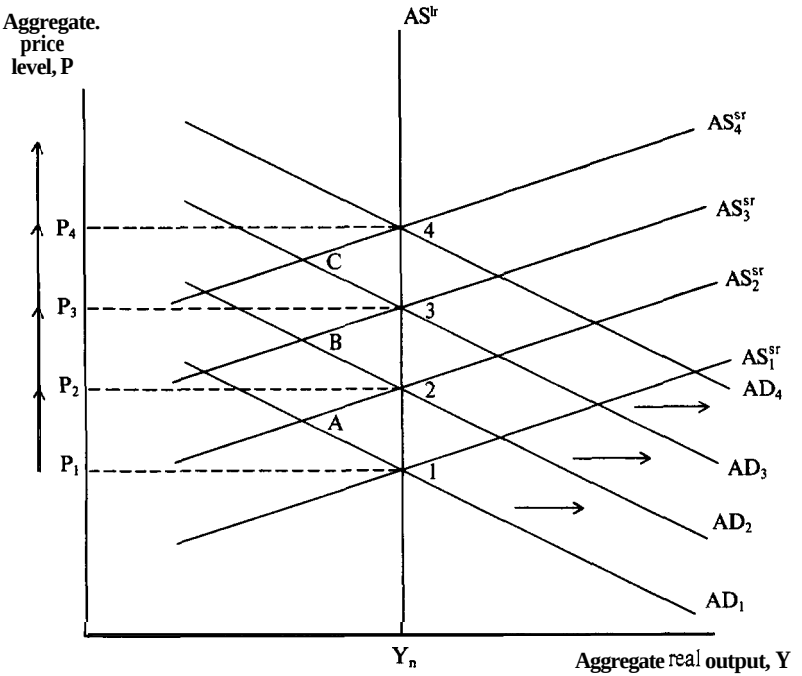
The consequence for the workers and firms is that they have achieved their goal of higher wages and prices without the appearance of too much unemployment. As a result they might want to try to raise their wages and prices again. In addition, other workers and firms might also raise their wages and prices in order not to be left behind and suffer a decline in their relative wages and prices. The net result will be that the short-run aggregate supply curve will shift up again, say to AS_3^{sr}. Unemployment would rise again when the economy moves to Point B, and accommodating, activist policy will now again be used to shift the economy to Point 3 by shifting the aggregate demand curve out to AD_3.

The above process can keep on continuing, and the price level will keep on rising, sending us to Point 4 and beyond. The sustained inflation that results is known as cost-push inflation because it has been triggered by the push of workers and firms to raise their wages and prices.

At first glance, it might appear as though the cost-push inflation provides a counter example to the Friedman proposition that inflation is a monetary phenomenon. This is not the case because in order for a sustained inflation to occur, the aggregate demand curve has to shift out continually, and as the earlier discussion indicates, this can occur only if the

FIGURE 3

A Cost-Push Inflation with an Activist Policy to Promote High Employment



money supply is continually rising. If a non-accommodating monetary policy is followed because the government is not bound to a high employment target, then the upward push of wages and prices that raises the short-run aggregate supply curve from AS_1^{sr} to AS_2^{sr} will not be followed by expansionary policy to shift the aggregate demand curve outward; instead the aggregate demand curve will remain at AD,. Now when the economy moves to Point A and unemployment develops there will be pressure on wages and prices to fall. The aggregate supply curve will begin to shift back down to AS_1^{sr}, and eventually the economy will return to Point 1, where output is at the natural rate level and the price level has returned to its initial value of P_1. A continuing rise in the price level does not occur.

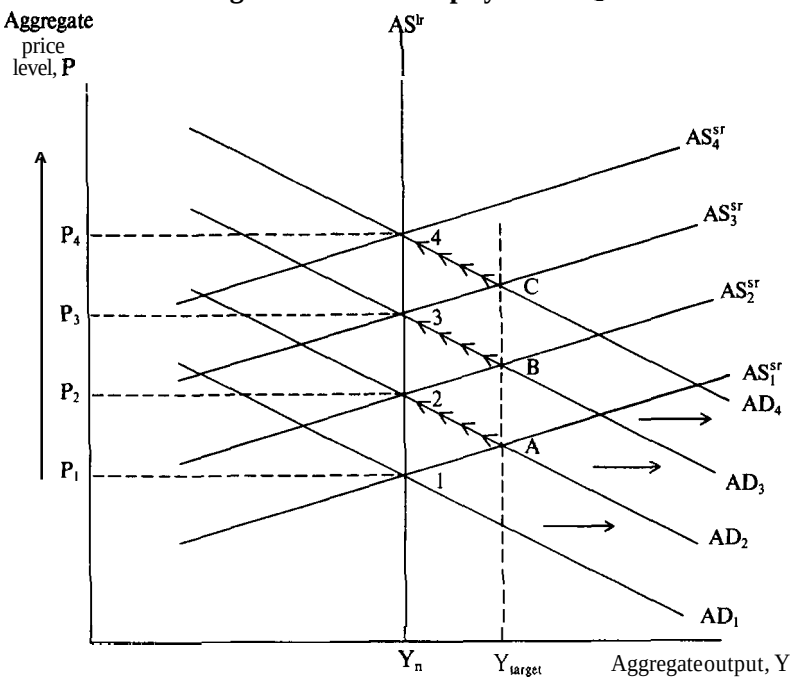
The conclusion of this analysis is that an attempt by workers and firms to push up their wages and prices cannot by itself trigger sustained inflation. Policymakers have to lend a hand by pursuing an accommodating, activist policy of eliminating high unemployment with expansionary monetary policy. Another way of stating this is the following. ***Sustained*** *cost-*

push inflation is also a monetary phenomenon because it cannot occur without the acquiescence of the monetary authorities to a higher rate of money growth.

There is a second way that pursuing the goal of high employment can lead to inflationary monetary policy: policymakers can set a target for unemployment that is too low because it is below the natural rate of unemployment. The consequences of a policy of too low an unemployment target is depicted in Figure 4.

FIGURE 4

A Demand-Pull Inflation as a Consequence of Setting Too Low an Unemployment Target



Because the policymakers target on a level of unemployment below the natural rate level, the targeted level of real output, marked as Y_{target} in Figure 4, is above the natural rate level of output, Y_n. If the economy is initially in long-run equilibrium, Point 1, the policy authorities will feel that there is too much unemployment because output is less than the target level. In order to hit their output target, the policymakers will conduct an expansionary policy that will shift the aggregate demand curve out to AD_2 and the economy will move to Point A. Because unemployment is now

below the natural rate level, wages and prices will begin to rise, shifting the short-run aggregate supply curve up to AS_2^{sr} and sending the economy to Point 2. The price level has now risen from P_1 to P_2, but the process will not stop there. The economy is still operating at an output level below the target, and so the policymakers will shift the aggregate demand curve out again, this time to AD_3. The economy will eventually head to Point 3, and policymakers will again shift the aggregate demand curve outward, sending the economy to Point 4 and beyond.

The discussion above indicates that the aggregate demand curve can be continually shifted outward only by a higher rate of money growth, and so the sustained inflation that results from too low an unemployment target (or equivalently too high an employment target) is again a monetary phenomenon. This type of inflation is characterized as demand-pull inflation because it arises from the conscious effort to shift out the aggregate demand curve. Clearly, policymakers do not intend to start demand-pull inflations because they do not gain a permanently higher level of output.[4] Demand-pull inflations can be explained, however, by the fact that policymakers may mistakenly think that the target level of output is not above the natural rate level. Before they realize their mistake, they would have started the process that we see in Figure 4.

Although theoretically we can distinguish between demand-pull and cost-push inflation, it is much harder to label particular episodes of inflation. Both types of inflation are associated with high rates of money growth so they cannot be distinguished on this basis. However, as Figures 3 and 4 indicate, demand-pull inflation will be associated with periods when output is above the natural rate level, while cost-push inflation is associated with periods when output is below the natural rate level. It would then be quite easy to distinguish which type of inflation is occurring—if we knew what the value of the natural rate of unemployment or output is. Unfortunately, the economics profession has not been able to ascertain the value of the natural rate of unemployment or output with a high degree of confidence.

In any case, the distinction between demand-pull and cost-push inflation is not important. Whether it is the government or workers and firms that initiates the inflation is irrelevant; the ultimate source of either type

4. In the aggregate supply and demand diagram above, it might appear as though a higher level of output can be achieved at the cost of a higher rate of inflation. Recent evidence that finds that the long-run Phillips curve is vertical rules out such a long-run tradeoff between inflation and unemployment.

of inflation is the commitment of the government to a high employment target.

Budget deficits and inflation

Frequently, a government cannot or does not find it politically feasible to raise taxes when it needs to increase government spending. This appears to be the situation for such Latin American countries as Argentina, and this was clearly the situation that occurred during the 1921-23 German hyperinflation. Similarly, during wartime, the need to rapidly increase military spending results in government expenditures rising faster than tax revenues. Alternatively, the desire to reduce taxes in the face of continuing high level of government spending can also lead to large budget deficits, as currently is the case in the United States.

Large budget deficits can also be the source of inflationary monetary policy. When a government is running a budget deficit, it must finance it in either of two ways: It can issue bonds, or it can resort to the printing press by expanding the amount of high-powered money. The first method of financing the deficit does not have an independent effect on the aggregate demand curve separate from any direct tax or government spending effects, and so it should not have any inflationary consequences. The second method does lead to a continually growing money supply if the budget deficit persists for a substantial period of time. In the first period, the rise in high-powered money leads to a rise in the money supply that shifts the aggregate demand curve out to the right, as in Figure 2. In subsequent periods, if the budget deficit is still present, then it has to be financed again, leading to a rise in high-powered money, a rise in the money supply, and another outward shift in the aggregate demand curve. Sustained inflation will thus occur if a large budget deficit is persistent and if it is financed by issuing high-powered money.

The key question that requires an answer in order to understand the link between budget deficits and inflation is why do governments with budget deficits finance them by creating high-powered money rather than by issuing bonds? If a government does not have access to a capital market that can absorb its bonds in substantial quantities, then the answer is straightforward. The only way the budget deficit can be financed is by printing money. This appears to be the situation in Latin American and many other developing countries, and in these countries the link between budget deficits and inflationary monetary policy is quite clear.[5]

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5. For example, see Arnold Harberger (1978).

Even in a country where well developed capital markets exist that can absorb substantial quantities of bonds, if the budget deficit is a sufficiently large fraction of GNP and is permanent, a policy of pure bond financing will be dynamically unstable, leading to an explosion in the stock of debt. Once the public recognizes that this will occur, then the government will not be able to sell enough of its bonds to completely finance the deficit and will be forced to issue high-powered money.[6]

The case for an important role of budget deficits in the inflationary process is much less clear-cut when the economy has a well developed bond market in which the government can sell its bonds, and when the size of the budget deficit is small relative to GNP Although a government may not have to finance its deficit by increasing the amount of high-powered money, it still may end up doing so because it has a goal of preventing rises in interest rates. A common view is that budget deficits, which require the issuing of a large amount of government bonds, raise the level of interest rates. This view has intuitive appeal because in a usual supply and demand analysis of the bond market, the increased supply of bonds resulting from a deficit leads to a decline in bond prices and hence a rise in interest rates. If this rise in interest rates is considered undesirable, the monetary authorities might try to prevent it by purchasing bonds to prop up their price and by so doing increase the amount of high-powered money. This monetization of the debt will then lead to a continuing rise of the money supply if the deficit persists and so will lead to inflation through the mechanism depicted in the aggregate supply and demand diagram of Figure 2.

The evidence that budget deficits have led to higher interest rates in the U.S. is not strong. This might be the result, however, of inappropriate measurement of the budget deficit. The National Income Accounts deficit, the deficit number that is most widely cited in the popular press, is a particularly flawed measure of the government budget deficit because it does not make any correction for inflation. Although in the period from 1946 to 1980 there were some substantial deficits on a National Income Accounts basis, when corrected for inflation these deficits disappear? This is reflected in the fact that the real per capita level of net federal debt has fallen steadily from 1946 to 1980. Only in the last few years have we begun to see large budget deficits (correctly measured) and a rise in the level of federal debt as a fraction of GNP Thus it is not surprising that the past search for

6. *See* Sargent and Wallace (1981) and McCallum (1982).
7. *See* Eisner and Pieper (1984).

higher interest rates as a result of budget deficits in the United States has not found strong supporting econometric evidence.

The current Reagan budget deficits, even when measured correctly, are unprecedently high for the postwar period. If these deficits persist, we then may find stronger evidence in the future that budget deficits do matter to the level of interest rates and therefore have a potentially stimulative effect on monetary policy.[8] The evidence on the link between budget deficits and inflationary monetary policy is, however, inconclusive at the present time.

The rise in core inflation in the US.

The above analysis provides us with some clues as to why the core inflation rate rose from the early 1960s to the late 1970s. Because the inflation-adjusted budget deficit was never substantial during this period, there is little support, either on a theoretical or an empirical basis, for budget deficits as the source of the rise in the core inflation rate. This leaves high employment targets as the other candidate for the underlying cause.

A likely scenario for what triggered the rise in core inflation in the 1965-73 period is that policymakers pursued an overly high employment target. In the mid 1960s, policymakers, economists, and politicians became committed to a target unemployment rate of 4 percent because they thought that this level of unemployment was consistent with price stability. In hindsight, most economists now agree that the natural rate of unemployment was above this figure and was steadily rising in the late 1960s and '70s because of demographic shifts in the composition of the labor force and increased coverage of unemployment insurance programs. The activist policy during the Johnson and Nixon administrations, which pursued unemployment targets that were too low (and thus employment targets that were too high), might then be the primary reason why a temporary inflation resulting from the Vietnam war buildup in the mid 1960s was converted into a sustained rise in inflation along the lines of Figure 4.

The attempt of workers and firms to obtain higher wages and prices could also have been a factor in the rise of the core inflation rate, but it is important to remember that these cost-push elements of inflation could not have occurred without the accommodating, high-employment policy of the monetary authorities shown in Figure 3. The persistence of the high

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8. Blanchard and Summers (1984) make the case that when viewed in an international context, the currently high budget deficits in the U.S. are not the source of the current high levels of real interest rates. Thus, their analysis casts some doubt on the position that the current U.S. budget deficits will ultimately prove to be inflationary.

core inflation rate into the late 1970s can be attributed to workers' and firms' knowledge that government policy continued to be concerned with achieving high employment; they thus continued to raise their wages and prices because they expected accommodating policy. This raises the issue that expectations are an important element in the inflationary process and leads us to the role of credibility of policymakers in eliminating and preventing inflation.

Credibility and expectations in the anti-inflation process

Monetarists have always been leery of activist policy because they see the economy as inherently stable and because there is some uncertainty about the timing of monetary policy effects (long and variable lags). They thus see activist policy as likely to do more harm than good. Keynesians, on the other hand, are much less sanguine about the stability of the economy since they view price and wage adjustment as proceeding quite slowly because of rigidities such as long-term contracts. Does this mean that an activist policy of preventing high employment is desirable? The answer depends crucially on whether expectations are important in the wage and price setting process.

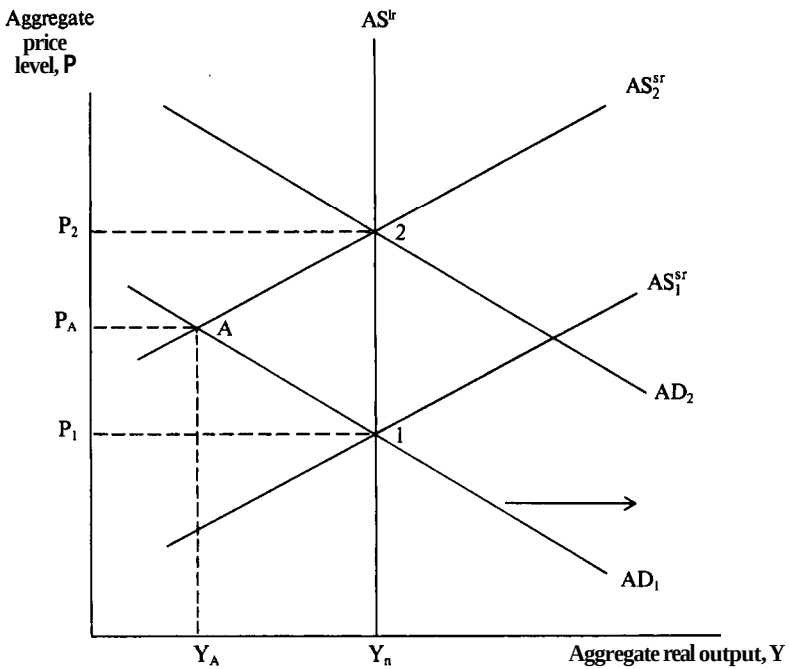
Figure 5 depicts a situation where the economy has moved to excessive unemployment at Point **A** as a result of an upward shift in the short-run aggregate supply curve from AS_1^{sr} to AS_2^{sr}. This upward shift could arise from an attempt by workers and firms to raise their wages and prices, or it could arise from a supply shock of the type we experienced in 1973 and 1979. **A** non-activist policy that left the aggregate demand curve at AD_1 and allowed high unemployment would eventually drive the short-run aggregate supply curve back down to AS^{sr}, and real output would be restored to the natural rate level. In the monetarist or new classical macroeconomic view of their world, this adjustment would take place quickly, and so the non-activist policy would have low cost. To a Keynesian, the adjustment process would be very slow, and substantial output loss would result from the non-activist policy. Since the tendency to return to the natural rate of output is too slow, the only way to eliminate the excessive unemployment quickly is to shift out the aggregate demand curve to AD_2 to move the economy to Point 2.

In an economy where expectations do not matter to wage and price setting behavior, this accommodating, activist policy is optimal if the adjustment to the natural rate of output is slow. In an economy where expectations do matter to wage and price setting, however, we must ask two questions: Will the economy remain at Point 2 after the accommodating

policy has been executed, and will the economy be any more likely to move from Point 1 to Point A in the first place if workers and firms expect this high employment policy?

FIGURE 5

An Activist Response to Unemployment



As we have seen in Figure 3, the accommodating policy that moves the economy from Point A to Point 2 may encourage workers and firms to raise wages and prices further, thus leading to a sustained inflation. In addition, if workers and firms know that an accommodating policy is going to be pursued, they will be more likely to try to raise their wages and prices in the first place, thus moving the economy to a situation like Point A with high unemployment. Because of these two possibilities, there is a hidden cost to the activist high employment policy.

The problem with the accommodating, activist policy is the dynamic inconsistency of such a policy described by Kydland and Prescott (1977). Although the first time that unemployment develops eliminating it with

an activist policy may be optimal, the expectations that this activist policy creates leads to a suboptimal outcome of higher inflation and even possibly higher unemployment as well. A hidden benefit of a non-activist, non-accommodating policy is that movements to Point A in Figure 5 may occur less often as workers and firms recognize that there will be substantial costs in terms of persistent high unemployment as a result of any attempts to raise wages and prices.

Two non-economic examples illustrate why non-accommodating policies may be optimal as a result of dynamic inconsistency of accommodating policy. First is a problem that I have recently experienced as a new father with a two-year-old son. I have an office in my house where I do much of my work. Whenever I went into this office, my son would bang on the door and cry. The first time he did this, it was optimal for me to pursue an accommodating policy of going out to him. Unfortunately, he would keep on coming back to the door and disrupting my work. Having read Kydland and Prescott's paper, I recognized that I would be better off pursuing a non-accommodating policy. (Who says economics isn't useful?) Sure enough, after not going out to him several times when he came to the door—a wrenching experience because of his crying—he stopped coming back. Now as a result of my non-accommodating policy, I can work in peace in my office.

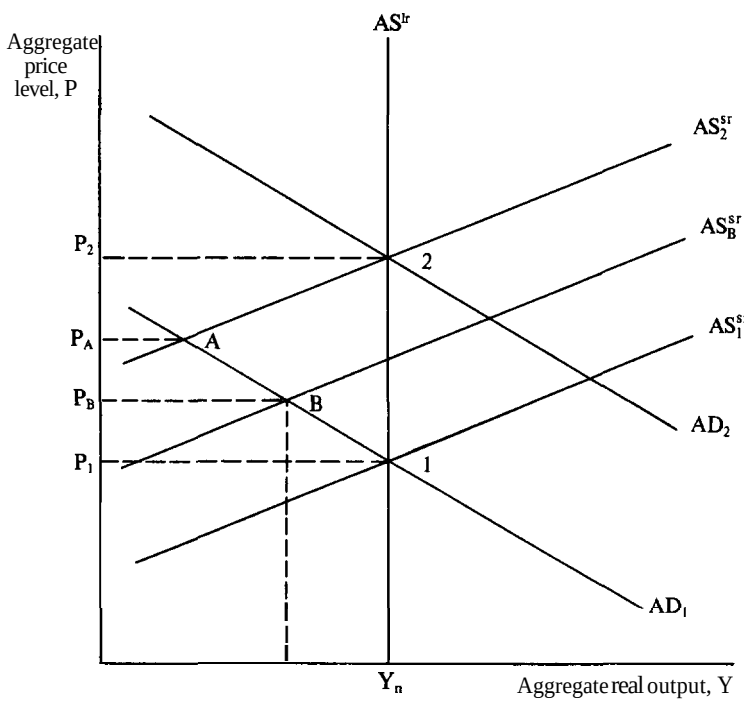
A second example is relevant to the appropriate way to conduct foreign policy. When Hitler threatened war if he were unable to dismember Czechoslovakia, it may have appeared optimal to pursue the accommodating policy of obtaining peace at any price. Unfortunately, this just whetted Hitler's appetite for more territorial acquisitions and encouraged him to invade Poland. In hindsight, the world would have been better off if the allies had pursued a non-accommodating policy and stopped Hitler earlier.

A non-accommodating policy will be most successful if economic agents expect it, that is, if the non-accommodating policy is credible. In the case of Figure 5, knowing that the aggregate demand curve will not be shifted out if the economy is pushed to Point A will make it less likely that the economy will end up at Point A; workers and firms now recognize that pushing up the aggregate supply curve will entail substantial costs. If credibility of a non-accommodating policy is not achieved and it is then actually pursued, we have the unhappy outcome of stagflation in which both prices and unemployment rise because movement to Point A in Figure 5 is a likely possibility. The undesirable outcome of a non-credible, non-accommodating policy had even more serious consequences in 1939 when World War II began.

What if we are already experiencing a rapid inflation? What role does credibility play in the success of an anti-inflation policy? Again we can use the aggregate supply and demand framework to analyze the response to an anti-inflation policy. Figure 6 depicts a sustained inflation in which the economy is moving from Point 1 to Point 2 each period and the inflation rate is built into wage and price contracts so that the short-run aggregate supply curve is rising at the same rate as the aggregate demand curve. Consider the announcement of a cold-turkey anti-inflation policy where money growth will be reduced sufficiently so that the aggregate demand curve will remain at AD_1 and will not shift out to AD_2. If this anti-inflation policy is not credible, the short-run aggregate supply curve will continue to rise to AS_2^{sr} when the policy is implemented. The result is that the economy will move to Point A, where there is some slowing of inflation (the price level does not rise all the way to P_2), but there is substantial output loss.

FIGURE 6

Anti-Inflation Policy and Credibility



If, on the other hand, the announced cold-turkey policy is believed because the policymakers have credibility, a much more desirable outcome can result. If expectations of future policy do enter into workers and firms wage and price setting decisions, then the announcement of the credible cold-turkey policy will cause the short-run aggregate demand curve to rise less than it otherwise would. In an economy where expectations of future policy do matter but wage and price contracts impose some wage and price rigidity on the economy, the aggregate supply curve will not rise to AS_2^{sr} but instead will rise only to AS_B^{sr}. Here the economy moves to Point B and does experience a loss in output, but this loss is less than is experienced when the policy is not credible; in addition, the decline in inflation is more rapid (the price level rises only to P_B rather than to P_A). Credibility is thus an important element to a successful anti-inflation policy.[9]

This conclusion is even stronger in the context of the new classical macroeconomics model. In this model, there is sufficient wage and price flexibility so that the short-run aggregate supply curve responds fully to changes in expectations about future policy: the announcement of the credible cold-turkey policy will cause the short-run aggregate supply curve to remain at AS_1^{sr}. Thus, when the cold-turkey policy is implemented, the economy will remain at Point 1, with the happy outcome of an inflation rate that has returned to zero, and it is achieved with no output loss.

The crucial element required for credibility to matter to the success of anti-inflation policy is that expectations of policy affect the position of the short-run aggregate supply curve. The notorious instability of the Phillips curve provides indirect evidence that expectations about future policy matter to aggregate supply. More direct tests such as Lucas (1973) also support the importance of expectations to aggregate supply. The evidence on whether short-run aggregate supply responds fully to changes in expectations about future policy is more mixed, however.[10]

Strong direct evidence supporting the importance of credibility to a successful anti-inflation program has been provided by Sargent (1982). In the four hyperinflations that Sargent studies, inflation was eliminated quickly with little apparent output loss. A key characteristic of these successful cases of anti-inflation policy is their credibility. The threat of intervention

9. Taylor (1982) has shown that a more gradual approach to reducing inflation may be able to eliminate inflation without producing any output loss. One criticism of his conclusion, however, is that establishing credibility with such a gradual approach may be infeasible.

10. For example, see Barro (1977), Gordon (1982), and Mishkin (1983).

by foreign powers made credible the fiscal reforms that eliminated the huge budget deficits and ended rapid money growth. In a related but somewhat more controversial paper,[11] Sargent contends that the Poincare anti-inflation program in France in the 1920s was more successful than the Thatcher program because Poincare's program established credibility by pursuing budget reforms while Thatcher's program did not.

Does evidence from the recent disinflationary experience in the United States shed light on whether credibility is an important factor to the success of an anti-inflation program? If one assumes as in Perry (1983) that a shift to an anti-inflationary monetary policy regime did occur with the change in the Federal Reserve operating procedures in October 1979, then a believer in the importance of credibility might expect to see a more rapid decline in wage and price inflation since 1979 than would be predicted by traditional Phillips curves estimated from pre-1979 data. Several recent papers (Perry [1983], Eckstein [1984], and Blanchard [1984]) have found no evidence that traditional Phillips curve equations have undergone structural shifts in the 1979-83 period, while Cagan and Fellner (1983) and Fisher (1984) do find that wage inflation has declined more rapidly than would be predicted by a traditional Phillips curve. Does evidence that tends to show that large overpredictions by traditional Phillips curves do not occur in the 1979-83 period cast doubt on the importance of credibility to the behavior of aggregate supply? The answer is no.

An important point raised by Taylor (1984) is that the switch from interest rate targeting to reserve targeting by the Federal Reserve starting in October 1979 does not imply that there was a significant change to an anti-inflation policy regime. Taylor (1984) finds that there was some shift to a less accommodative policy regime, but the change was not dramatic. Blanchard (1984) looks at an equation describing the term structure of interest rates and he finds that there is no evidence that the financial markets believed that a change to an anti-inflation policy regime had occurred. The conclusion that arises from this evidence is that the recent disinflationary experience cannot provide a test of the importance of credibility to anti-inflationary policy because a credible anti-inflation policy never occurred. This should not be very surprising considering the budgetary policy pursued by the Reagan administration: The shift to large-budget deficits as a result of the Reagan tax cuts would not help promote confidence in a continuing anti-inflation monetary policy.

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11. Sargent (1981)

A prescription for monetary policy

The discussion in this paper leaves us with the following conclusion. Since sustained inflation is a monetary phenomenon and expectations about future policy appear to have an important impact on the behavior of aggregate supply, a successful anti-inflation program must involve a credible, non-accommodating, anti-inflationary monetary policy. What does this conclusion suggest about the appropriate conduct of monetary policy?

Achieving credibility for an anti-inflationary monetary policy is no easy task, especially when accommodating policies have been pursued in the past. This is an important reason why we can not expect the disinflation process to occur without costs. As my two-year-old son understands, talk is cheap—only actions can establish credibility. The same principle has been understood by successful practitioners of foreign policy such as Teddy Roosevelt, who stated that the United States should 'talk softly, but carry a big stick." Luckily, we are currently in a situation where credibility for a non-accommodating, anti-inflation monetary policy should be easier to establish because of recent actions by the Federal Reserve. The unwillingness of the Fed to raise the rate of money growth to eliminate unemployment during the most recent recession provides some indication that it is finally willing to pursue a serious anti-inflation policy. Some slight evidence that this Fed policy is starting to establish credibility is found in Cagan and Fellner (1983), Blanchard (1984), and Eckstein (1984), who document that more rapid wage disinflation than would have been predicted by traditional Phillips curve equations seems to have taken place in 1982 and 1983.

A key feature of making a non-accommodating, anti-inflationary monetary policy even more credible is that the Fed pursue a non-accommodating monetary policy rule that can easily be evaluated by the public. If the rule is sufficiently understandable that the public can verify whether the Fed is adhering to it, then the action of adhering to the rule will more rapidly establish credibility for this policy. One suggested policy rule is the constant money growth rate rule proposed by Milton Friedman. Although this rule has the advantage of being easily understandable, it has two serious problems. First, financial deregulation and the recent large swings in velocity imply that such a rule may entail more substantial shifts in the aggregate demand curve than would be optimal. Second, the money supply cannot be precisely controlled by the Fed. This lack of control makes it harder for the public to verify whether the Fed is abandoning its

prescribed rule when the money supply deviates from its target level or is rather continuing to adhere to its rule but is sufferingsome bad luck. This difficulty in verification of Federal Reserve intentions would make credibility harder to establish.

An alternative suggested rule is that the Fed target nominal **GNP** growth. **A** serious problem with targeting nominal **GNP** growth is that it may give the Fed so much leeway in its conduct of monetary policy that the public will have no way to verify whether or not the Fed is actually pursuing a **non-accommodating** policy.

An alternative policy **rule** that is very close to a suggestion of **McCallum** (1984) involves Fed targeting the monetary base in order to hit specified values of nominal **GNP Targeting** the monetary base has the advantage that the monetary base is easily controlled by Federal Reserve actions, particularly open market operations, whiie this is not true for the money supply or nominal **GNP** With a monetary base target, the Fed can no longer have the excuse of saying that it has missed its targets because of factors outside of its control, and the public will be able to verify easily whether the Fed is adhering to its rule.

The need to choose monetary base targets so that specified values of nominal **GNP** can be achieved, rather than a constant growth rate rule, has been made necessary by the recent large swings in velocity, both for money and for the monetary base. The target level of nominal **GNP** should be chosen to coincide with a rate of nominal **GNP** growth that is consistent with price stability. **If** a large decline in base velocity occurs so that nominal **GNP** has fallen well below its target level, then the target for the monetary base next period should be raised accordingly to bring nominal **GNP** back up **to** its target level. Similarly, a too rapid rise in nominal **GNP** would result in a smaller rate of growth of the base. The targeting rule wouldobviously have to be specified more precisely than in the discussion here, and this would require econometricresearch on the link between the monetary base and nominal **GNP** This econometric analysis is unlikely to yield a tight link between these two variables, but this is just a reflection of the uncertainty inherent in any macroeconomic analysis. Designing a reasonable policy rule from this research should not present any major difficulties.

One change in the Fed's operating procedure that would make the monetary base even easier to control, and would lead to enhanced credibility of a policy rule relying on base targeting, is the tying of the discount rate to some market interest rate, such as the three-month Treasury bill rate or the Federal funds rate. Most of the uncontrolled movements in the monetary

base arise from fluctuations in borrowings from the Fed occurring as a result of large swings in market interest rates relative to the discount rate. Tying the discount rate to a market rate would keep the spread between these two rates constant and would thus eliminate this source of fluctuations in the base.

The analysis in the previous sections of this paper indicates that such a policy regime might go a long way to promoting price and even output stability. However, there is still the issue of the current large budget deficits. As noted above, the role of budget deficits in the inflation process in the United States is unclear. My personal view is that a serious attempt to balance the budget needs to be made because, at a minimum, the prospects of huge budget deficits in the future may decrease the credibility of the anti-inflationary monetary policy proposed here.

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Commentary

William Nordhaus

As we gather here in these magical mountains to analyze strategies for efficiently combating inflation, something bizarre is going on. *The* New York *Times* on Tuesday editorialized about the dangers of deflation. An outside observer might think that we should be sent to a sanatorium rather than an auditorium. Perhaps, like Hans Castorp, who went to visit his lieutenant cousin, we should use our trip to this mountain paradise to pause and question whether, in a world of deflation, 'tis sane to continue our obsessional pursuit of credible anti-inflationary rules.

But conferences, like inflation, have their inertia. So I will turn to my assigned task of discussing the paper of Rick Mishkin. His argument takes three steps:

- Inflation is a monetary phenomenon.
- Credible policies will make inflation even more of a monetary phenomenon.
- A programmable rule—such as nominal GNP targeting—is an effective credible policy.

To dispel any suspense, let me say that while each of these is plausible, they are incomplete. To rest policy on these three doctrines is to commit an unproven and perhaps a dangerous oversimplification.

Inflation as a monetary phenomenon

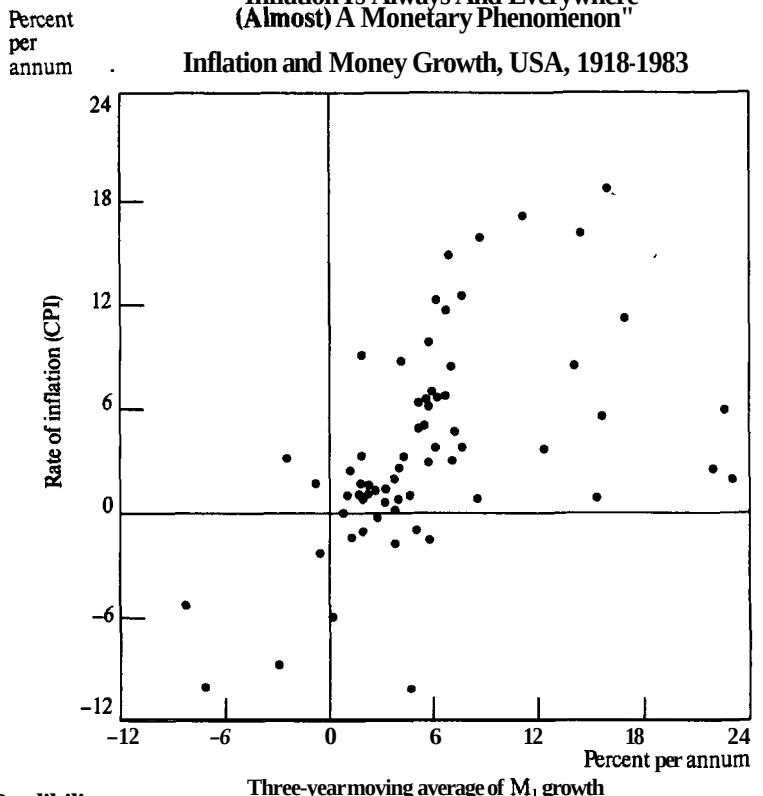
The proposition that inflation is a monetary phenomenon is, of course, an old saw. I thought that by this point its half truth was well established. In today's canonical model of inflation, it is a correct long-run proposition: That is, a step-up of money growth from x to x + 1 percent per annum will, in the long run, lead to close to a 1 percent per annum increase in inflation.

The only problem with this proposition is that—because the long-run may be long and because other things will not remain equal—it 'is a poor

approximation to reality over periods of one, two, or five years. It is akin to the saying, 'Death is an octogenarian phenomenon." Surely few people survive 80 years, and few die before 40. But to base the practice of medicine on the proposition that death results only from reaching four-score years would be a tragic error.

Figure 1 will give you an idea of how tight the monetarist suit fits. It is the regression of CPI inflation on money in the current and two previous years over the period since 1918. If it gives you the impression of a pretty weak relationship, I would like to agree with you.

FIGURE 1
"Inflation Is Always And Everywhere (Almost) A Monetary Phenomenon"

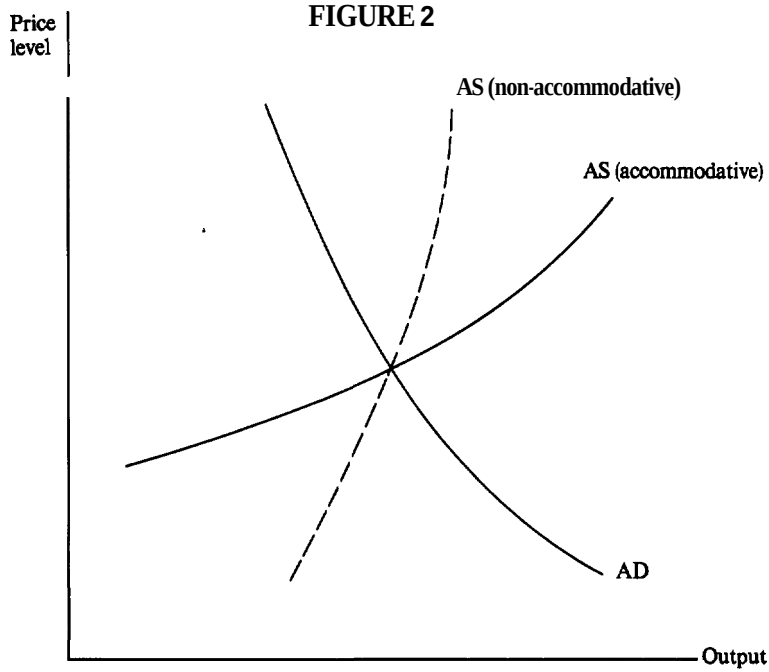


Credibility

The major thrust of Mishkin's paper is to endorse the proposition that a credible anti-inflation policy will achieve disinflation at lower output cost than will a non-credible anti-inflation policy. Putting this somewhat more technically, a non-accommodative policy is defined as one that does not

shift AD to offset a shift in AS. The proposition is then that, when workers and firms know that policymakers will not accommodate supply shocks, the AS curve will become steeper (as in Figure 2). This steepness means that AD shocks will have less impact on Q and that 'cold-turkey" disinflation policies will be more efficient (in Okun's sense of lowering the output loss per point of disinflation) than gradual policies.'

FIGURE 2



This analysis raises two issues: First, Mishkin and others claim that a discretionary policy will be more accommodative than a policy based on rules. And second, some claim that a non-accommodative policy will have a significant effect on wage and price behavior, rotating the AS curve in Figure 2 by many degrees. I will argue that the first of these points is misleading, while the second is not supported by empirical evidence.

Starting with the first contention, would the nation and world be well served by a shift to a programmable economic policy?

I am skeptical. The theories are weak, and the lessons of history argue strongly against discarding in favor of a simplistic rule the brains that it took us one billion years to evolve.

To begin with, remember that the case for rules is partly political—an aversion by conservatives to government taking any actions, a plea for

neutrality. How government can be neutral today is beyond me—claiming to be neutral is like claiming to be dead.

The more interesting and novel argument is that fixed rules induce better behavior on the part of workers and firms. Knowing that the Fed will bomb the real economy whenever inflation rises, the theory goes, workers and firms will restrain their wage and price increases. This strategy is similar to the "doomsday device" of early strategic theory.

You may recall that the doomsday device was a deterrent strategy described by the late Herman Kahn. The idea was that, should the Soviets drop a bomb on us, the doomsday device would automatically explode and wipe out the globe. When faced with such a device, all rational agents would clearly be deterred from nuclear attack. The anti-inflationary fixed rules have a similar theme—you have to be credibly willing to destroy the economy in order to save it.

Why, you might ask, was a doomsday defense policy not pursued? Simply because of its lack of robustness to unforeseen events—like accidents. And this is indeed the main problem with fixed economic rules. We simply don't understand the world well enough to program our response. Think of every time a rule ran contrary to what discretion would dictate. For example, 1977, 1978, 1979, 1980, 1981, 1982, 1983, and 1984. It is just those periods when Paul Volcker and his colleagues earn their salary. Every time there is a price, output, unemployment, or velocity surprise, we need a human brain to figure out why the surprise occurred and what to do about it.

Recent history should also convince the openminded about the perils of fixed rules. The Federal Reserve turned to a close approximation of precommitted monetary rules in 1979. Who foresaw the 60 percent real appreciation of the dollar, the $100-billion current-account deficit, the enormous rise in real interest rates, the deep recession, the flight from fixed-interest rate securities, and the problem of Latin debt? We can only be grateful that a fixed-M rule had not been imposed by a constitutional amendment and that the Fed had the wit and wisdom to break with rigid monetarism before construction workers stormed the Fed.

Fixed-rules advocates, in short, suffer from the Maginot fallacy. They think that we know who the enemy is and where he will strike. In fact, we often don't; and on just those occasions we need some common sense.

There are other problems with the doomsday theory. One is that it misconstrues the protagonists. The uncertainties facing firms and workers are predominantly microeconomic, not monetary. Allied Van Lines and the Teamsters don't much care about whether policy is accommodative, because their livelihoods depend much more on trucking regulation and the

NLRB. Given the bounded rationality of most firms, workers, and unions, I would guess that a change of policy regime would be below the threshold of perception and of reaction. It is hard to believe that there would be any direct effect on Ford Motor Company's pricing policy or the UAW's wage negotiations, or on most wage-price behavior outside of auction markets, of a change in the monetary operating rule.

Put differently, in an economy where the policymakers face a rational agent who controls a substantial proportion of an economy's wage or price decisions, a doomsday threat might indeed work. But in the U.S. today, there are too many firms and workers, who are more concerned about Japanese engineers than about Fed economists, for any credible or incredible policy to have a substantial independent effect on aggregate wage-price dynamics.

If we turn from military to economic history, the evidence is not supportive of the power of credibility. I am sure this conference will debate the effect of the Volcker-Carter-Reagan disinflation. The numerous studies on this period for the United States indicate that the contribution of credibility was somewhere between nil and small. Buiter and Miller find that the much more credible disinflationary policies in the U.K. had extremely high output and unemployment costs.

I would like to present a small piece of independent evidence on this issue. The credibility view implies that inflation should fall faster during a credible disinflation regime than outside it. We might write such a system as follows:

$$(1) \quad p_t = ap_t^* + (1-a)p_{t-1} - bu_{t-1} + e_{1,t}$$

$$(2) \quad p_t^* = \Sigma\lambda_i p_{t-i} - d\,\mathrm{Cred}_t + e_{2,t}$$

where

p_t = rate of price inflation in period t
p_t^* = expected rate of price inflation in period t
u_t = unemployment rate in period t
Cred = credibility of policy in period t
λ, a, b, d = parameters
e_1, e_2 = random errors

The usual fashion of testing for credibility (see particularly the work of R. J. Gordon) is to substitute (2) into (1). Assuming $e_2 = 0$,

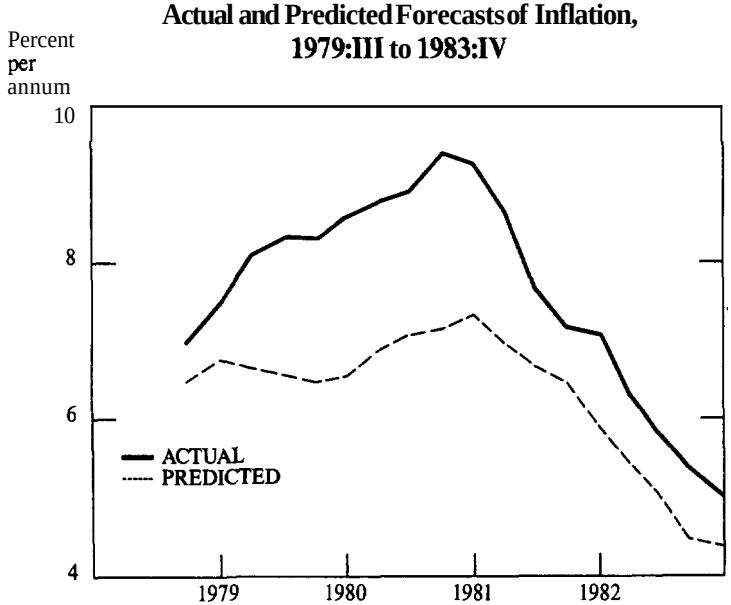
$$(3)\ p_t = a\left[\Sigma\lambda_i p_{t-i}\right] + (1\text{-}a)p_{t-1} - bu_{t-1} - a\,d\,Cred_t + e_{1,t}$$

By examining forecast errors in the inflation equation (say during 1979-83), we can test whether the term $a\,d\,Cred_t$ was significant.

A different and simpler route is to test (2) directly. I have constructed, therefore, an expected rate of inflation, using the ASA-NBER survey of 50 forecasters. This was estimated during the 1970s and then forecast out-of-sample during 1979:III-1983:IV. Such a forecast may have included both lagged inflation and policy variables, so I performed the test with and without money growth as right-hand side variables.

The results, shown in Figures 3 and 4, give no comfort to the credibility hypothesis. If a credible policy had been installed, actual inflation forecasts should have been ***below*** those predicted by the structure of earlier years. Instead, both with and without money growth in the equation, the actual forecasts were above the predicted forecasts.

FIGURE 3
Actual and Predicted Forecasts of Inflation, 1979:III to 1983:IV



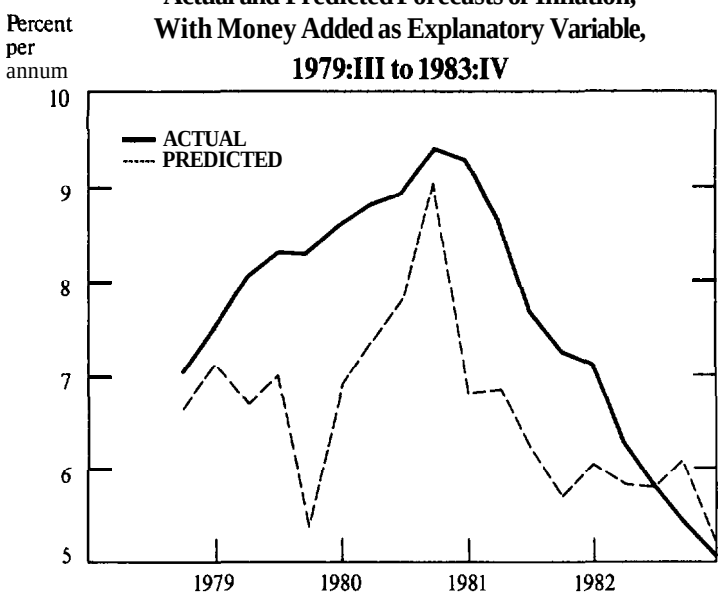
Notes: The dependent variable is the ASA-NBER median forecast of inflation for the **GNP** deflator over the four quarters ahead of the survey month. In this figure the explanatory variables are lagged inflation for the last and three earlier quarters. The forecasts are made on the basis of an equation fitted over the 1972-1979:II period and forecast with the actual values of the right-hand side variables in the post-sample period.

This result suggests that there was no identifiable effect of the credibility through expectations and onto inflation. Rather, it was events in the real (as opposed to the perceived) economy that disinflated the economy. This, of course, is just what studies of Gordon, Blanchard, Eckstein, Perry, and others have shown.

Fixed rules

What can we then conclude about fixed rules, such as targeting nominal GNP? Surely there is something to be said for a nominal GNP rule (or a Hall rule). It is better than an M_1-growth rule, an M_2-growth rule, a monetary-base rule, or a credit rule. It is better than chaos or a random number rule. It is better than a gold standard or a plywood standard.

FIGURE 4
Actual and Predicted Forecasts of Inflation, With Money Added as Explanatory Variable, 1979:III to 1983:IV



Notes: The procedure is exactly the same as in Figure 3, except that four lagged money terms are added to the right-hand side of the regression equation.

But is it better than the flexible discretionary guidance of W. M. Martin, Arthur Burns, or Paul Volcker? I think not. The scientific argument for a rule rests entirely on the view that by changing regimes we can improve the nation's macroeconomic performance. If the best evidence suggests that our macroeconomic performance has deteriorated, as I think it does,

then the intellectual foundation for the fixed rule crumbles.

In the movie War ***Games,*** the fixed-rule crowd has captured the Pentagon. An enormous computer known as the Whopper has taken over all strategic decisions. Of course an enormous Blooper sets the Whopper off onto the game called Global Thermonuclear War. Only the daring of a teenage hero and his friend can save the world by heading off the Whopper. ***All*** I can hope is that when we program the Fed's Whopper to run the global economy, some sensible teenager—not mesmerized by elegant but misleading theories—will figure out how to save us from global macroeconomic disaster.

2
The Benefits of Price Stability

Stanley Fischer

> "... *Such a spirit* [zeitgeist] *seems at work in the 1960s and 1970s, and is evidenced by what appears as a generalized erosion in public and private manners, increasingly liberalized attitudes toward sexual activities, a declining vitality of the Puritan work ethic, deterioration in product quality, explosion of the welfare rolls, widespread corruption in both the private and governmental sector, and, finally, observed increases in the alienation of the voters from the political process.* ... [*W*]*ho can deny that inflation, itself one consequence of that conversion, plays some role in reinforcing several of the observed behavior patterns. Inflation destroys expectations and creates uncertainty; it increases the sense of felt injustice and causes alienation. It prompts behavioral responses that reflect a generalized shortening of time horizons. Enjoy, enjoy'—the imperative of our time—becomes a rational response in a setting where tomorrow remains insecure and where the plans made yesterday seem to have been made in folly.*"
>
> Buchanan and Wagner (1977), pp. 64-65.

Economic analysis of the costs of inflation—the mirror image of the benefits of price stability—is inevitably disappointing to the many, such as Buchanan and Wagner, who know that inflation is a deep societal problem.[1] The question is whether what the many know is merely difficult to prove, or rather is substantially exaggerated.

Some of the views expressed in this paper are the result of seeing triple-digit inflation in close up while I was Max Bogen Visiting Professor of Economics at the Hebrew University, Jerusalem, in the spring of 1984. I am indebted to Robert Shiller and other conference participants for useful comments, to Patricia Mosser for research assistance, and to the National Science Foundation for financial support.

1. A footnote in the original, quoting Ropke to the effect that inflation undermines the foundations of a free society, has been omitted.

In this paper I start by reviewing the standard analysis of the costs of inflation, which depend on the institutional structure of the economy. Despite two decades of inflation, most of the developed economies have chosen not to encourage indexation or other institutional changes that would reduce the costs of a given rate of inflation. In the next section I examine the reasons for and the desirability of the decision to keep inflation painful. Concluding comments on the passion gap between the economic analysis of the desirability of price stability and rhetoric about inflation are then presented.

The costs of inflation

The costs of any given rate of inflation differ depending on whether the inflation was anticipated or not, and on whether the economy's institutions have adapted to the presence of inflation.[2] The greater the extent of institutional adaptation, and the longer any given inflation has been anticipated, the lower its costs.[3] We now examine the major economic costs of inflation, starting with costs that occur even when inflation is anticipated, and then moving on to consider costs associated with uncertainty about inflation and the variability of relative prices.

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2. This paper should be interpreted as an attempt to assess the costs society should assign to inflation as part of an analysis of optimal policy: The other components are the costs or benefits of alternative results of policy, and the model of the economy that describes the feasible economic tradeoffs among various economic goods and bads—like inflation and unemployment. More technically, this paper concentrates on exploring one argument in the social welfare function; it examines neither the other arguments in the social welfare function nor in any detail the Phillips curve-type tradeoffs among inflation, unemployment, and growth that are needed for a full analysis of optimal inflation policy. There is no difference in this regard between estimates of the costs of unemployment based on Okun's Law, and estimates of the costs of a given rate of inflation given in this paper. The attempts made in this and earlier papers to measure the costs of inflation are sometimes criticized for their failure to describe the policies that would reduce inflation, but I do not see those who criticize this type of paper applying the same criticisms to Okun's Law-based estimates of the costs of cyclical unemployment.

James Tobin in his concluding comments at the conference stated that he wished I had given estimates of the costs of alternatives to inflation—for instance, the German hyperinflation was one way of trying to raise revenues to pay reparations after World War I. Perhaps it was the best way. But it is nonetheless an interesting question as to what costs the inflation imposed on the German economy.

3. On the costs of inflation, see Fischer and Modigliani (1978) and Fischer (1981); for other accounts, see Jaffee and Kleiman (1977), Klein (1976), Leijonhufvud (1981), Chapters 9 and 10 (originally published in 1977), Nordhaus (1973), and Okun (1975).

The money triangle and menu costs

The best analyzed cost of inflation is the money triangle, representing the increased transaction costs resulting from economizing on holdings of currency and bank reserves as the inflation rate increases. The money triangle is a cost of anticipated inflation. As long as currency continues to be an efficient medium for making small transactions, the triangle welfare cost cannot be removed by institutional adaptation.[4] At a 10 percent inflation rate, the welfare cost corresponding to the money triangle—the area under the demand curve for currency—is about 0.25 percent of GNP in the United States. However, since currency is used more extensively in illegal than in legal transactions, a tax on currency has desirable allocative and distributional implications that offset this particular welfare cost of inflation.

Because the government has to use distortionary taxation to raise revenue, there is some welfare loss associated with all types of taxation. There is accordingly an argument, made originally by Phelps (1973), that some inflation is desirable on public finance grounds. However no one has established a case for high rates of inflation on this basis.

As transaction technology changes, for instance as the use of credit cards and futuristic means of making payments spreads, the size of the money triangle can be expected to fall. Improvements in the transactions technology are themselves in part induced through inflation, but are not reversible. The experience of inflation accordingly tends over time to reduce the welfare costs that result from economizing on the use of currency.

The money triangle becomes large at high rates of inflation. For instance, under reasonable assumptions about the form of the currency demand function, the money triangle welfare loss of a' 400 percent per annum inflation rate (corresponding to 160 percent with continuous compounding) is 3.3 percent of GNP.[5] These losses correspond to the famous descriptions of increased transactions costs in hyperinflations as individuals are paid more frequently and scurry to spend their incomes before the money loses its value. They alone provide good reasons to avoid

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4. The payment of interest on bank reserves would reduce the welfare loss triangle. I assume that money-stamping would not be an efficient means of paying interest on currency.

5. The assumptions are that the currency/GNP ratio at a zero inflation rate would be 0.075, at 160 percent inflation 0.025, and that the demand for currency equation is of the Cagan form, with unitary income elasticity: CIPY = A · exp(-b · gpe), where gpe is the expected inflation rate. Under these same assumptions, the cost of an 800 percent inflation rate (corresponding to 220 percent compounded continuously) is 4.9 percent of GNP.

hyperinflation, even if the hyperinflation was perfectly anticipated and if there were no other welfare costs of inflation. But they do not by themselves account for popular reactions to rates of inflation in the low double-digit range, of the type experienced in many of the OECD economies in the '70s. Menu costs of inflation arise from the need to change prices more frequently with a higher inflation rate. These are the physical costs of changing prices, the costs of reprinting menus, changing telephone coin boxes, and the like. When the inflation rate becomes high, one-time changes—such as moving to the use of tokens in pay phones—are introduced that make the marginal costs of further inflation low. There are no well-established estimates of the menu costs of inflation.

Institutional non-adaptations

Many of the costs of the recent United States inflation were a result of the failure to adjust regulations and laws that were based on the presumption of stable prices. Interest rate controls in the banking system and non-indexation of taxes are the most important examples.

Controls on nominal interest rates payable by financial institutions ensure that the welfare loss associated with the currency triangle extends to other financial assets, to an extent that depends on the availability of substitute assets (equivalently on the interest elasticity of demand for the controlled asset).[6] Such controls bear particularly heavily on less sophisticated investors who keep their wealth in deposits. Interest ceilings on loans create an additional welfare loss from the misallocation of credit. The invention of money market funds and other financial innovations of the '70s, together with deregulation of the banking system in the '80s, substantially reduced the welfare costs of inflation arising from these controls.

Adjusting the tax system for inflation requires not only bracket indexation, but also appropriate inflation adjustments in the taxation of capital. Such adjustments would be administratively complicated and, if implemented, would imply major shifts in the tax burden.[7]

Accordingly, capital taxation has made few explicit adjustments for inflation except in countries with high rates of inflation—and even in these countries, major inflation-related distortions remain.[8]

The welfare losses associated with inflation-induced capital tax distortions occur because both savings behavior and the allocation of

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6. The existence of interest rate controls modifies the analysis of the welfare costs of the currency triangle.

7. The issues are discussed in Aaron (1976).

8. Use of a consumption tax would avoid these difficulties.

investment are affected.[9] The size of the distortions is difficult to pin down: Under fairly modest assumptions about saving elasticities and with the tax code of the early '80s, it is possible to associate a welfare loss of close to 3 percent of GNP with a 10 percent inflation.[10]

The losses discussed in this section are avoidable consequences of inflation. Financial deregulation will reduce the costs of any future inflation. The losses resulting from inappropriate treatment of capital income could be avoided either by adjusting taxes or by moving to a consumption tax. But the fact is that such distortions remain in many countries: It is evidently no simple matter, administratively and politically, to make the adjustments.

Despite a variety of initiatives by the accounting profession and economists, inflation-adjusted corporation accounts have not gained wide acceptance. Similarly, inflation adjustments to significant macroeconomic variables, such as the government budget deficit or savings rates, are far from routine, even though the principles of inflation-adjusted accounting are well understood. With existing systems of accounting, budget deficits are exaggerated under inflationary conditions because nominal rather than real interest payments are treated as a current expense. The nominal component of interest should be deducted as a repayment of principal."[11] The adjustments may be substantial: For instance, Italy has a debt equal to 80 percent of GNP and an inflation rate of about 12 percent. The inflation adjustment is then nearly 10 percent of GNP, transforming Italy's budget deficits from 15 percent of GNP to 5 percent.[12]

There are no estimates of the welfare costs of fiscal policy mistakes, if any, resulting from mismeasurements of deficits. Nonetheless, systematically poor information is an unlikely aid to intelligent policymaking.[13]

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9. See Feldstein (1982) for a review of some of his work in this area.

10. See Fischer (1981) for estimates based on earlier work by Feldstein and Summers (1979). King and Fullerton (1984), pp. 244-45 criticize the Feldstein-Summers results, showing that most of the effect is a result of the continued use by firms of FIFO accounting in inflationary conditions. However, given that firms do use FIFO accounting, King and Fullerton show sizable increases in marginal corporate tax rates as the inflation rate rises.

11. Equivalently, the government should count as part of its income its capital gain on the real value of outstanding liabilities.

12. A complete set of adjustments for the EC countries is presented in Cukierman and Mortensen (1983). The magnitude of the adjustment for the U.S. can be calculated based on a privately held public debt equal to 30 percent of GNP and an inflation rate of, say, 4 percent, implying an adjustment to the deficit equal to 1.2 percent of GNP!

13. It may be argued that budget deficits should always be exaggerated since governments always overspend. But in high inflation countries, for example Brazil, the exaggeration can be so large as to lead to excessively contractionary fiscal policy when stabilization is attempted.

The examples of this section show that nominal thinking and nominal institutions are deeply embedded in the structure of the economy. That is one of the main reasons price stability should be a goal of policy.

Inflation and price level uncertainty

The empirical evidence is that there is more uncertainty about future price levels at high than at low average rates of inflation.[14] There is no logical reason that this has to be so: In principle, it should be possible to produce the same stability of the price level around a trend rising at 10 percent per year as around a stable trend. And indeed, as the cross-sectional Figure 1 shows, there are countries, such as Australia and Italy, that have reasonably stable inflation rates at high levels.[15]

A highly variable inflation rate is not necessarily an unpredictable one, since the fluctuations might be foreseen—just as a retail business can predict the highly seasonal pattern of its annual sales. Here too the empirical evidence is that in the United States (and Australia) uncertainty about inflation is positively associated with the rate of inflation.[16] The most persuasive explanation of this relationship, due to Okun (1971) and Flemming (1976), is that because economies cannot adjust fully to inflation, monetary policy is more likely to be reversed at high than at low inflation rates.

Uncertainty about future 'price levels and unanticipated changes in prices both have welfare costs. Observers of inflationary economies often point to the diversion of managerial resources to financing rather than production activities, though there is as yet little evidence on the extent of this loss in the developed economies.

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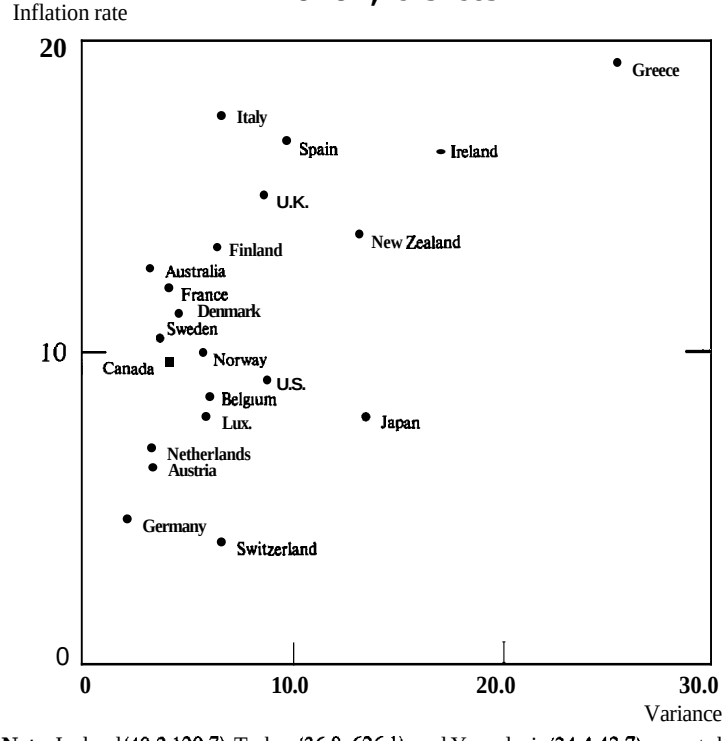
14. See Pagan, Hall, and Trivedi (1983) for a critical review of the literature.

15. Within the OECD, and across a sample of 53 countries for which data were available on the IFS tapes, there is a strongly significant positive correlation between the variance of the inflation rate and its level, for both the 1960-73 and 1973-83 periods. For earlier examination of this relationship, see Okun (1971), Gordon (1971), and other studies reviewed in Fischer (1981).

16. There are two types of evidence, presented in Fischer (1981). First, the variance of the error term in a forecasting equation for the inflation rate is heteroscedastic, increasing with the inflation rate. Second, as Cukierman and Wachtel (1979) and others have shown, the cross-sectional variance across forecasters is an increasing function of the inflation rate. Pagan, Hall, and Trivedi (1983) critically examine much of the earlier literature before establishing a positive relationship between price level uncertainty and the inflation rate for Australia.

FIGURE 1

CPI Inflation and the Variability of the Inflation Rate, OECD, 1973-1983



Note: Iceland (48.2,120.7), Turkey (36.8, 626.1), and Yugoslavia (24.4,42.7) are not shown on the graph. Entries in parentheses are inflation rate and its variance respectively.

In an economy without indexed assets or other safe means of hedging against inflation, there is an ex ante loss from greater uncertainty about future price levels. The size of the loss can be approximated starting from an estimate of the premium that indexed bonds would command over nominal bonds, and it turns out to be of the same order of magnitude as the money triangle at a 10 percent inflation rate.[17] The difficulty in this procedure is, though, that the larger the welfare loss associated with the absence of indexed bonds, the harder it is to explain their non-existence

17. The **premium** for indexed bonds is the excess of the expected real rate of return on nominal bonds over the real return on indexed bonds.

except where introduced by governments.[18] Marketable indexed bonds have been issued by governments in high-inflation countries, and in the United Kingdom. Indexed Social Security provides a similar inflation hedge, albeit in restricted amounts.

In the absence of indexed bonds, increased uncertainty about future price levels imposes welfare costs. The difficulties apply particularly to long-term, for example retirement, savings. With equity not having sparkled as an inflation hedge, the long-term saver is substantially exposed to inflation risk. The two best inflation hedges are housing and the rolling over of short-term nominal assets, but in neither case is the real value assured as it would be with indexed bonds. The inability to protect the value of savings against inflation is almost certainly a—if not the—major reason that the public reacts so viscerally to the threat of inflation.

Ex post redistributions of income and wealth caused by unanticipated inflation create both gainers and losers. Empirical research has not uncovered any consistent effects of unanticipated or anticipated inflation on the distribution of income, despite the popularity in the '50s and earlier of the view that inflation redistributes income from labor to capital. On wealth account, within the private sector, unanticipated inflation redistributes wealth from the wealthy, who own nominal assets, to the middle income groups who are largely nominal borrowers. As Hurd and Shoven (1983) show, the elderly wealthy are extremely vulnerable to unanticipated inflation, while the elderly poor, who have no assets beyond Social Security wealth, are impervious to the effects of unanticipated inflation. As between the private and public sectors, unanticipated inflation benefits the public sector. This redistribution is in part intergenerational, since the reduction in the real value of the national debt implies that future generations will have to pay lower real taxes.

What are the welfare costs of such redistribution? The simple answer is that the costs depend on how society weights the marginal utilities of the gainers and losers—that is to say, we do not know. But such redistributions are costly to society because they create and destroy wealth for individuals on an apparently random basis, and not on a basis that rewards the Protestant virtues. Certainly, the well-known inspired polemics of Keynes (1919, 1923) on the dangers of inflation emphasize the role of wealth redistributions and the loss of legitimacy such redistributions imply for capitalist institutions.

18. The welfare economics of government issue of indexed bonds is discussed in Fischer (1983).

Inflation and relative price variability

A persistent theme in the inflation literature is that inflation interferes with the efficient operation of the price system. Greater uncertainty about the aggregate price level implies greater uncertainty about relative prices, and accordingly less response to changes in relative prices. Further, if it is costly to change prices, the variability of relative prices will increase as the overall inflation rate rises.[19]

There is a strong positive relationship between the inflation rate and relative price variability, in the United States and in other countries. The most convincing explanation of the relationship is that it reflects causation in both directions: Exogenous shocks to relative prices are associated with increases in the inflation rate, and exogenous increases in the inflation rate cause increases in the variability of relative prices. Increases in relative price variability in high-inflation countries are in part attributable to lags in the adjustment of prices administered by the government.

There are, so far as I know, no estimates of the welfare costs of the reduced efficiency of the price system caused by inflation.[20] The size of the estimates would depend on the underlying theory: If the theory builds on informational inefficiencies, then the welfare costs are related to unanticipated inflation; if the theory builds on costs of changing prices, then inflation per se is to blame. Okun's theory of customer markets (1975) would also assign costs to inflation-induced price changes. However, the Okun theory could also be recast to say that customer relationships would be preserved by constancy of real (indexed) prices in an inflationary economy.

Adapting to inflation

Only two of the many costs of inflation discussed above could not be removed by institutional innovation: the money triangle and the reduced efficiency of the price systems associated with higher inflation and/or

19. The extensive literature on inflation and relative price variability is reviewed in Marquez and Vining (1984).

20. Fischer (1981a, pp. 419-22) argues that quantity rather than price variability should be the basis of welfare calculations, and that rough calculations suggest that the costs arising from quantity variability are small. Simple regressions, presented in Fischer (1983a), show a negative relationship between the growth rate of real output and the inflation rate in a cross-section time series analysis of 53 countries, over the periods 1960-73 and 1973-81. But these results certainly cannot be attributed solely or even mainly to the effects of inflation on the efficiency of the price system, since supply shocks and business cycle timing relationships play major roles. Another strand of the literature, for instance Friedman (1977) and Mullineaux (1980), argued that inflation uncertainty increases the unemployment rate. The comments about supply shocks apply in this case too.

greater inflation uncertainty. In the latter case, I am assuming that in practice higher rates of inflation are also more uncertain rates of inflation, though I believe that if an ironclad fixed growth rate monetary rule were introduced, uncertainty about inflation would be much the same whether the growth rate were zero or 10 percent.

Most of the remaining costs could be avoided by completing financial deregulation, by the government's issuing indexed bonds, thoroughly indexing the tax system, and removing legal impediments to the use of indexed contracts.[21] Government indexation would likely be followed by increased private sector indexation. For instance, the absence of privately issued indexed annuities is doubtless one of the major sources of private sector concern about inflation; private insurance companies would probably start selling such annuities as soon as government indexed bonds were available. Legal restrictions also play a role in slowing indexing innovation. For instance, despite the proliferation of new forms of mortgage in the last decade, there has been only one issue of price level adjusted mortgages (PLAM's). It turns out that there are still legal impediments to their issue. HUD is currently considering proposals that would facilitate the issue of PLAM's.[22]

Why should the government not index the economy as completely as possible to reduce the costs of inflation? Most governments have resisted indexation, typically arguing that it would be a 'confession of failure" in the fight against inflation 'and might easily have disruptive consequences" for the economy.[23] The arguments fall into three categories: First, indexation may affect expectations; second, it may make the government more willing to tolerate inflation; and third, indexation may reduce the stability of the economy.[24]

The first and second arguments are essentially the same. If indexation reduces the costs of inflation, then the government is likely, when faced with any disturbance that requires it to contemplate an increase in the

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21. Since I am examining the benefits of price stability, I do not discuss innovations such as those of Irving Fisher (1920) and Robert Hall (1982) that would reduce the costs of inflation by removing inflation--either by redefining the monetary unit or by operating a commodity currency scheme.

22. The proposal is described in 'Insurance of Indexed Mortgages: Docket No. R-84-1153, FR-1915, in the Federal Register, Vol. 49, No. 108, June 4, 1984. I am indebted to Huston McCulloch for this information.

23. Report of the [Radcliffe] Committee on the Working of the Monetary System, 1959, para 573.

24. See also Okun (1971).

price level, to permit more inflation. This suggests at the least that the government would be willing to permit greater instability of the inflation rate if the system were fully indexed.

Indexation also reduces the stability of the price level by affecting the slope of the Phillips curve. The Phillips curve becomes steeper, so that a given increase in the money stock translates in the short run into more inflation and less reduction in unemployment in an indexed than in a non-indexed economy. Similarly, it is well known that by making the real wage less flexible, indexation worsens the response of the economy to supply shocks: An adverse supply shock raises prices and reduces output more with indexed than with non-indexed wages. It is analytically possible to avoid this difficulty by tying wages to an index that excludes the effects of supply shocks, but such complicated indexation schemes have not yet been introduced.

Widespread indexation of the returns on financial assets creates another potential source of instability of the price level. The larger the indexed component of the stock of financial assets, the smaller the nominal base of the system that serves to determine and, through the real balance effect maintain the stability of, the price level. For instance, if the returns on deposits are indexed, then most of the money stock automatically accommodates itself to inflationary shocks.[25] Similarly, because the national debt is indexed, inflationary shocks exert no stabilizing effect on the private sector by reducing the real value of their assets. In the extreme, the only nominal friction restraining inflation is the stock of currency, which in an inflationary economy will be small.[26]

These valid arguments all suggest that indexation would reduce the stability of the price level.[27] It is a different matter to argue that indexation would also raise the average inflation rate. Indexation reduces the cost of inflation to the private sector by removing inflationary distortions. It also reduces the marginal benefit of inflation to the public sector, by removing

25. This is the current situation in the Israeli economy.

26. In the Israeli economy, with its current 300-400 percent per annum inflation, the stock of currency is less than 2.5 percent of GNP.

27. It is possible though that resolute monetary and fiscal policy could nonetheless maintain the stability of the price level in an indexed economy. In Fischer (1983b) I found no significant difference between the inflationary responses of economies with and without indexation to the first oil shock. There was a statistically insignificant tendency for the existence of bond indexation (present in Argentina, Brazil, France, and Israel in 1972) to worsen the inflationary response.

the possibility of inflating away the public debt. The combined effect of these changes on the average inflation rate is uncertain.[28]

The question of whether indexation causes a higher rate of inflation cannot be settled by pointing to the empirical association between indexation and high inflation, because the causation is mutual. Nonetheless, while there are enough examples to show that the introduction of indexation need not cause the rate of inflation to increase, inflation rates above the low double digits cannot be sustained without substantial indexation because the economic disruptions become too large. In this sense, indexation is potentially inflationary. Even so, we do not know whether indexation reduces economic well being. Are people better or worse off when there is more, but per unit less costly, inflation?

Where does this leave the discussion of the benefits of price stability? Are higher rates of inflation with indexation an adequate substitute for price stability? The answer is no. Even with extensive indexation, the money triangle and the increased uncertainty associated with higher inflation rates (and increased aggregate price level uncertainty with indexation) remain as costs of inflation. Further, nominal institutions and methods of thinking and calculating are so deeply entrenched in all economies—including the high-inflation economies—that the task of completely indexing the economy would take many years to implement.

At the end of such a process, inflation would still be costly because it affects the payments mechanism and is associated with increased uncertainty and relative price variability. And the costs of inflation resulting from other distortions would still increase with the inflation rate, for indexation does not work well at high rates of inflation. Indexation lags have substantial distortionary effects at high rates of inflation. For instance, the price level is typically available with a one-month lag. Today's payments have to be made in today's dollars, and therefore cannot be tied to today's price level.[29] If monthly inflation rates fluctuate between, say 5 percent and 15 percent, then there remains substantial uncertainty about the real

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28. Suppose that the average inflation rate is influenced by the costs and benefits of the always-exploitable short-run tradeoff between inflation and unemployment, as in Barro and Gordon (1983), or in a less extreme form of the analysis in which governments only sometimes have short horizons. The problem is that the reduced cost of inflation to the private sector and reduced benefit to the public sector leave the effects of indexation on the government's utility function uncertain.

29. They can, however, be tied to today's exchange rate, which is one reason indexation in high-inflation countries is frequently to the exchange rate.

value of even a price-indexed debt. Similarly, because of the lag in announcing the price level, and then in adjusting the wage payment to the price level, wage indexation agreements leave considerable uncertainty about the real value of wages; the result is negotiation for retroactive wage adjustments. The potential solution to this difficulty of collecting prices more frequently may merely worsen the inflation problem.[30] Tax indexation, in particular, works badly in highly inflationary economies.

I conclude that extensive indexation should be avoided, but not that inflation should be made as painful as possible by removing all indexation—because no society can ensure stability of the price level, however devoted it is to that goal. It is important not to introduce indexing mechanisms that substantially increase the short run instability of the inflation process. It is probably most important not to index the returns on short-term deposits. Further, so long as inflation remains at reasonable levels, there is little reason for indexation of short-term nominal government debt. But some indexation beyond that already in place in the United States would be desirable. In particular, the government should issue indexed long-term government debt—as in Britain—to reduce the costs to the public from long-run uncertainty about the price level. Because tax regulations are changed infrequently, it would also be desirable to make the tax system inflation-neutral.

These changes would remove the major long-run costs of inflation without substantially affecting the short-run dynamics of the economy. Price stability would remain a goal of policy, to be traded off in the short run against unemployment, with due awareness on the part of the policymakers of the problem of dynamic inconsistency that can transform a sequence of desirable short-run policy decisions into undesirable long-run outcomes.[31]

Is that all?

Surely inflation is associated with the decline of public morality, the rise and fall of nations, and more weighty matters than money triangles and

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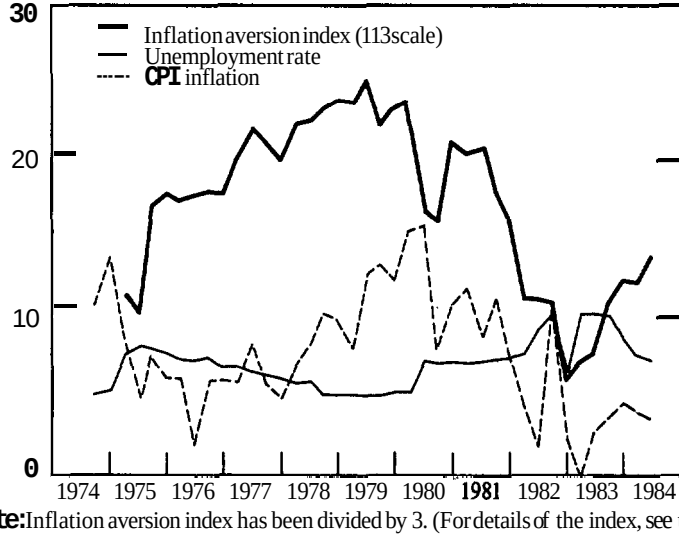
30. In a heavily indexed system, the lags in wage and other adjustments are important elements in the dynamics of the inflationary process.

31. The notion of dynamic inconsistency was introduced to macroeconomics by Kydland and Prescott (1977), and is seen by them and others as the main argument in favor of monetary rules. An alternative interpretation is that it can become a self-denying prophecy, by emphasizing to policymakers the difference between policy choices that are desirable in the short and the long run.

the efficiency of the price system. Buchanan and Wagner are merely more explicit than Keynes (1919), who claimed that Lenin declared "that the best way to destroy the Capitalist System was to debauch the currency" (p. 77).[32]

The view that comparatively low rates of inflation are a serious problem is reflected also in the results of public opinion polls: Figure 2 shows the results of a University of Michigan Institute for Social Research poll asking, "Which of the two problems—inflation or unemployment—do you think will cause the more serious economic hardship for people during the next year or so?" The inflation aversion index is defined to be the share of those answering "inflation" plus half the share of those answering "both."[33] Note that the inflation aversion index was at its lowest level at the end of 1982 as the recession reached its trough, and that concern over inflation began to increase as soon as unemployment stopped rising. Early in 1984, nearly as many people thought inflation would cause more hardship over

FIGURE 2
Inflation Aversion Index, Inflation, and Unemployment 1974-1984



Note: Inflation aversion index has been divided by 3. (For details of the index, see text.)

32. Leijonhufvud (1981), Chapter 10, struggles with the view that economists' analyses of the costs of inflation miss the seriousness of the issue.

33. Fischer and Huizinga (1982) present an analysis of opinion poll results about inflation, including a regression that explains the behavior of the inflation aversion index, with changes in the unemployment rate and the expected rate of inflation as prime determinants of the index. In this article we also attempted to track down the common view that polls have shown people attribute inflation—caused increases in their incomes to their own merit rather than inflation. We found the evidence for this view weak—see the discussion surrounding Table 4.

the next year as would unemployment, even though low rates of inflation were expected.[34]

The views expressed in the opinion polls are probably a result of the mixture of genuine vulnerability of many people—holders of nominal assets and those whose nominal wages are fixed for the next year—to price level changes, and their failure to recognize that they also have nominal liabilities. Because wages are adjusted infrequently, even someone whose nominal wage increase is adjusted for expected inflation is worse off the higher the inflation rate.

More passionate concerns about inflation reflect the fear that it is a signal of a society and a government out of control—and that hyperinflation, which destroys the "existing basis of society" (Keynes, 1919, p. 78), waits at the end of the road.[35] Accounts of hyperinflations make it clear that they were profoundly disturbing events, including most of the phenomena described by Buchanan and Wagner.

But hyperinflation is not the inevitable result of low double-digit inflation. More likely, an equilibrium is established with the inflation rate fluctuating around a moderate level. But with no long-run tradeoff between inflation and unemployment, there is nothing favorable to be said for moderate rates of inflation except that they are costly to reduce. The inflation is itself costly because of the money triangle, uncertainty, relative price distortions, and institutional non-adaptations. The marginal cost of inflation is high enough for inflationary disturbances to be countered by contractionary policy. Society has at that point to make the hard choices it did not make at a lower inflation rate, and is in addition paying a price for having decided not to fight earlier. But none of this is to say that the costs of low rates of inflation, 5 percent or less, are such as to justify the typical inflationary rhetoric.

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34. Peretz (1983) reviews much of the recent evidence on the effects of inflation and measures of output or unemployment on presidential popularity and voting patterns.

35. It is sometimes pointed out that Hitler came to power during a period of high unemployment, and not as a direct result of the German hyperinflation. Keynes' dictum stands even so.

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Commentary

Robert J. Shiller

Fischer's paper is the culmination of a series of important papers (one jointly with John Huizinga and one with Franco Modigliani) in which he enumerated the various costs of inflation and attempted a quantitative evaluation of these costs. We have learned a great deal from these papers. The enumeration included not only the obvious costs but also some less obvious and less easily quantified ones. It was surprising to see how *many* costs that we do not usually consider may rank in importance with the obvious ones. This list of costs of inflation must surely be welcome to policymakers who need some guidance as to what is important and what isn't.

This list is of course not the list that we would really have liked to have: a list of the relative costs and benefits of policies to deal with inflation. The whole reason for enumerating the costs of inflation is, apparently, to provide some guidance to policymakers. But by providing this enumeration, Fischer is not solving any of the fundamental problems in macroeconomic theory. These fundamental problems concern the interpretation of the correlations observed among macroeconomic variables in terms of a causal structure of the macroeconomy.

His list of costs of inflation seems to include any costs that a) are correlated with inflation and b) sound in some loose, intuitive sense like a part of the inflation process itself rather than of some other part of the business cycle. The source of this intuitive sense is not always presented to the reader. He does not include costs associated with variables related to the level of economic activity that are correlated with inflation.

Why does he not count wars as a cost of inflation? Wars are certainly correlated with inflation. Some of the fundamental economic problems that he associates with inflation might be transformed but not go away any more than wars would go away following an anti-inflationary policy.

In spite of this undeniably fundamental problem with the interpretation

of his analysis, I do feel that by plunging ahead and making *some* accounting of the costs, Fischer has taught us a lot, so that his series of papers, with their creative empirical work, ranks as one of the major contributions to monetary economics in the last decade.

Apparently, from his accounting, the important costs of inflation are not what economists would think of first. Fischer points out that the pure economic cost of inflation, measured by welfare economists as the area of a certain triangle and representing the inconveniences that people suffer in economizing on cash balances, must be weighed against the welfare costs of other modes of taxation. In an earlier paper (1981b), Fischer presented some rough calculations, using Hausman's estimates of the ratio of excess burden to government revenue for labor income taxation, which suggested that a 9 percent inflation rate is probably too high. However, this conclusion appears to be rather imprecise, and it is certainly vulnerable to changes in transaction technology that might alter the demand curve for money. There is certainly no economic case against moderate inflation from these calculations. The cost of inflation that economists think of first, and which is clearly logically related to inflation, may not be a cost at all.

As Fischer himself suggests, all the remaining costs of inflation are costs of phenomena that we do observe with inflation but that have no necessary logical connection with inflation. These remaining costs of inflation are placed into three categories: costs of institutional nonadaptations, costs of price level uncertainty, and costs of relative price variability.

The institutional nonadaptations he refers to are apparently largely imposed by governments: nonindexation of government debt, legal restrictions preventing indexation of private debt, nonindexation of the tax system, and ceilings on nominal interest rates. The private sector institutional nonadaptations might be corrected if the government led the way. For example, he says that indexed private annuities would probably appear if indexed government bonds existed.

The price level uncertainty that he associates with inflation is also not necessarily logically connected with inflation. His scatter diagram in Figure 1 shows that some high-inflation countries have had low price level uncertainty. The Okun-Flemming explanation of the correlation between inflation levels and inflation variance that he cites attributes it to a tendency for policy regime shifts to accompany inflation. Anyway, the costs of inflation would largely disappear if the economy were more fully indexed. We thus do not need to eliminate inflation to deal with this cost.

The relative price variability that is associated with inflation is not logically related with inflation either. He does not show here a scatter diagram

(like his Figure 1) between inflation rates and the variance of relative price movements for various years, but his regression results in an earlier paper, with quarterly **U.S.** data from 1948 to 1980 (1981b), show an R^2 of only around 0.4.[1] Thus, there ***are*** times of high inflation and low relative price variability. There is no reason to think that a deliberate policy of maintaining a higher inflation rate would cause higher relative price variability. In fact, his own econometric analysis (1981a) suggests that the observed correlation of relative price variability with inflation is largely due to the effect on both of energy and food supply shocks, evidence of problems an anti-inflation policy would not eliminate.

It's also not obvious that the relative price variability that tends to accompany inflation is a cost and not a benefit. We must know what happens to an appropriately defined measure of real income when inflation variability increases. There is a theorem in welfare economics that people are made ***better*** off by price level variability if their real income (measured using the stable prices before the variability) is not affected by the variability. Fischer addressed this issue before (1981a).

Fischer concludes that this standard list of costs of inflation really amounts to nothing much at all, for inflations of moderate range or variability, if the government takes steps to allow indexation.

He says that the reason governments resist indexation is that they deliberately wish to keep inflation painful to prove their resolve to contain it, and to constrain themselves from failing to do so. But I think that a more important reason may be that political systems do not deal well with problems whose solutions are poorly understood by the public, due to what he calls 'nominal thinking." For example, the public has shown little interest in inflation-adjusted earnings figures even though these make eminently good sense. If the government were to revise its deficit accounting to take account of the erosion in the real value of private debt, the public might tend to view this as a trick.

Fischer is right that nominal thinking is the core of the problem here. The source of all these institutional nonadaptations may ultimately be human error: difficulty in comprehending the arithmetic of inflation correction. The benefits of price stability here may thus be analogous to the benefits of our way of implementing daylight savings time: by setting clocks forward. We don't ask everyone individually to get up an hour earlier, come to work an hour earlier, etc., because people would find it

1. Fischer (1981b), Table 3, p. 32.

difficult to subtract 1 from all the times on their schedule. How much more difficult than subtracting 1 from all the times on one's schedule it is to make all the necessary inflation corrections! Even for such a simple matter as comparison shopping people must, in an inflationary environment, remember not only prices but dates when prices were observed, as well as inflation rates over the various intervals. A result of inflation is thus that many simple errors are made (and this may be part of the reason for the correlation between inflation and relative price variability). Stable prices should be viewed as great simplifiers of our lives.

Let me say something in closing about the quotation from Buchanan and Wagner at the beginning of Fischer's paper, a quotation that attributes a sort of cost to inflation that is not in Fischer's list, and a cost that is allegedly very big. I suspect that this quote would win widespread applause from the general public (though they might think it a little overstated), even if we economists are inclined not to take it seriously. Inflation, in this view, "increases the sense of felt injustice and causes alienation," and "prompts the behavioral respqnses that reflect a general shortening of time horizons. Enjoy, enjoy."

Despite the overstatement, there is something that seems possibly true in this statement: People do seem to regard inflation as a major injustice to them, and this sense of injustice might have some effect on their ideals or social commitment. The views of the common man are the issues here, and these may be described most accurately by relying on surveys that document actual, widely held views.

The inflationary period since the mid-1960s has in fact been a period of increasing alienation. The Hams Poll has since 1966 asked a battery of questions aimed at gauging the level of alienation: "The rich get richer and the poor get poorer," "Most people with power try to take advantage of people like yourself," etc. The level of alienation as indicated by agreement with such statements has shown a steady increase since 1966.[2] Poll analysts Lipset and Schneider thought that this increase in alienation was related to inflation: "The effects of inflation can be seen clearly: It decreases optimism and increases pessimism about peoples' lives, the country, and the economy."[3]

Katona (1975) has provided a useful summary of the lessons from 30 years of data collected by the Survey Research Center of the Institute for Social Research at the University of Michigan. People, he said, *resent* price

2. See Lipset and Schneider (1983), p. 110.
3. Ibid., p. 145.

increases. Someone has *cheated* them, they think, when an item they are interested in has a higher price than it had a month or two earlier: "'Right' or 'normal' prices, as well as prices which are 'too high' have psychological meaning even though from an economic point of view they are undefinable concepts."[4]

One might have thought that the sense of injustice comes largely from the creditors (particularly those who lent to the government), but this point is not mentioned by Katona. An important factor contributing to the actual sense of injustice is that people do not *see* their own wage increases as part of an inflationary process, but tend to interpret the increases instead as the result of their own accomplishments. This fact has been widely mentioned, but the survey data that are the source of the observation are not widely cited. In Survey Research Center surveys taken in 1968-70, those respondents who said their income was higher than it was four years ago were asked why they were now making more. Of the respondents, 44 percent answered in terms of their own efforts: "Did good job, worked hard, deserved increase, advance in career, acquired more skill, experience, or changed job to a better one." Only 25 percent answered in terms of references to external causes, such things as inflation, business conditions, or labor unions. Only *6* percent mentioned inflation per se as the cause of their wage increase.[5]

Respondents were asked who is hurt most by inflation. "Overwhelmingly, people replied that poor people or the little man was hurt most, and only one out of five mentioned people with fixed or stable incomes. . . . Practically nobody said that lenders lose and borrowers profit from inflation"[6]

Fischer and Huizinga (1982) looked at other survey evidence regarding the 'misunderstanding hypothesis: the idea that people fail to see the connection between their own income increases and inflation. They summarize the evidence for this hypothesis as "mixed." However, none of the survey evidence cited there repeated Katona's question asking respondents to come up with a reason why their income increased. Every survey question they cited directly asked respondents to assess the effects of inflation on income. It's not inconsistent with the misunderstanding hypothesis that people answer as they do to such question.

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4. Ibid.

5. Ibid., p. 191. Katona reported a lower proportion who attributed their wage increases to their own efforts in surveys taken in Europe, so that what we observe here may to some extent be a cultural phenomenon in the United States.

6. Ibid., p. 142.

The perceived costs of inflation by a public that thinks inflation is the No. 1 problem in the country[7] have little relation to the ***actual*** costs of inflation, and this perception may have important consequences. Well be happy to leave this dilemma to the policymakers themselves.

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7. The Gallup Poll reports that when people were asked 'What do you think is the most important problem facing this country today?" answers classified as 'inflation" or "high cost of living" ranked first or second in every year from 1973 through 1983.

3
Estimated Tradeoffs Between Unemployment and Inflation

Ray C. Fair

An important question in macroeconomics is the size of the tradeoff between unemployment and inflation. I have been asked by the organizers of this symposium to consider this question, and so this is yet another paper on the tradeoff issue. Given an econometric model of price and wage behavior, it is straightforward to compute the tradeoff. The key problem is finding the model that best approximates the unknown structure, and this problem is the focus of this paper.

Three models of price and wage behavior are considered. The first, Model 1, is the one contained in my macroeconomic model of the United States (Fair, 1984). The second, Model 2, is one that is closer to what might be considered the standard model in the literature. The third, Model 3, is one in which there is no long-run tradeoff between unemployment and inflation. Model 3 is Model 2 with a certain restriction on the coefficients.

The paper is organized as follows. Some methodological issues are discussed first. The models are then presented, estimated, and tested. The unemployment-inflation tradeoffs implied by each model are then presented, and the final section contains a general evaluation of the results and a discussion of their consequences for macroeconomic policy and research.

Some methodology

It will be useful to present a few of my views about macroeconomic research before launching into the specification of the equations. The first issue concerns how much information one expects to get out of macro time series data. Consider, for example, the question of which demand variable to use in a price or wage equation. My experience is that macro data are not capable of discriminating among many different measures of

demand. Similar results are obtained using such variables as the overall unemployment rate, the unemployment rate of married men, various weighted unemployment rates, various output gaps, and various nonlinear functions of these variables.[1] It is also difficult to discriminate among alternative lag distributions for the explanatory variables, a point made by Griliches (1968) many years ago and one that still seems valid.

If one feels, as I do, that macro data contain a fairly limited amount of information, the obvious procedure to follow in econometric work is to keep the specifications simple. If the data cannot discriminate among alternative detailed specifications, there is no sense in making detailed specifications in the first place. One should also avoid making strong inferences from results that are sensitive to alternative specifications among which the data may not be able to discriminate. This is an obvious point, but it is perhaps worth emphasizing. In particular, note that one should be wary about making strong conclusions regarding the validity of a model's long-run properties. This is because long-run properties are likely to be sensitive to alternative lag distributions, which are in turn likely to be difficult to discriminate among.

The approach of keeping macro specifications fairly simple is at odds with the approach of Robert Gordon and George Perry, two of the leading figures in the field of price and wage behavior. Gordon's specifications are characterized by the use of high-order polynomial distributed lags with long lag lengths, the use of detailed dummy variables, and considerable work in the construction of many of the explanatory variables. One reason that Gordon's specifications change so much from year to year is probably that they are too detailed to be supported by the data. New data seem to imply a change in specification when in fact no specification for a given year is really supported.[2] Perry's specifications are also usually somewhat involved, especially with respect to the choice of the demand variable and the use of dummy variables[3] It will be clear in what follows that my specifications are simpler than those of Gordon and Perry, and one should keep in mind my reason for this difference.

Another view I have about macroeconomic research is that there have been too few attempts to test one model against another. One reason there

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1. See, for example, the discussion in Fair (1978), pp. 176-80, and in Fair (1984), p. 128-29.

2. A minor but illustrative example of Gordon's changing specifications concerns the use of dummy variables for the Nixon control period. In Gordon (1980) one dummy variable is used, which is 0.67 for 1971:III-1972:IV, − 1.0 for 1974:II-1975:I, and 0.0 otherwise. In Gordon and King (1982) two variables are used. One is 0.8 for 1971:III-1972:II and 0.0 otherwise, and the other is 0.4 for 1974:II and 1975:I, 1.6 for 1974:III and 1974:IV, and 0.0 otherwise.

3. See, for example, the specifications in Perry (1980).

is currently so much disagreement in macroeconomics is probably that there has been so little testing of alternative specifications. I developed a few years ago a method for testing alternative models (Fair [1980]), and this is the method that I have used in this paper to compare the three models of price and wage behavior. One of the premises upon which this method is based is that all models are at least somewhat misspecified. An important feature of the method is that it accounts for the effects of misspecification in making the comparisons across models.

Finally, my approach in examining macroeconomic issues is to specify and estimate structural equations. A few years ago this was standard operating procedure, but it is now somewhat out of fashion. Some have turned to vector autoregressive equations, while others have turned to reduced form equations. In his recent work, for example, Gordon has switched to estimating reduced form price equations.[4] The reduced form approach ignores potentially important restrictions on the reduced form coefficients, and in this sense it is inefficient. Also, it is not possible in Gordon's recent work to know whether a variable that is added to the reduced form price equation belongs in the structural price equation, in the structural wage equation, or in both. Important questions about the wage-price process are simply left unanswered when only reduced form equations are estimated. For example, one important question with respect to a particular set of structural wage and price equations is whether the implied behavior of the real wage is sensible, and this question cannot be answered by the reduced form approach. Real wage behavior is considered below.

The three models

Model 1

Model 1 is the model of price and wage behavior in my U.S. model. The following is a brief discussion of it. A more complete discussion is contained in Fair (1984). Firms in the theoretical model are assumed to set prices and wages in a profit-maximizing context. They have some monopoly power in the short run in their price- and wage-setting behavior. Raising their prices above prices charged by other firms does not result in an immediate loss of all their customers, and lowering their prices below prices charged by other firms does not result in an immediate gain of everyone else's customers. There is, however, a tendency for high-price firms to lose customers over time and for low-price firms to gain customers. Similar statements hold for wages. Firms expect that the future prices and wages

4. See, for example, Gordon (1980) and Gordon and King (1982).

of other firms are in part a function of their own past prices and wages. Since a firm's market share is a function of its price relative to the prices of other firms, its optimal price strategy depends on this relationship. Expectations of firms are in some cases determined in fairly sophisticated ways, but none of the expectations are rational in the Muth sense. Firms do not know the complete model, and their expectations can turn out to be incorrect.

There are five main decision variables of a firm in the theoretical model. In addition to the firm's price level and wage rate, the variables are the firm's production, investment, and demand for employment. These decision variables are determined by solving a multiperiod maximization problem. The predetermined variables that affect the solution to this problem include (1) the initial stocks of excess capital, excess labor, and inventories, (2) the current and expected future values of the interest rate, (3) the current and expected future demand schedules for the firm's output, (4) the current and expected future supply schedules of labor facing the firm, and (5) expectations of other firms' future price and wage decisions.

The transition in macroeconomics from theoretical models to econometric specifications is usually difficult, and the present case is no exception. The aim of the econometric work is to try to approximate the decision equations of the firms that result from the solutions of the maximization problems. The empirical work for the price and wage equations consisted of trying the variables listed above, directly or indirectly, as explanatory variables. Observed variables were used directly, and unobserved variables were used indirectly by trying observed variables that seemed likely to affect the unobserved variables. The main unobserved variables are expectations.

I will not review here the work that led to the final estimated equations; this is discussed in Fair (1984, pp. 126-31). The final estimated equations are presented in Table 1. The equations are in log form. The explanatory variables in the price equation include the price level lagged once, the wage rate inclusive of employer Social Security taxes, the price of imports, and the unemployment rate lagged once. The unemployment rate is taken to be a proxy for the current and expected future demand schedules for the firms' output. For the work in Fair (1984) an alternative measure of demand was used, which was a measure of the real output gap. As noted above, a variety of demand variables work about equally well. The unemployment rate was used in this paper in order to make the tradeoff calculations below somewhat simpler. The other three variables in the price equation are taken to be proxies for expectations of other firms' price decisions. Increases in the lagged price level, the wage rate, and the price of

TABLE 1
The Price and Wage Models
Sample Period is 1954:I–1984:I (121 observations)

Dependent Variable	*Explanatory Variables Model 1*						
log P_t	*const.*	*log* P_{t-1}	*log* $W_t(1+d_t)$	*log PIM,*	UR_{t-1}	*SE*	*DW*
2SLS	.159	.937	.0268	,0335	−.205	.00377	1.75
	(7.32)	(107.01)	(6.33)	(11.05)	(6.19)		
3SLS	.160	.936	.0271	.0336	−.205	.00377	1.74
	(7.42)	(107.99)	(6.43)	(11.24)	(6.26)		
3SLS[a]	.164	.934	.0279	.0340	−.201	.00377	1.74
	(7.66)	(109.60)	(6.68)	(11.53)	(6.15)		
log W_t	*const.*	*log* W_{t-1}	*log* P_t	*log* P_{t-1}	t	UR_t	
2SLS	−.477	.921	.503	−.456	.000754	−.0753	.00578 1.99
	(1.69)	(20.13)	(3.47)	(3.49)	(1.93)	(1.22)	
3SLS	−.293	.951	.514	−.485	.000493	−.0716	.00581 2.04
	(1.08)	(21.77)	(3.64)	(3.80)	(1.32)	(1.18)	
3SLS[a]	−.291	.951	.515	−.485	.000479	−.0799	.00581 2.04
	(2.73)	(52.50)		(5.35)	(3.61)	(1.62)	

Models 2 and 3

log P_t − *log* P_{t-1}	*const.*	*log* P_{t-1} − *log* P_{t-2}	*log* $W_{t-1}(1+d_{t-1})$ − *log* $W_{t-5}(1+d_{t-5})$	*log* PIM_{t-1} − *log* PIM_{t-3}		
Model 2: OLS	−,00260	.293	.146	.0582	,00404	2.04
	(2.07)	(3.73)	(5.27)	(5.78)		
Model 2: 3SLS	−.00264	.292	.147	.0578	,00404	2.04
	(2.11)	(3.72)	(5.31)	(5.74)		
Model 3: 3SLS[b]	−.00536	.323	.191	.0461	.00415	2.04
	(5.48)	(4.14)	(7.77)	(4.87)		
log W_t − *log* W_{t-1}	*const.*	*log* P_{t-1} − *log* P_{t-5}		UR_t		
Model 2: 2SLS	.0142	.175		−.114	,00565	1.96
	(7.48)	(8.69)		(3.27)		
Model 2: 3SLS	.0142	.175		−.116	.00565	1.96
	(7.52)	(8.68)		(3.30)		
Model 3: 3SLS[b]	.0144	.221		−.151	,00578	1.87
	(7.60)			(4.50)		

Notes: t-statistics in absolute value are in parentheses.

[a]Coefficient constraint (4) in text imposed on the equations.

[b]Coefficient constraint (10) in text imposed on the equations.

OLS = ordinary least squares

2SLS = two stage least squares

3SLS = three stage least squares

First stage regressors:

A = second basic set of variables in Fair (1984), Table 6.1, p. 228.

Model 1, 2SLS, log P, eq. : A minus ZZ_{t-1} plus log $(1+d_t)$. (ZZ is a demand pressure variable.)

Model 1, 2SLS, log W, eq.: A plus log PX_{t-1}. (PX is a price deflator.)

Model 1, 3SLS : A plus log $(1+d_t)$ plus log PX_{t-1}.

Model 2, 2SLS : A plus log PX_{t-1} plus log P_{t-1} − log P_{t-5}.

Models 2 and 3, 3SLS : A plus log $(1+d_t)$ plus log PX_{t-1} plus log P_{t-1} − log P_{t-5} plus log PIM_{t-1} − log PIM_{t-3} plus log $W_{t-1}(1+d_{t-1})$ − log $W_{t-5}(1+d_{t-5})$ plus log P_{t-1} − log P_{t-2}

Variable	*Notation in Fair (1984)*	*Description*
d_t	$d_{5g} + d_{5s}$	Employer social security tax rate
P_t	P_f	Price deflator for private nonfarm output
PIM_t	PIM	Price deflator for imports
UR_t	UR	Civilian unemployment rate
W_t	W_f	Average hourly earnings excluding overtime of workers in the private sector

imports are assumed to lead to expectations of future price increases, which in the theoretical model lead to an increase in current prices.

The explanatory variables in the wage equation include the wage lagged once, the current price level, the price level lagged once, a time trend, and the unemployment rate. The unemployment rate is taken to be a proxy for the current and expected future supply schedules of labor facing the firms. The lagged wage variable and the current and lagged price variables are taken to be proxies for expectations of other firms' wage decisions. Increases in these variables are assumed to lead to expectations of future wage increases, which in the theoretical model lead to an increase in current wages. The time trend was added to account for trend changes in the wage rate relative to the price level. The inclusion of the time trend is important, since it helps identify the price equation. Aside from the different lags for the unemployment rate, the time trend and the lagged wage rate are the only two variables not included in the price equation that are included in the wage equation.[5]

Before discussing the estimates, a constraint that was imposed on the real wage rate needs to be explained. It does not seem sensible for the real wage rate (W_t/P_t) to be a function of either W_t or P_t separately, and in order to ensure that this not be true, a constraint on the coefficients of the price and wage equations must be imposed. The relevant parts of the two equations are

$$(1) \quad \log P_t = \beta_1 \log P_{t-1} + \beta_2 \log W_t + \ldots$$

$$(2) \quad \log W_t = \gamma_1 \log W_{t-1} + \gamma_2 \log P_t + \gamma_3 \log P_{t-1} + \ldots.$$

From these two equations, the equation for the real wage is

$$\log W_t - \log P_t = \frac{1}{1 - \beta_2\gamma_2}\gamma_1(1 - \beta_2) \log W_{t-1} - \frac{1}{1 - \beta_2\gamma_2}[\beta_1(1 - \gamma_2) - \gamma_3(1 - \beta_2)]\log P_{t-1} + \ldots.$$

5. There is one slight difference between the wage equation here and the one in Fair (1984). The same price deflator is used in both equations here (the private nonfarm deflator), whereas a different price deflator is used in the wage equation in Fair (1984) (the private deflator, both farm and nonfarm). This difference is not important in the sense that the data cannot discriminate between the two, and the simpler specification was used here for ease of interpretation.

In order for the real wage not to be a function of the wage and price levels, the coefficient of log W_{t-1} in (3) must equal the negative of the coefficient of log P_{t-1}. This requires that

$$(4) \quad 0 = (\gamma_1 + \gamma_3)(1 - \beta_2) - \beta_1(1 - \gamma_2).$$

Three sets of estimates of Model 1 are presented in Table 1. The estimation technique for the first set is two-stage least squares (2SLS), and the estimation technique for the second and third sets is three-stage least squares (3SLS).[6] Restriction (4) is imposed for the third set, but not for the first and second. The endogenous variables in the price equation are log P_t and log W_t, and the endogenous variables in the wage equation are log W_t, log P_t, and UR_t. UR_t is taken to be an endogenous variable even though no equation is specified for it in this paper. It is an endogenous variable in my U.S. model. The first-stage regressors that were used for the estimates are discussed in the notes to Table 1. The basic set of variables referred to in the notes consists of 34 variables. These are the main predetermined variables in my U.S. model. The 2SLS estimated residuals were used for the estimation of the covariance matrix of the error terms that is needed for the 3SLS estimates. The correlation coefficient for the error terms in the two equations was -0.299.

The data base used in Fair (1984) was updated through 1984:I for the results in this paper. The estimation period for all the equations in Table 1 is 1954:I-1984:I, which is a total of 121 observations.

The three sets of estimates of Model 1 are quite close, and there is little to choose among them. The coefficient restriction (4) is clearly supported by the data. The value of the 3SLS objective function was -96.471 for the unrestricted estimates and -96.567 for the restricted estimates, for a difference of only 0.096. This difference is asymptotically distributed as χ^2 with one degree of freedom, and the 0.096 value is far below the critical χ^2 value at the 95 percent confidence level of 3.84.

Model 1 differs from traditional models of wage and price behavior in a number of ways, and it will be useful to discuss two of these differences. First, most price and wage equations are specified in terms of rates of change of prices and wages rather than in terms of levels. Given the theory behind Model 1, the natural decision variables seemed to be the levels of

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6. All calculations for this paper, except for those in the section on properties of the models, were done using the Fair-Parke program. The Parke (1982) algorithm was used to compute the 3SLS estimates.

prices and wages rather than the rates of change, and so this was the specification used. For example, the market share equations in the theoretical model have a firm's market share as a function of the ratio of the firm's price to the average price of other firms. These prices are all price levels, and the objective of the firm is to choose the price level path (along with the paths of the other decision variables) that maximizes the multiperiod objective function. A firm decides what its price level should be relative to the price levels of other firms. The use of levels instead of rates of change has important consequences for the long-run properties of the model. This is discussed below.

Second, most price equations are postulated to be markup equations, where little or no demand effects are expected. Wage equations are postulated to be the ones where demand effects are most likely to exist. Model 1 is to some extent the reverse of this. The unemployment rate has a larger coefficient estimate (in absolute value) and is more significant in the price equation than in the wage equation. Also, the coefficient estimate of the wage rate in the price equation is too small to be interpreted as a markup coefficient. The theory behind the price and wage equations is not a markup theory, and so there is no reason to expect the estimated equations to have properties of markup equations. The equations do not appear to have such properties.

Model 2

As just noted, price and wage equations are typically specified in terms of rates of change of prices and wages rather than in terms of levels, and price equations are typically specified to be markup equations. This specification has been used for Model 2. I tried a number of equations that seemed consistent with this specification. The final equations are presented in Table 1.

The equations for Model 2 are in log form. The quarterly change in price is a function of the quarterly change in price lagged once, the four-quarter change in the wage rate lagged once, and the two-quarter change in the import price deflator lagged once. The quarterly change in the wage is a function of the four-quarter change in the price level lagged once, and of the unemployment rate. These equations are consistent with the interpretation of the price equation as a markup equation and of the wage equation as the one in which demand effects appear. The unemployment rate appears in the wage equation but not in the price equation. It was of the wrong sign and not significant when included in the price equation (both the current rate and the rate lagged one quarter were separately

tried). The following is a discussion of some of the experimentation behind the choice of the final equations.

The data seemed to support the use of the four-quarter change in the wage lagged once in the price equation. When the four one-quarter changes, $\log W_{t-i}(1 + d_{t-i}) - \log W_{t-i-1}(1 + d_{t-i-1}), i = 1, 2, 3, 4$, were used in place of the four-quarter change, the coefficient estimates and t-statistics were: 0.139 (2.33), 0.144 (2.41), 0.181 (3.00), and 0.120 (1.97). These coefficients seemed close enough to warrant simply using the four-quarter change. When the one-quarter change unlagged was included with the other four one-quarter changes, it was not significant (coefficient estimate of 0.071, with t-statistic of 1.17). Similarly when the one-quarter change lagged five quarters was included with the other four, it was not significant (coefficient estimate of −0.001, with t-statistic of −0.02). The data seemed to support the use of the two-quarter change in the price of imports lagged once. When the one-quarter changes lagged once and twice were used in place of the two-quarter change, the coefficient esimtates and t-statistics were 0.0674 (3.20) and 0.0477 (2.03).

The quarterly change in the wage rate lagged once was not significant when added to the wage equation. The t-statistic was only −0.49. The use of the four-quarter change in the price in the wage equation was supported less than was the use of the four-quarter change in the wage in the price equation, but the four-quarter change in the price was used in the wage equation anyway. When the four one-quarter changes were used in place of the four-quarter change, the coefficient estimates and t-statistics were 0.249 (2.22), 0.126 (1.07), −0.017 (−0.14), and 0.352 (2.94). When the one-quarter change unlagged was included with the other four one-quarter changes, it was not significant (coefficient estimate of 0.110, with t-statistic of 0.72). Similarly, when the one-quarter change lagged five quarters was included with the other four, it was not significant (coefficient estimate of −0.120, with t-statistic of −1.05). When the one-quarter changes lagged five and six quarters were included with the other four, the coefficient estimates and t-statistics were −0.099 (0.84) and −0.079 (0.72). There is thus no evidence that price changes lagged more than four quarters belong in the wage equation.

Two sets of estimates of Model 2 are presented in Table 1. The estimation techniques for the first set are ordinary least squares for the price equation and 2SLS for the wage equation. The estimation technique for the second set is 3SLS. There are no endogenous explanatory variables in the price equation. The unemployment rate in the wage equation was taken to be an endogenous variable. The two sets of estimates are very

close. The correlation coefficient for the error terms in the two equations was only 0.030, and so very little was gained by using 3SLS. Comparing the single-equation fits with those for Model 1, the price equation has a larger standard error (0.00404 versus 0.00377) and the wage equation has a smaller standard error (0.00565 versus 0.00581).

Model 3

As will be seen in a later section, there is a tradeoff between the unemployment rate and inflation implicit in Model 2.[7] There is, however, a restriction that can be placed on the coefficients of Model 2 that implies no long-run tradeoff. Model 3 is Model 2 with this restriction imposed. The restriction is as follows. Let $\dot{p}_{t-i} = \log P_{t-i} - \log P_{t-i-1}$ and $\dot{w}_{t-i} = \log W_{t-i} - \log W_{t-i-1}$, i = 0, 1, . . . , 4. Write the price and wage equations of Model 2 as

$$\dot{p}_t = Z_t + \beta_1 \dot{p}_{t-1} + \beta_2(\dot{w}_{t-1} + \dot{w}_{t-2} + \dot{w}_{t-3} + \dot{w}_{t-4}), \tag{5}$$

$$\dot{w}_t = \gamma_0 + \gamma_1(\dot{p}_{t-1} + \dot{p}_{t-2} + \dot{p}_{t-3} + \dot{p}_{t-4}) + \gamma_2 UR_t, \tag{6}$$

where $Z_t = \beta_0 + \beta_2[\log(1 + d_{t-1}) - \log(1 + d_{t-5})] + \beta_3(\log PIM_{t-1} - \log PIM_{t-3})$. Consider now a steady state where $p = \dot{p}_t = \dot{p}_{t-1} = \ldots$, $\dot{w} = \dot{w}_t = \dot{w}_{t-1} = \ldots$, $Z = Z_t = Z_{t-1} = \ldots$, and $UR = UR_t = UR_{t-1} \ldots$. In this case (5) and (6) can be written

$$\dot{p} = Z + \beta_1 \dot{p} + 4\beta_2 \dot{w}, \tag{7}$$

$$\dot{w} = \gamma_0 + 4\gamma_1 \dot{p} + \gamma_2 UR. \tag{8}$$

Substituting (8) into (7) and rearranging terms yields

$$(1 - \beta_1 - 16\beta_2\gamma_1)\,\dot{p} = Z + 4\beta_2\gamma_0 + 4\beta_2\gamma_2 UR. \tag{9}$$

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7. There is a tradeoff in the sense that given the two estimated equations of Model 2, a change in the unemployment rate leads to a finite long-run change in the rate of inflation. This assumes that the structure of the wage and price equations is stable over time. For example, part of what the equations are picking up are effects of expectations of future wage and price behavior on current behavior. If the expectation mechanism that is approximated by the equations changes, for whatever reason, the stability assumption is violated. Sargent (1971) has stressed the fact that estimated coefficients of lagged dependent variables in wage and price equations are picking up both the effects of lagged values on expected future values and the effects of expected future values on current values. Without extra assumptions, it is not possible to separate the two kinds of effects. For present purposes it is unnecessary to do this if one is willing to make the above stability assumption, as is done here.

$$(10) \quad 1 - \beta_1 - 16\beta_2\gamma_1 = 0.$$

there is no long-run tradeoff, and this is the restriction that was imposed on Model 3.

The estimates with this restriction imposed are presented in Table 1. The equations were estimated by 3SLS, where UR, was treated as an endogenous variable. The value of the 3SLS objective function was -116.669 for the unrestricted estimates and -128.525 for the restricted estimates, for a difference of 11.856. Again, this difference is asymptotically distributed as χ^2 with one degree of freedom. The 11.856 value is considerably above the critical χ^2 value at the 95 percent confidence level of 3.84, and so the restriction is not supported by the data. The single equation fits for the price and wage equations are 0.00415 and 0.00578 for the restricted estimates, which compare to 0.00404 and 0.00565 for the unrestricted estimates.

Given the coefficient estimates of Model 3 and given an assumption about the long-run value of Z, one can compute the value of the unemployment rate (say UR*) for which inflation neither accelerates nor decelerates. Under the assumption that the long-run growth rate of d_t is zero and that the long-run growth rate of the import price deflator is 7.0 percent at an annual rate, the value of UR* is 6.25 percent. This value is simply computed by solving the equation $0 = Z + 4\beta_2\gamma_0 + 4\beta_2\gamma_2 UR$ for UR. The long-run rate of change of the price level that corresponds to this value of UR is 3.39 percent at an annual rate. The corresponding growth rate for the nominal wage is 5.06 percent, and the corresponding growth rate for the real wage is 1.62 percent.

A comparison of the models

Although the single equation fits are available fmm Table 1, these fits are not the appmpriate criterion for comparing the models. Among other things, they do not test for the dynamic accuracy of the models, and they do not account in an explicit way for the possible misspecification of the models. The method in Fair (1980) can be used to compare models, and this method is used in this section to compare the three models.

The method accounts for the four main sources of uncertainty of a forecast: uncertainty due to 1) the error terms, 2) the coefficient estimates, 3) the exogenous variables, and 4) the possible misspecification of the model. Because it accounts for these four sources, it can be used to make comparisons across models. In other words, it puts each model on an equal footing for purposes of comparison. Exogenous variable uncertainty is not a problem in the present case because each model has the same exogenous variables,

namely d, and PIM,. Therefore, exogenous variable uncertainty has not been taken into account: both d_t and PIM, have been assumed to be known with certainty. The following is a brief outline of the method except for the part pertaining to exogenous variable uncertainty.

The method

Assume that the model has m stochastic equations, p unrestricted coefficients to estimate, and T observations for the estimation. The model can be nonlinear, simultaneous, and dynamic. Let S denote the covariance matrix of the error terms, and let V denote the covariance matrix of the coefficient estimates. S is m x m and V is p x p. An estimate of S, say $\hat{S}$, is $(1/T)UU'$, where U is an m x T matrix of estimated errors. The estimate of V, say $\hat{V}$, depends on the estimation technique used. Let $\hat{\alpha}$ denote a p-component vector of the coefficient estimates, and let u_t denote an m-component vector of the error terms for period t.

Uncertainty from the error terms and coefficient estimates can be estimated in a straightforward way by means of stochastic simulation. Given assumptions about the distributions of the error terms and coefficient estimates, one can draw values of both error terms and coefficients. For each set of values the model can be solved for the period of interest. Given, say, J trials, the estimated forecast mean and estimated variance of the forecast error for each endogenous variable for each period can be computed. Let $\tilde{y}_{itk}$ denote the estimated mean of the k-period-ahead forecast of variable i, where t is the first period of the forecast, and let $\tilde{\sigma}^2_{itk}$ denote the estimated variance of the forecast error. $\tilde{y}_{itk}$ is simply the average of the J predicted values from the J trials, and $\tilde{\sigma}^2_{itk}$ is the sum of squared deviations of the predicted values from the estimated mean divided by J.

It is usually assumed that the distributions of the error terms and coefficient estimates are normal, although the stochastic-simulation procedure does not require the normality assumption. The normality assumption has been used for the results in this paper. Let u_t^* be a particular draw of the error terms for period t, and let a^* be a particular draw of the coefficients. The distribution of u_t^* is assumed to be $N(0,\hat{S})$, and the distribution of a^* is assumed to be $N(\hat{\alpha},\hat{V})$.

Estimating the uncertainty from the possible misspecification of the model is the most difficult and costly part of the method. It requires successive reestimation and stochastic simulation of the model. It is based on a comparison of estimated variances computed by means of stochastic simulation with estimated variances computed from outside-sample (i.e., outside the estimation period) forecast errors. Assuming no

stochastic-simulation error, the expected value of the difference between the two estimated variances for a given variable and period is zero for a correctly specified model. The expected value is not in general zero for a misspecified model, and this fact is used to try to account for misspecification.

Without going into details, the basic procedure is to estimate the model over a number of different estimation periods and for each set of estimates to compute the difference between the two estimated variances for each variable and length ahead of the forecast. The average of these differences for each variable and length ahead provides an estimate of the expected value. Let $\bar{d}_{ik}$ denote this average for variable i and length ahead k. Given $\bar{d}_{ik}$, the final step is to add it to $\tilde{\sigma}^2_{itk}$. This sum, which will be denoted $\hat{\sigma}^2_{itk}$, is the final estimated variance. Another way of looking at $\bar{d}_{ik}$ is that it is the part of the forecast-error variance not accounted for by the stochastic-simulation estimate.[8]

The results

Table 2 contains the results. The values in the a rows are stochastic-simulation estimates of the forecast standard errors based on draws of error terms only. The values in the b rows are based on draws of both error terms and coefficients. The results are based on 500 trials for each of the two stochastic simulations.[9] The simulation period is 1982:II-1984:I. In terms of the above notation, the b-row values are values of $\tilde{\sigma}^2_{itk}$. Each model consists of three equations: the price equation, the wage equation, and an identity determining the real wage, W/P.

For the misspecification results, each model was estimated and stochastically simulated 37 times.[10] For the first set, the estimation period ended

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8. Strictly speaking, $\bar{d}_{ik}$, is not a measure of the misspecification of the model (for the k-period-ahead forecast of variable i). Misspecification can affect the stochastic simulation estimate of the variance, ($\tilde{\sigma}^2_{itk}$), and $\bar{d}_{ik}$ is merely the effect of misspecification on the total variance not reflected in $\tilde{\sigma}^2_{itk}$. For purposes of comparing the models, it does not matter how much of the misspecification is in $\tilde{\sigma}^2_{itk}$. The variance that is used for comparison is the total variance, $\hat{\sigma}^2_{itk}$.

9. The 3SLS estimates of each model were used for these simulations, including the 3SLS estimates of S and V. The errors in Table 2 are in units of percent of the forecast mean. See the discussion in Chapter 8 in Fair (1984) for the exact way in which the percentage errors are computed.

10. Because the 0LS-2SLS and 3SLS estimates of Model 2 were so close for the results in Table 2, the 0LS-2SLS techniques were used for the successive reestimation for Model 2. Estimating a model 37 times by 3SLS is expensive, and for Model 2 it seemed unnecessary to do this. The estimate of V for the 0LS-2SLS techniques was assumed to be block diagonal for purposes of the stochastic simulation draws. Both Models 1 and 3 were estimated 37 times by 3SLS.

TABLE 2
Estimated Standard Errors of Forecasts for 1982:II–1984:I for the Three Models

	1982 II	1982 III	1982 IV	1983 I	1983 II	1983 III	1983 IV	1984 I
Price level (P)								
Model 1: a	.37	.51	.61	.69	.75	.78	.83	.86
b	.37	.54	.67	.79	.87	.98	1.03	1.15
d	.50	.83	1.11	1.47	1.84	2.21	2.55	2.94
Model 2: a	.41	.66	.88	1.11	1.38	1.62	1.90	2.17
b	.39	.68	.93	1.21	1.51	1.79	2.09	2.42
d	.53	.99	1.45	1.99	2.59	3.18	3.80	4.51
Model 3: a	.41	.70	.98	1.27	1.59	1.94	2.33	2.75
b	.43	.73	1.00	1.31	1.71	2.10	2.54	3.05
d	.49	.85	1.17	1.59	2.13	2.65	3.09	3.67
Nominal wage (W)								
Model I: a	.54	.78	.96	1.06	1.17	1.26	1.32	1.40
b	.57	.78	.98	1.18	1.40	1.51	1.64	1.82
d	.52	.72	.87	1.00	1.24	1.47	1.77	2.10
Model 2: a	.54	.76	.98	1.20	1.41	1.68	1.90	2.15
b	.56	.83	1.10	1.38	1.72	2.07	2.41	2.72
d	.54	.80	.99	1.21	1.61	2.16	2.54	2.95
Model 3: a	.57	.82	1.05	1.30	1.60	1.93	2.25	2.65
b	.60	.87	1.13	1.41	1.76	2.14	2.57	2.98
d	.66	1.08	1.41	1.71	2.13	2.63	2.99	3.28
Real wage (W/P)								
Model I: a	.62	.90	1.10	1.19	1.30	1.38	1.45	1.52
b	.66	.94	1.15	1.29	1.49	1.63	1.74	1.89
d	.70	.92	1.07	1.14	1.35	1.55	1.82	2.22
Model 2: a	.67	.88	1.04	1.15	1.20	1.27	1.31	1.40
b	.68	.97	1.13	1.27	1.39	1.47	1.54	1.59
d	.73	1.01	1.22	1.45	1.60	1.69	1.84	1.97
Model 3: a	.66	.93	1.08	1.10	1.14	1.20	1.26	1.36
b	.71	1.01	1.20	1.25	1.33	1.35	1.39	1.46
d	.78	1.06	1.28	1.47	1.58	1.64	1.81	1.96

Notes: a = Uncertainty due to error terms.
b = Uncertainty due to error terms and coefficient estimates.
c = Uncertainty due to error terms, coefficient estimates, and the possible misspecification of the model.
Errors are in percentage points.

in 1974:IV and the simulation period began in 1975:I. For the second set, the estimation period ended in 1975:I and the simulation period began in 1975:II. For the final set, the estimation period ended in 1983:IV and the simulation period began in 1984:I. The beginning quarter was 1954:I for all estimation periods. The length of the first 30 simulation periods was eight quarters. Since the data set ended in 1984:I, the length of the 31st simulation period, which began in 1982:III, was only seven quarters. Similarly, the length of the 32nd period was six, and so on through the length of the 37th period, which was only one quarter. For each of the 37 sets of estimates, new estimates of

V and S were obtained. Each of the 37 stochastic simulations was based on 200 trials.

The results produced for the one-quarter-ahead forecast for each of the three endogenous variables 37 values of the difference between the estimated forecast-error variance based on outside-sample errors (i.e., the squared forecast errors) and the estimated forecast-error variance based on stochastic simulation. The average of these 37 values was taken for each variable. In terms of the above notation, this average is $\bar{d}_{il}$, where i refers to variable i and the 1 refers to the one-quarter-ahead forecast. The total variance for the one-quarter-ahead forecast of variable i is $\hat{\sigma}^2_{itl} + \bar{d}_{il}$, which in terms of the above notation is $\hat{\sigma}^2_{itl}$. For the results in Table 2, t is 1982:II, and the d-row value for 1982:II for each variable is the square root of $\hat{\sigma}^2_{itl}$. The calculations for the two-quarter-ahead forecasts are the same except that there are only 36 values of the difference between the two estimated variances for each variable. Similarly, there are only 35 values for the three-quarter-ahead forecast, and so on.

The d-row values in Table 2 can be compared across models. For both the price level and the nominal wage, Model 1 is the clear winner. It has the lowest standard errors for all the periods except for the one-quarter-ahead forecast of the price level, where the standard error is 0.50 for Model 1 and 0.49 for Model 3. By the end of the eight-quarter horizon, the differences in the standard errors are fairly large. For the price level, the eight-quarter standard errors are 2.94 for Model 1, 4.51 for Model 2, and 3.67 for Model 3. For the nominal wage, the errors are 2.10 for Model 1, 2.95 for Model 2, and 3.28 for Model 3. With respect to Model 2 versus Model 3, Model 3 does better for prices and Model 2 does better for wages.

The results for the real wage are closer. Model 1 is the best for the first six quarters, the models essentially tie for the seventh quarter, and Models 2 and 3 are better than Model 1 for the eighth quarter. In general the results are fairly close, and there is no clearcut winner.

Properties of the models

For each model, it is straightforward to compute the tradeoff between the unemployment rate and inflation. A simulation is first run using a particular value of the unemployment rate, and then another simulation is run using another value. The differences in the predicted values from the two simulations are the estimated tradeoffs. Before doing this, however, it will be useful to consider some issues regarding the behavior of the real wage.

Real wage issues

There appear to be constraints on the long-run behavior of the real wage that are not necessarily captured by equations like those for Models 1, 2, and 3. Consider, for example, a profit share variable, denoted SHRa, which is defined to be the ratio of after-tax profits of the firm sector to the wage bill of the firm sector net of employer Social Security taxes.[11] The mean of this variable for the 1954:I-1984:I period is 0.109, with a maximum value of 0.136 in 1979:III and a minimum value of 0.066 in 1983:I. The variable has essentially no trend throughout this period. A regression of SHRa on a constant term and time trend for this period yields a coefficient estimate of the time trend of −0.000084, with a t-statistic of −1.91. This coefficient multiplied by 121, the number of observations, yields −0.010, which is the estimated trend change in $SHR\pi$. This is a fairly small change over the 30-year period.

Now, a fall in the level of the real wage of 1 percent leads to a rise in SHRa of approximately 0.0075. If a given experiment with the price and wage equations results in a large change in the long-run level of the real wage, this may imply values of SHRa that are considerably beyond the historical range. If so, this may call into question the long-run properties, since there may be forces at work (not captured by the equations) keeping SHRa at roughly a constant level in the long run. It is thus important when examining the following results to look carefully at the long-run behavior of the real wage.

Unemployment-inflation tradeoffs

Results for the first set of experiments are presented in Table 3. The first simulation for each model began in 1984:II, which means that the initial conditions through 1984:I were used. The simulation was allowed to run for 140 quarters. An unemployment rate of 7.8 percent was used for all future periods. The annual rate of growth of the import price deflator was taken to be 7.0 percent. The rate of growth of the employer Social Security tax rate (d_s) was taken to be zero throughout the period. The second simulation for each model differed from the first only in the unemployment rate that was used. Unemployment was lowered to 6.8 percent for all future periods for this simulation. The results in Table 3 are the differences between the two simulations.

As can be seen, the models have quite different long-run properties. For Model 1, the 1 percentage point drop in the unemployment rate leads to an eventual rise in the price level of 5.15 percent and in the wage level of 4.81

11. $SHR\pi$ is a variable in my U.S. model. See Fair (1984) for the precise definition of it.

TABLE 3
Response of Prices and Wages to a One Percentage Point Fall in the Unemployment Rate

	Model 1						*Model 2*						*Model 3*					
Quarters Ahead	$\frac{P^b}{P^a}$	$\dot{P}^b - \dot{P}^a$	$\frac{W^b}{W^a}$	$\dot{W}^b - \dot{W}^a$	$\frac{W^b/P^b}{W^a/P^a}$	$\left(\frac{\dot{W}^b}{P^b}\right)-\left(\frac{\dot{W}^a}{P^a}\right)$	$\frac{P^b}{P^a}$	$\dot{P}^b - \dot{P}^a$	$\frac{W^b}{W^a}$	$\dot{W}^b - \dot{W}^a$	$\frac{W^b/P^b}{W^a/P^a}$	$\left(\frac{\dot{W}^b}{P^b}\right)-\left(\frac{\dot{W}^a}{P^a}\right)$	$\frac{P^b}{P^a}$	$\dot{P}^b - \dot{P}^a$	$\frac{W^b}{W^a}$	$\dot{W}^b - \dot{W}^a$	$\frac{W^b/P^b}{W^a/P^a}$	$\left(\frac{\dot{W}^b}{P^b}\right)-\left(\frac{\dot{W}^a}{P^a}\right)$
1	1.0000	0.01	1.0008	0.35	1.0008	0.32	1.0000	0.0	1.0012	0.48	1.0012	0.48	1.0000	0.0	1.0015	0.63	1.0015	0.61
2	1.0021	0.88	1.0026	0.79	1.0005	−0.10	1.0002	0.07	1.0023	0.48	1.0022	0.40	1.0003	0.12	1.0030	0.63	1.0027	0.49
3	1.0041	0.84	1.0044	0.76	1.0003	−0.10	1.0006	0.16	1.0035	0.50	1.0030	0.32	1.0010	0.28	1.0046	0.65	1.0036	0.36
4	1.0060	0.80	1.0061	0.73	1.0001	−0.09	1.0012	0.26	1.0048	0.52	1.0036	0.25	1.0021	0.45	1.0063	0.71	1.0043	0.25
5	1.0079	0.77	1.0077	0.70	0.9999	−0.08	1.0021	0.37	1.0062	0.57	1.0041	0.20	1.0036	0.64	1.0083	0.82	1.0047	0.16
6	1.0096	0.74	1.0093	0.67	0.9997	−0.08	1.0031	0.41	1.0077	0.64	1.0046	0.22	1.0054	0.73	1.0106	0.96	1.0052	0.21
7	1.0113	0.71	1.0108	0.64	0.9995	−0.07	1.0041	0.44	1.0094	0.70	1.0052	0.24	1.0075	0.83	1.0133	1.10	1.0058	0.25
8	1.0130	0.68	1.0123	0.62	0.9993	−0.07	1.0053	0.48	1.0112	0.75	1.0058	0.25	1.0098	0.94	1.0163	1.22	1.0064	0.26
9	1.0145	0.65	1.0137	0.59	0.9992	−0.06	1.0066	0.53	1.0131	0.79	1.0064	0.25	1.0124	1.08	1.0196	1.33	1.0070	0.23
10	1.0160	0.62	1.0150	0.57	0.9990	−0.06	1.0080	0.57	1.0151	0.82	1.0070	0.23	1.0155	1.21	1.0231	1.43	1.0075	0.19
11	1.0175	0.60	1.0163	0.55	0.9989	−0.06	1.0095	0.61	1.0171	0.85	1.0076	0.22	1.0188	1.35	1.0269	1.53	1.0079	0.16
12	1.0188	0.57	1.0176	0.53	0.9988	−0.05	1.0110	0.64	1.0192	0.88	1.0081	0.22	1.0225	1.47	1.0310	1.65	1.0083	0.15
13	1.0202	0.55	1.0188	0.51	0.9986	−0.05	1.0127	0.67	1.0214	0.90	1.0086	0.22	1.0265	1.59	1.0354	1.77	1.0087	0.15
14	1.0214	0.53	1.0199	0.49	0.9985	−0.05	1.0144	0.70	1.0237	0.93	1.0092	0.22	1.0308	1.71	1.0401	1.88	1.0090	0.14
15	1.0226	0.51	1.0210	0.47	0.9984	−0.04	1.0162	0.72	1.0260	0.95	1.0097	0.21	1.0355	1.83	1.0452	1.99	1.0093	0.13
16	1.0238	0.48	1.0221	0.45	0.9983	−0.04	1.0180	0.74	1.0284	0.97	1.0102	0.21	1.0405	1.96	1.0505	2.10	1.0096	0.11
17	1.0249	0.46	1.0231	0.43	0.9982	−0.04	1.0198	0.76	1.0308	0.99	1.0108	0.21	1.0459	2.08	1.0562	2.21	1.0098	0.09
18	1.0260	0.45	1.0241	0.41	0.9981	−0.04	1.0218	0.78	1.0333	1.00	1.0113	0.21	1.0516	2.21	1.0621	2.32	1.0100	0.08
19	1.0270	0.43	1.0250	0.40	0.9980	−0.03	1.0237	0.80	1.0358	1.02	1.0118	0.21	1.0577	2.33	1.0685	2.43	1.0102	0.06
20	1.0280	0.41	1.0259	0.38	0.9980	−0.03	1.0257	0.81	1.0383	1.03	1.0123	0.20	1.0641	2.45	1.0751	2.54	1.0103	0.05
30	1.0360	0.27	1.0333	0.26	0.9974	−0.02	1.0474	0.90	1.0655	1.11	1.0173	0.20	1.1504	3.66	1.1613	3.62	1.0095	−0.10
40	1.0412	0.18	1.0382	0.17	0.9971	−0.01	1.0709	0.93	1.0947	1.14	1.0222	0.19	1.2829	4.83	1.2895	4.68	1.0052	−0.24
60	1.0470	0.08	1.0438	0.08	0.9969	−0.00	1.1209	0.95	1.1566	1.16	1.0318	0.19	1.7514	7.09	1.7267	6.73	0.9859	−0.53
80	1.0496	0.03	1.0462	0.03	0.9968	−0.00	1.1736	0.95	1.2224	1.16	1.0416	0.19	2.7077	9.23	2.5812	8.68	0.9533	−0.83
100	1.0508	0.02	1.0474	0.02	0.9968	−0.00	1.2289	0.95	1.2921	1.16	1.0514	0.19	4.7408	11.25	4.3077	10.54	0.9086	−1.13
120	1.0513	0.01	1.0479	0.01	0.9968	−0.00	1.2868	0.95	1.3657	1.16	1.0613	0.19	9.3999	13.17	8.0258	12.32	0.8538	−1.42
140	1.0515	0.00	1.0481	0.00	0.9968	−0.00	1.3474	0.95	1.4435	1.16	1.0714	0.19	21.1064	14.98	16.6934	14.00	0.7909	−1.72

Notes. [a]Predicted value for a sustained unemployment rate of 7.8 percent.
[b]*Predicted value for a sustained unemployment rate of 6.8 percent*
[*]Percentage change at an annual rate.

Initial conditions were the actual values through 1984 I.
The import price deflator was assumed to grow at an annual rate of 7.0 percent throughout the period.
The rate of growth of d_t was assumed to be zero throughout the period.

percent. The real wage falls slightly (by 0.32 percent). At the end of the first year the price level is 0.60 percent higher; at the end of the second year it is 1.30 percent higher; and at the end of the fourth year it is 2.38 percent higher, which is about halfway to the final increase of 5.15 percent. Not counting the first quarter, the increase in the rate of growth of the price level falls from 0.88 in the second quarter, to 0.80 in the fourth quarter, to 0.68 in the eighth quarter, to 0.48 in the sixteenth quarter, and to zero after 140 quarters. A similar pattern holds for the nominal wage.

For Model 2, the 1 percentage point drop in the unemployment rate leads to an eventual increase in the rate of change of the price level of 0.95 percent. The eventual increase in the rate of change of the nominal wage is 1.16 percent, and the eventual increase in the rate of change of the real wage is 0.19 percent. The price and wage levels are, of course, ever-increasing. After 140 quarters the price level is 34.74 percent higher, the nominal wage is 44.35 percent higher, and the real wage is 7.14 percent higher. At somewhere between 30 and 40 quarters, the price level becomes 5.15 percent higher, which is the long-run total for Model 1.

It is interesting to compare the first few quarters for Models 1 and 2. The rate of inflation is initially much larger for Model 1 than for Model 2. After eight quarters the price level is 1.30 percent higher for Model 1, compared to 0.53 percent higher for Model 2. The rate of inflation for Model 1 falls from 0.88 in the second quarter to 0.68 in the eighth quarter. For Model 2 the rate of inflation rises from 0.07 in the second quarter to 0.48 in the eighth quarter. There is thus much more of a short-run tradeoff for Model 1 than for Model 2. The rates of inflation cross at quarter 11, where they are 0.60 for Model 1 and 0.61 for Model 2. After quarter 11 the rate of inflation rises to 0.95 for Model 2 and falls to zero for Model 1. The price levels cross somewhere between quarters 20 and 30.

Consider now the results for Model 3. The unemployment rates of 6.8 and 7.8 percent are above the non-decelerating rate of 6.25, and so for both simulations the rate of inflation is decelerating. Although not shown in Table 3, the rate of inflation becomes negative in quarter 18 for the simulation in which the unemployment rate is 7.8 percent. By quarter 140 the rate of inflation is −20.96 percent. The differences in Table 3 for Model 3 are thus differences between two decelerating paths. It is interesting to note that the differences for the first few quarters for Model 3 are not all that different from the differences for Model 2, although they are somewhat higher for Model 3.

With respect to the behavior of the real wage, the results for Model 1 show little change in the long-run level of the real wage. The fall in the

TABLE 4

Response of Prices and Wages to a One Percentage Point Increase in the Rate of Change of the Import Price Deflator

Quarters Ahead	Model 1						Model 2						Model 3					
	$\frac{P^b}{P^a}$	$\dot{P}^b - \dot{P}^a$	$\frac{W^b}{W^a}$	$\dot{W}^b - \dot{W}^a$	$\frac{W^b/P^a}{W^b/P^a}$	$\left(\frac{\dot{W}^b}{P^b}\right) - \left(\frac{\dot{W}^a}{P^a}\right)$	$\frac{P^b}{P^a}$	$\dot{P}^b - \dot{P}^a$	$\frac{W^b}{W^a}$	$\dot{W}^b - \dot{W}^a$	$\frac{W^b/P^b}{W^a/P^a}$	$\left(\frac{\dot{W}^b}{P^b}\right) - \left(\frac{\dot{W}^a}{P^a}\right)$	$\frac{P^b}{P^a}$	$\dot{P}^b - \dot{P}^a$	$\frac{W^b}{W^a}$	$\dot{W}^b - \dot{W}^a$	$\frac{W^b/P^b}{W^a/P^a}$	$\left(\frac{\dot{W}^b}{P^b}\right) - \left(\frac{\dot{W}^a}{P^a}\right)$
1	1.0001	0.03	1.0000	0.02	1.0000	−0.02	1.0000	0.0	1.0000	0.0	1.0000	0.0	1.0000	0.0	1.0000	0.0	1.0000	0.0
2	1.0002	0.07	1.0001	0.03	0.9999	−0.03	1.0001	0.06	1.0000	0.0	0.9999	−0.05	1.0001	0.04	1.0000	0.0	0.9999	−0.04
3	1.0005	0.10	1.0002	0.05	0.9998	−0.04	1.0004	0.13	1.0000	0.01	0.9996	−0.11	1.0004	0.10	1.0000	0.01	0.9997	−0.09
4	1.0008	0.12	1.0004	0.07	0.9996	−0.06	1.0008	0.15	1.0001	0.03	0.9993	−0.11	1.0007	0.12	1.0001	0.03	0.9994	−0.09
5	1.0011	0.15	1.0006	0.08	0.9995	−0.07	1.0012	0.16	1.0002	0.06	0.9990	−0.10	1.0010	0.14	1.0002	0.06	0.9993	−0.08
6	1.0015	0.18	1.0008	0.10	0.9993	−0.08	1.0016	0.17	1.0005	0.09	0.9988	−0.08	1.0014	0.15	1.0005	0.09	0.9991	−0.06
7	1.0020	0.20	1.0011	0.11	0.9990	−009	1.0021	0 19	1.0007	0.11	0.9986	−0.08	1.0018	0.17	1.0007	0.11	0.9990	−0.06
8	1.0026	0.23	1.0013	0.12	0.9988	−0.10	1.0026	0.21	1.0010	0.12	0 9984	−0.09	1.0023	0.20	1.0011	0.13	0.9988	−0.07
9	1.0032	0.25	1.0017	0.13	0.9985	−0.11	1.0031	0.23	1.0013	0.13	0.9982	−0.10	1.0028	0.23	1.0014	0.15	0.9986	−0.08
10	1.0038	0.27	1.0020	0.15	09982	−0.12	1.0037	0.24	1.0016	0.14	0.9979	−0.10	1.0034	0.25	10018	0 17	0.9984	−0.09
11	1.0045	0.29	1.0024	0.16	0 9979	−0.13	1.0043	0.25	1.0020	0.15	09977	−0.10	1.0041	0.27	10023	0.19	0.9982	−0.09
12	1.0052	0.31	1.0028	0 17	0.9976	−0.14	1.0050	0.26	1.0024	0.16	0.9974	−0.10	1.0048	0.29	1.0028	0.21	0.9980	−0.09
13	1.0060	0.33	1.0032	0.18	0.9972	−0.14	1.0056	0.27	1.0028	0.17	0.9972	−0.10	1.0056	0.32	1.0034	0.23	0.9977	−0.09
14	1.0068	0.34	1.0036	0.19	0.9968	−0.15	1.0063	0.28	1.0033	0.18	0.9970	−0.10	1.0065	0.34	1.0040	0.25	0.9975	−0.09
15	1.0077	0.36	1.0041	0.20	0.9965	−0.16	1.0070	0.29	1.0037	0.19	0.9967	−0.10	1.0074	0.36	1.0047	0.27	0.9973	−0.09
16	1.0086	0.38	1.0046	0.21	0.9961	−0.16	1.0078	0.30	1.0042	0.20	0.9965	−0.10	1.0084	039	1.0054	0.29	0.9971	−0.10
17	10095	0.39	1.0051	0.22	0.9956	−0.17	1.0085	0.31	1.0047	0.20	0.9962	−0.10	1.0094	0.41	1.0062	0.31	0.9968	−0.10
18	1.0105	041	10056	0.23	0.9952	−0.17	1.0093	0.31	1.0052	0 21	0.9959	−0.10	1.0105	0.43	1.0070	0.33	09966	−0.10
19	1.0115	042	1.0062	0.24	0.9948	−0.18	1.0101	0.32	1.0057	0.21	0.9957	−0 10	1.0116	0.45	1.0079	0.35	0.9963	−0.10
20	10125	043	10067	0.24	0.9943	−0.18	10108	0.32	10062	0.22	0.9954	−0 10	1.0128	0.47	1.0088	0 37	0.9960	−0 11
30	1.0242	053	10133	0.31	0.9893	−0.22	10193	0.36	10120	0.25	0.9928	−0.11	1.0282	069	1.0210	0.57	0.9930	−0.13
40	1.0379	0.59	10211	0.35	0.9838	−0.23	1.0283	0.37	1.0181	0.26	0.9901	−0.11	1.0499	0.90	1.0387	0.76	0.9894	−0.16
60	1.0687	0.65	1.0389	0.39	0.9721	− 025	1 0470	0.37	1.0310	0.26	0.9848	− 0 11	1.1138	1.30	1 0917	1.13	0.9802	−0.22
80	1.1025	0.68	1.0585	0.41	0.9601	− 025	1.0661	0.38	1.0442	0.27	09794	−0.11	1.2093	1.67	11712	1.47	0.9685	−0.27
100	1.1381	0.69	1.0791	0.42	0.9482	−0.25	1.0856	0.38	1.0575	0.27	0.9741	−0.11	1.3437	2 01	1.2824	1.79	0.9544	− 0 33
120	1.1752	0.69	1.1004	0.43	0.9364	−0.25	1.1055	0.38	1.0710	0.27	0.9688	−0.11	1.5280	2.32	1.4332	2.09	0.9380	−0.39
140	1.2137	0.69	1.1223	0.43	0 9247	−0.25	1.1257	0.38	1.0847	0.27	0.9636	−0.11	1 7782	2.61	1.6350	2.36	0 9194	−0.44

Notes [a] Predicted value for an annual rate of change of the import price deflator of 7.0 percent.
[b] Predicted value for an annual rate of change of the import price deflator of 8.0 percent.
[c] Percentage change at an annual rate.

Initial conditions were the actual values through 1984 I.
The unemployment rate was assumed to be 7 8 percent throughout the period.
The rate of growth of d_t was assumed to be zero throughout the period.

unemployment rate lowered the long-run level of the real wage by only 0.32 percent. The results for Model 2, on the other hand, show that the level of the real wage is ever increasing. After 140 quarters the level of the real wage is 7.14 percent higher, which implies a fall in SHR_π of approximately 0.0075 x 7.14 = 0.054. This is about five times larger than the trend change over the last 121 quarters between 1954:I and 1984:I. The long-run properties of Model 2 with respect to the real wage are thus questionable.

Effects of a change in import prices

One can also examine how the models respond to a change in import prices. Again, two simulations can be run, one using one set of values for future import prices and one using another. The results of this exercise are presented in Table 4. The first simulation used an annual rate of change of import prices of 7.0 percent, and the second used a rate of 8.0 percent. The initial conditions were the same as those for the simulations in Table 3. An unemployment rate of 7.8 percent was used for these results.

The increase in the rate of change of import prices led to an increase in the rate of change of prices and wages for both Models 1 and 2. For prices, the long-run effect is 0.69 for Model 1 and 0.38 for Model 2. For wages, the two numbers are 0.43 and 0.27. The long-run rate of change in the real wage fell in both cases. The fall was larger for Model 1 than for Model 2 (-0.25 vs. -0.11). Although the long-run properties differ somewhat, the short-run properties of the two models are quite close, as an be seen from examining, say, the first eight quarters in Table 4. The short-run results for Model 3 are also fairly close to those for Models 1 and 2. The long-run results for Model 3 are, of course, vastly different.

All three models have ever falling real wage levels, which is not sensible. All three models are thus at fault in this regard. This problem is discussed in the next section.

General remarks

Long-run tradeoffs

The two key questions regarding the long-run tradeoff between unemployment and inflation are 1) whether there is any tradeoff and 2) if there is one, whether it is in terms of the level of prices or the rate of change of prices. The results of comparing the three models above indicate that Model 1 is more accurate than Models 2 and 3, and so from these results one would conclude that there is a tradeoff and that it is in terms of

the level of prices. If the choice is merely between Models 2 and 3, the results are inconclusive.[12]

Although Model 1 does seem to be the best approximation of the three, the results must be interpreted with considerable caution. As noted in the first section, macro data have a difficult time discriminating among alternative lag distributions, and alternative lag distributions can have large effects on the long-run properties of a model. One should clearly put much less weight on the long-run properties of the models than on the short-run properties (say, up to eight or twelve quarters ahead).

One may at first be surprised to think that the tradeoff between unemployment and inflation may be in terms of the level of prices rather than the rate of change, but there is no theoretically compelling reason to rule out the level tradeoff without testing the two possibilities. As noted above, it seems natural, given my theoretical model, to specify the price and wage equations in level terms. In general, there seems no reason to expect that a permanent shift in demand will necessarily lead to a permanently higher rate of change of prices and thus to an ever-increasing price level. At the least, this issue seems open to empirical test, and the tests in this paper provide support for the proposition that the tradeoff is in terms of levels.

Another point that should be kept in mind about Model 1 is the following. One might argue—I think correctly—that it is not sensible to expect that the unemployment rate could be driven to, say, 1.0 percent without having any more effect on prices than on their levels. (The same argument could even be made for Model 2 regarding the rates of change of prices.) There are clearly unemployment rates below which it is not sensible to assume that any of the three models provides a good approximation. Any attempt to extrapolate a model beyond the extremes of the data is dangerous, and this seems particularly true in the case of price and wage equations.

I sometimes try to account for the nonlinearities in price responses that one expects to exist as the unemployment rate approaches very low levels by using, as the demand variable in the price and wage equations, some function of the unemployment rate (or other measure of demand). These functions approach infinity or minus infinity as the unemployment rate approaches some small value. This means that as the unemployment rate

12. In future work it may be possible to provide a better test of Model 2 versus Model 3. The comparisons in this paper were only for forecasts up to eight quarters ahead. It can be seen from Table 3 that the main differences between the two models occur after eight quarters. It may thus be possible to get more conclusive results by using a forecast horizon longer than eight quarters. No attempt was made to do this in this study.

approaches this value, prices approach infinity. In a complete model of the economy, prices can never by driven to infinity, and so this approach effectively bounds the unemployment rate from below. The problem with this approach is that the data generally cannot discriminate among alternative functional forms, and so any choice is somewhat arbitrary. The approach that I have taken in this paper is to keep the specification simple by merely using the level of the unemployment rate as an explanatory variable. The consequence of this is that one should not extrapolate the equations much beyond the range of the historical data.

The real wage and the price of imports

One of the most serious problems with the models considered in this paper is that the long-run behavior of the real wage is a function of the price of imports. In each model the price of imports is in the price equation but not in the wage equation, and the reduced form equation for the real wage has the price of imports on the right hand side with a negative coefficient. In order to constrain the price of imports not to have a long-run effect on the real wage, one would have to add it to the wage equation (with perhaps a different lag from the one in the price equation) and constrain the coefficients in the two equations to imply no long-run effect of the price of imports on the real wage.

Another possible way to look at this problem is the following. Over the sample period there has been a certain trend change in the price of imports. The coefficient estimates of the price and wage equations are based on this trend. In the case of Model 1, the key coefficient estimate is the estimate of the time trend in the wage equation. Given that the coefficient estimates are based on this trend, it is not necessarily sensible to run an experiment in which the rate of change of the price of imports is permanently changed without also changing the coefficient estimate of the time trend in the wage equation to adjust for this trend change. **A** similar adjustment should be made to one or both of the constant terms in Model 2. With these adjustments, the models would still show an increase in the rate of change of prices and wages in response to the increase in the rate of change of the price of imports, but the coefficient adjustments could be made to show no change in the real wage in the long run. This type of adjustment would imply no changes in the estimated equations, only changes in the coefficients at the time of a particular experiment.

It should be noted that an answer to the real wage problem is not to use as the price of imports variable in the price equation the price of imports ***relative*** to the domestic price level (i.e., PIM relative to P). Consider, for

example, the price equation for Model 1 in Table 1, and assume that the price of imports variable were log PIM, $-$ log P_{t-1} rather than log PIM,. Since log P_{t-1} is already in the equation, this change merely has the effect of making the new coefficient of log P_{t-1} equal to the old coefficient plus the coefficient of log PIM,. The reduced form equation for the real wage would still be the same.

The question of the nominal price of imports versus the relative price of imports brings up an important issue about the experiments in Table 4. Consider Model 1. The increase in the rate of change in the price of imports of 1.0 percent led to a long-run increase in the rate of change in the domestic price of 0.69 percent, which implies a long-run increase in the rate of change in the relative price of imports of about 0.31 percent. Although the relative price of imports fluctuates considerably in the short-run and even in the intermediate run, it is not necessarily sensible to assume that it will continually rise or fall in the very long run. One may thus want to design experiments in which the relative price of imports does not change in the long run. Again, however, this issue is separate from the problem of the real wage being a function of the price of imports.

If one believes that the nominal price of imports should be constrained to grow at the same rate as the domestic price level in the long run, then the coefficient constraint imposed on Model 3 should be changed. The constraint (10) should read $1 - \beta_1 - 16\beta_2\gamma_1 - 2\beta_3 = 0$, where β_3 is the coefficient of log PIM_{t-1} $-$ log PIM_{t-3} in the price equation. This was not done for the present set of results.

It is clear that more work needs to be done regarding the long-run behavior of the real wage and the price of imports. In some cases alternative specifications should be tried, such as the choice of constraint imposed on Model 3, and in some cases alternative experiments should be designed. This is an important area for future research.

Policy options

There is little more to be said about policy options that is not obvious from the results in Table 3. If one believes that Model 1 is the best approximation, the tradeoffs can be read from the results for Model 1. The cost of a fall in the unemployment rate of 1 percentage point is an increase in the price level of 1.30 percent after 8 quarters. If Model 2 is chosen, the cost is an increase of 0.53 percent after 8 quarters. If one's horizon is 20 quarters, the estimated cost is about the same for both models: 2.80 percent for Model 1 and 2.57 percent for Model 2. After 20 quarters, the estimated costs from the two models diverge rapidly, and this is where the most

uncertainty lies. For Model 1 there is an increase in the price level of 5.15 − 2.60 = 2.55 percent left. For Model 2 there is an increase in the rate of change of prices of 0.95 − 0.81 = 0.14 left.

Consequences for macroeconomic research

One of the important results of this paper is that the no long-run tradeoff model, Model 3, does not appear to be as good an approximation to the economy as does Model 1. The comparison with Model 2 is inconclusive, although it is certainly not the case that Model 3 dominates Model 2. This result has important consequences for macroeconomic research. Economists with such diverse views as Tobin and Lucas seem to agree with the Friedman-Phelps proposition that there is no long-run tradeoff between unemployment and inflation. (See Tobin [1980], p. 39, and Lucas [1981], p. 560.) Lucas (1981) points out in his review of Tobin's (1980) book that most of the recent developments in macroeconomic theory have been motivated by the problem of reconciling the natural rate hypothesis of Friedman and Phelps with an adequate treatment of output and employment fluctuations. I think Lucas is right in arguing that Tobin cannot accept the proposition of no long-run tradeoff and at the same time accept short-run propositions that do not imply the Friedman-Phelps proposition in the long run. The long run is simply a sequence of short runs.

Where I think both Tobin and Lucas have missed the mark is in so readily accepting the Friedman-Phelps proposition. The evidence in this paper suggests that this proposition may not be true, and at the least, the validity of the proposition is highly uncertain. It seems unwise to me to have based more than a decade of macroeconomic research on such a proposition. The present results suggest that more thought should be given to the possibility that the concept of a natural rate of unemployment is not a useful one upon which to base a theory.[13] One can argue that the present results do not discredit the natural rate hypothesis if one believes that the structure of the price and wage equations is not stable because of shifts in the mechanism by which expectations are formed (see footnote 7). While this is certainly true, it again seems unwise to have based so much research on this particular belief.

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13. The theory upon which my macroeconometric model is based does not use the concept of a natural rate of unemployment. See Fair (1984), in particular pp. 15-16 and 90-91.

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Commentary

Robert J. Gordon

The long-run tradeoff debate in perspective

During much of the past decade the Phillips curve was treated by many macroeconomists as an extinct prehistoric fossil, ridiculed as 'fundamentally flawed" and part of the more general failure of Keynesian macroeconometrics.[1] But more recently a modest revival has begun for the beleaguered Phillips curve, a label that I mean to embrace any dynamic econometric specification in which the rate of change of wages or prices is related to the level of unemployment (or some similar utilization variable) and other factors. This revival is one more example of the impact of economic events on ideas. The Phillips curve had earlier been discredited when its prediction of an inverse relationship between inflation and unemployment was contradicted in the 1970s by the emergence of a positive relationship. The revival can be attributed to the relative success of pre-1981 Phillips curves in tracking the 1981-83 disinflation. Indeed, recent papers by Eckstein (1983), Englander-Los (1983), Perry (1983), Blanchard (1984), and myself (1984) find little evidence of instability in the Phillips curve, nor a failure to track the major portion of the recent disinflation.

Partly because Phillips-curve econometrics has been out of fashion, in recent years there have been relatively few conference sessions devoted to the numerous issues that arise in the specification of wage and price dynamics for the postwar U.S. economy.[2] Several weeks ago Ray Fair and I agreed that this session would provide a useful occasion to expose some of

1. The quotes are from Lucas and Sargent, 1978, pp. 49, 56.

2. This neglect reflects in part the greater attention to long-period historical analyses, as in Schultze (1981, 1984), Taylor (1984), and the references cited therein. There has also been substantial attention to contrasts between the wage-price adjustment process in Europe and the U.S., as in Sachs (1983).

these issues to open discussion and scrutiny, and to facilitate this interchange he provided me with his data, so that we need not be concerned about data discrepancies as a source of differing conclusions in what follows.

Fair's paper raises two major issues that I'll discuss in detail: (1) his evidence against the Friedman-Phelps proposition of no long-run tradeoff, and (2) the case he makes for a simple specification as contrasted with mine that he rightly characterizes as being more detailed in its implementation. His paper also develops a methodology for model comparison that is novel but complex. I view model comparison the same way he views model specification—simpler is better. I'll report comparisons of his and my approaches to specification using the old-fashioned garden-variety criteria of t-ratios and F tests on sets of omitted variables, and Chow tests and post-sample-period dynamic simulations to reveal structural shifts, and I won't try to duplicate or comment on his more involved procedure for model comparison.

Fair's models 1 and 2 incorporate a long-run tradeoff between inflation and unemployment because, as a mechanical matter, the sum of coefficients on lagged inflation in the wage equation is less than unity. His claim that such a wage equation provides evidence against the Friedman-Phelps natural-rate hypothesis (NRH) that no such long-run tradeoff exists immediately confronts the counterargument provided by Sargent (1971). The coefficient on lagged inflation in the wage equation represents a convolution of two separate sets of coefficients that cannot be separately identified: the coefficient on expected inflation, and the coefficient on lagged inflation in the formation of expected inflation. The finding that the product of the two coefficients is less than unity in one particular sample period does not provide any evidence that in another sample period, having a different monetary policy, the same rational agents might not apply a coefficient of unity to past inflation.

The logic of Sargent's argument is asymmetric. It demonstrates that those like Fair who estimate coefficients less than unity provide no evidence ***against*** the NRH, but it does not deny that those who estimate coefficients of unity provide evidence ***consistent*** with the NRH. Here again it is useful to recall the interaction of events and ideas. The Friedman and Phelps argument was brought to public attention in 1967 and 1968, just when the U.S. inflation rate was soaring upward beyond the predictions of the then-dominant econometric models. A last-ditch rear-guard action to defend the negative long-run tradeoff against the NRH was fought in 1969-71 by a number of economists, including myself in two early papers. However, there was no Dunkirk, and we did not escape from the invaders.

Instead, three factors came together to buy forever the opposition to the long-run version of the NRH. First was the failure of inflation to slow down in the recession of 1969-70, leading the Nixon administration in frustration to impose wage and price controls in August 1971. Second was the 1971 Sargent paper. Third was the growing econometric evidence, provided initially by Eckstein-Brinner (1972) and myself (1972), that, as additional data had accumulated, there was no longer evidence that the relevant sum of coefficients on past inflation was significantly less than unity. Thus the econometric argument that Sargent had invalidated could not even by sustained any longer on U.S. postwar data.

Over the past decade, whatever other changes have occurred in the way that Phillips curves are specified and estimated, one constant element has been that the data continue to be consistent with the NRH. Why, then, do the estimates of Fair's models 1 and 2 contain coefficients on past inflation low enough to yield a negatively sloped long-run tradeoff in his simulation exercises? The basic answer, as we shall see below, is an exclusion restriction imposed on his model—he allows only a short lag distribution on past prices, and dropping this restriction by introducing additional lags raises the sum of coefficients to unity.

Issues in the specification of reduced-form Phillips-curve equations

This restriction is just one example of the many choices that must be made in the specification of Phillips curve equations, or, more generally, of any reduced-form characterization of the economy's dynamic aggregate supply schedule. Yet these choices must be made, for too many important issues in understanding macroeconomic behavior and the choices open to policymakers rest on estimates of such schedules. Is there a natural rate of unemployment? Has it changed? How rapidly will inflation accelerate or decelerate when the economy is away from the natural rate? What is the economy's 'sacrifice ratio: that is, the amount of output that must be sacrificed to achieve a permanent reduction of inflation by a given amount? Why were inflation and unemployment related negatively in the 1950s and 1960s but positively in the 1970s?

And there are smaller questions as well, each of which has already stimulated a substantial literature. Does a change in the relative price of oil influence the aggregate price level? Did the Nixon price controls work, temporarily or permanently? Did changes in payroll tax rates or the minimum wage rate aggravate inflation in the past, and would the manipulation of these rates give policymakers an additional instrument to influence the economy's sacrifice ratio? Do changes in the exchange rate and/or

import prices influence domestic inflation, again giving policymakers an influence of the sacrifice ratio through changes in the monetary-fiscal policy mix?

At least in principle, this set of questions can be addressed with a single reduced-form dynamic aggregate supply equation. It is easiest to think of such an equation as quantifying a "triangle" model of inflation. Just as we all know that relative prices depend on demand and supply, so inflation depends on demand and supply. The third side of the triangle, in addition to demand and supply, is inertia, the tendency of the inflation rate to mimic its own past behavior, due to some combination of contracts and costs of adjustment. The reduced form of a two-equation wage-price model like those in Fair's paper and in my early papers, or an explicit single-equation reduced form like those in my more recent papers, includes variables for demand, supply, and inertia. The influence of demand is entered through the level of the unemployment rate or some other economy-wide utilization rate, and perhaps its rate of change. The influence of supply is entered, at least in my work, through a set of changes in relative prices, the effective exchange rate, and effective tax rates, all defined so that when relative prices are constant and the exchange rate and tax rates are steady, the supply variables have a zero influence on inflation. Inertia enters through the influence of past inflation on current inflation, with the length of the lag and the sum of coefficients on past inflation left as an empirical question.

The long set of questions that a dynamic supply schedule is asked to address, and the triangle approach to thinking about that schedule, help to provide a perspective for responding to Fair's criticisms that my inflation equations are 'too detailed" and 'change so much from year to year." First, my equations have not changed in basic format, and have always included variables to represent demand, supply, and inertia. Second, over the years I have addressed each of the questions in the above list, and this leads to a research tradeoff between developing an equation with special features designed to address a particular question, e.g. price controls or flexible exchange rates, and the alternative of attempting to develop a single equation to address all questions. Such an equation, however useful, will' strike as 'too detailed" those who are interested in a smaller set of questions. Third, over the years, responses to the emerging data and to the suggestions of others have inevitably led to constructive changes, including collapsing a two-equation wage-price model into a single-equation reduced-form, and eliminating a variety of specially constructed variables that were originally developed for a two-equation wage-price model but

are no longer necessary within the context of a single reduced-form inflation equation.

An assessment of Fair's model 2

Fair's paper presents three models, each of which contains a separate wage and price equation. Model 1 expresses wages and prices in levels and 2 in rates of change, while 3 differs from 2 by imposing constraints that incorporate the no-long-run-tradeoff (NRH) hypothesis. Leaving aside the constrained model 3, which Fair rejects, there are three reasons to limit our discussion to model 2. First, in most other comparable research, including mine, the dependent variable is the rate of change of prices, not the level. Second, people and policymakers appear to care about the rate of change of prices, not the level of prices. Third, inside model 1 is a rate-of-change equation struggling to get out, since in both the price and wage equations the coefficient on the lagged dependent variable is greater than 0.9.

Fair presents his model in the form of separate wage and price equations, whereas my approach (1982) has been to specify the wage and price equations and then to convert them into a general reduced form before estimation. Here the complex task of comparing alternative specifications is simplified if we solve Fair's two-equation model and convert it into a single equation for the rate of change of prices. When the wage change-equation in model 2 is substituted into the price change equation, we obtain

$$(1)\quad \dot{p}_t = \Theta_0 + \Theta_1 \left[\sum_{i=1}^{4}(UR_{t-1}/4)\right] + \Theta_2\dot{p}_{t-1} + \Theta_3\left\{\sum_{i=2}^{8}[(4 - |5 - i|)/16]\dot{p}_{t-i}\right\}$$

$$+ \Theta_4\left[\sum_{i=1}^{4}(\dot{D}_{t-1}/4)\right] + \Theta_5\left[\sum_{i=1}^{2}(\dot{p}^{IM}_{t-1}/2)\right],$$

where the notation follows Fair, except that

$$\dot{p}^{IM}_t = \log \text{PIM}_t - \log \text{PIM}_{t-1} \text{ and } D_t = \log(1 + d_t).$$

Equation (1) states that the inflation rate depends on four lagged values of the unemployment rate, UR, one lag of the dependent variable, a tent-shaped distribution on lags 2 through 8 of the dependent variable, four lagged values of changes in the employer Social Security tax rate, and two lagged values of changes in the import price deflator. The lag distributions on the unemployment rate, the tax rate, and the import deflator are all constrained to be rectangular. Note that the wage rate drops out of the

reduced form, since lagged wage changes do not appear in Fair's price equation. This aspect of Fair's model is the same as my approach and is supported by the data in both papers (see Gordon, 1982, Table 6).

Since from this point on we limit our discussion to the reduced-form equation 1, it is worthwhile pausing to consider several factors that make such reduced forms preferable to separate wage and price equations. First, separate wage and price equations cannot be distinguished as truly structural equations applying to behavior in particular markets. The behavior of wages, for instance, can be explained just as well by the GNP gap as by labor market variables like unemployment, suggesting that the wage equation does not provide us with any special insight about the working of labor markets. Second, the two-equation approach may be prone to simultaneous equations bias. Third, the use of separate equations led to an artificial separation of the variables that belong in each equation. For instance, the inflationary impact of the payroll tax or the Nixon wage controls depends not on just their coefficient in the wage equation, but also on the response of prices to that particular source of wage variation. Fourth, and perhaps most important, the specification of separate wage and price equations without any attention to the relation between the constant terms in these equations and the rate of productivity growth yields results like those in Fair's Table 3 that changes in nominal GNP growth yield not only permanent changes in the unemployment rate, but also permanent changes in the growth rate of the real wage. If productivity growth is exogenous, then Fair's simulations imply that monetary policy can cause labor's share in national income to veer off to zero or infinity.

Reduced-form equations like (1), as well as the more complex variants used in my work, should be viewed as a convenient characterization of the data rather than an attempt to describe structural behavior. Because the underlying structure may shift, the coefficients in the estimated equation may shift, so that any such single-equation approach should pay special attention to tests of the stability of coefficients across sub-intervals within the sample period.

Table 1 displays estimates of the separate wage and price equations of Fair's model 2 in columns 1a and Ib, and five alternative one-equation reduced forms for inflation in columns 2 through 6. Two differences in the choice of data distinguish the results in Table 1 from related equations that I have estimated (in 1982): The price variable here is the implicit price deflator for nonfarm output rather than the fixed-weight GNP deflator, and the official unemployment rate is used instead of Perry's weighted unemployment rate. Scanning down the left-hand side of the table, explanatory

variables are segregated among the "inertia," "demand: and "supply" categories. The number of lagged terms for each explanatory variable is indicated ("0" indicates the current value, "RD" indicates a rectangular distribution, "T" indicates a tent-shaped distribution as in equation 1, and "U" indicates that the lag coefficients are unconstrained.)

The bottom part of the table displays several summary coefficients and diagnostic checks. First is listed the sum of the coefficients on explanatory variables that are expressed as nominal rates of change, including lagged price changes, wage changes, and nominal import price changes. This is the relevant sum for tests of the long-run NRH (recall that a sum of unity confirms the NRH, but a sum significantly below unity does not reject the NRH, according to the asymmetry imposed by Sargent's argument). Next are two standard errors of estimate (S.E.E.), the first when the sample period terminates in 1984:I and the second for a termination date of 1980:IV. The subsequent line exhibits the F-ratio for a Chow test on a break in 1980:IV, a date of interest because of the 1981-83 disinflation that began thereafter. Finally, the last two lines display the mean error and root-mean-squared-error (RMSE) when the equation estimated through 1980:IV is subjected to a dynamic simulation for the 13 quarters ending in 1984:I.

Columns 1a and 1b reproduce exactly Fair's estimates of his two-equation model 2 (his Table 1), except that here all changes are expressed as annual percentage rates, replacing his inconsistent mixture of quarterly, annual, and semi-annual rates. This explains why our coefficient on lagged wage change in the price equation (column b) is exactly four times the coefficient listed in his table. Column 2 shows the estimate of the reduced-form, equation 1 above. Notable here are the low and insignificant coefficient on the unemployment rate, and the sum of coefficients on nominal explanatory variables of 0.84, significantly below unity (the relevant standard error is 0.08.).

The purpose of the remaining columns of Table 1 is to examine the robustness of Fair's rejection of the long-run NRH. As we shall see, minor changes in the specification of equation 1 raise the sum of coefficients on lagged nominal variables to unity. Second, evidence is provided to support the more detailed specifications of my inflation equations, namely the inclusion of additional supply variables. The first step in column 3 is to make two specification changes. The constrained rectangular distribution on lagged unemployment in line 8 is replaced by an unconstrained distribution, resulting in a substantial increase in the sum of coefficients, albeit not to the 5 percent significance level. Also the nominal import price change in

line 11 is replaced by the ***relative*** import price change in line 12, on the grounds that dynamic simulations of equations that take as exogenous a nominal rate of change (as do Fair's Tables 3 and 4) mix up relative and absolute price changes. Fair's approach leads him to conclude in his Table 3 that a permanent change in nominal GNP growth would lead not only to a permanent change in unemployment, but also to a continuous upward or downward movement in the real price of imports, analogous to his conclusion, previously pointed out, that such a shift in monetary policy would cause the real wage to go to zero or infinity.

We note that the two minor changes in moving from column 2 to 3 have another effect, and this is to raise the sum of coefficients on lagged nominal variables from 0.84 to 0.94, now insignificantly below unity. Another minor change in column 4 raises the sum to 1.01, and this is the addition of a single variable consisting of a rectangular distribution on the 9th through 12th lag of the dependent variable. While the sum of coefficients on this new variable (line 6) is not significant, it becomes significant in the next two columns in conjunction with other variables. The purpose of the extended specification in columns 5 and 6 is to judge the contribution of additional variables that are entered in my inflation equations. The first of these (line 13) is the change in the relative price of food and energy, a proxy for the impact of supply shocks on domestic inflation. Next is the change in the effective foreign exchange rate of the dollar (line 14), excluded from column 5 but included in column 6. As we shall see, this special treatment of the exchange rate is justified by the extraordinary shift in the economy's response to exchange rate changes before and after 1980:IV. Next in line 15 is the change in the effective minimum wage and the deviation of productivity growth from trend. The latter variable serves as an index of how cyclical changes in productivity growth are distributed between price and profit changes. A coefficient of zero would indicate that profits absorb all such cyclical productivity movements, with no price response to actual (as opposed to trend) unit labor cost. A coefficient of minus unity would indicate that price changes depend entirely on actual rather than trend unit labor cost and that profits are completely insulated from cyclical productivity movements. (The estimated coefficient of about -0.2 is very close to those reported in Gordon [1982], and earlier papers.)

The results in columns 5 and 6 suggest several general comments. First, most of the extra variables are significant, and an F test on the explanatory contribution of the extra variables passes at well beyond the 1 percent significance level. Second, the additional variables maintain the sum of coefficients on lagged inflation at between 0.99 and 1.01, consistent with the

NRH. Third, the additional variables result in an increase in the absolute value of the unemployment coefficient and hence a steeper short-run Phillips curve. Fourth, the additional variables lead to a substantial lengthening of the lag distribution on past inflation, signified by the larger and more significant coefficients on line 6.

The difference between column 5 and 6 is the presence of the exchange rate in the latter. This additional variable exhibits several signs of instability. Note that column 6 fits better through 1980:IV, but not when extended to 1984:I. The Chow test at the bottom of column 6 rejects stability. Most notably, the post-sample dynamic simulation performance of column 6 is abysmal, while that in column 5 is the best for any equation in Table 1.

Overall, there is a tradeoff among three alternative variables to represent the effect on aggregate U.S. inflation of supply shocks in the 1970s—changes in relative import prices, in the relative price of food and energy, and in the effective exchange rate. Any two of the three seem able to explain the data adequately through 1980, but in the 1981-83 period the exchange rate predicts much more disinflation than actually occurred. Why this structural shift occurred poses a challenge to specialists in international macroeconomics.

Conclusion

There is insufficient space here to report numerous other intriguing issues that have been uncovered in the course of my empirical work on Fair's model. For instance, my previous evidence that Perry's weighted unemployment rate yielded more reliable estimates of the natural unemployment rate than the official unemployment rate seems to have evaporated in the 1981-83 period. Further, use of the nonfarm private deflator yields a considerably lower estimate of the natural rate of unemployment than the fixed-weight GNP deflator, posing a tricky problem for policymakers who would like to know at what unemployment rate inflation is likely to accelerate.

However, at a minimum, it is safe to conclude that there is no evidence whatsoever in Fair's data that conflicts with the Friedman-Phelps NRH, and that a detailed consideration of 'supply" variables and lag specifications may yield a modest payoff in our understanding of the U.S. inflation process.

TABLE 1

Alternative Specifications for Quarterly Rate of Change of Wages and Prices Sample Period: 1954:I-1984:I

Variable Symbol	Lags Incl.	Code for Lag Constraint	Dependent Variable						
			w	P	P	P	P	P	P
			(1a)	(1b)	(2)	(3)	(4)	(5)	(6)
1. Constant	0	—	5.58**	-1.04*	1.10	1.68*	2.21**	2.46**	2.34**
Inertia									
2. w	1-4	RD		0.58**					
3. p	1	—		0.29**	0.31*	0.38**	0.37**	0.14	0.13
4. $\dot{p}$	1-4	RD	0.70**						
5. $\dot{p}$	2-8	T			0.39**	0.56**	0.49**	0.49**	0.50**
6. $\dot{p}$	9-12	RD					0.14	0.36**	0.38**
Demand									
7. UR	0	—	-0.44**						
8. UR	1-4	RD			-0.09				
9. UR	0.4	U				-0.25	-0.39*	-0.44**	-0.43**
Supply									
10. (lid)	1-4	RD			0.25	0.56	0.47	0.83	0.72
11. $\dot{p}^{I}$	1-2	RD		0.12**	0.14**				
12. $\dot{p}^{I} - \dot{p}$	1-4	RD				0.14**	0.13**	0.09**	0.07
13. $\dot{p}^{EF} - \dot{p}$	0-4	RD						0.58*	0.59*
14. $\dot{x}$	0-3	RD							-0.05
15. EMW	0-4	RD						0.06*	0.06*
16. LPDEV	0	—						-0.19**	-0.18**
17. NIXON	0	—						-1.12	-1.57
18. NIXOFF	0	—						1.57	2.09*
Sum Nominal RHS Coeffs			0.70	0.99	0.84	0.94	1.01	0.99	1.01
S.E.E. to 1984:Q1			2.28	1.64	1.69	1.67	1.67	1.49	1.49
S.E.E. to 1980:Q4			2.20	1.64	1.71	1.69	1.68	1.49	1.40
Chow F, break 1980:Q4			1.54	1.06	0.84	0.84	0.79	1.00	2.01*
Dynamic Simulation									
Mean Error			1.72		1.02	2.43	1.62	0.97	3.76
RMSE			2.17		1.83	2.82	2.15	1.78	4.28

Notes to Table 1: Asterisks designate the 5 percent (*) or 1 percent (**) significance level of coefficients or sums of coefficients. A dot over a variable indicates that the variable is defined as a percentage change at an annual rate, calculated as the first difference of the log level multiplied by 400. "RD" indicates a rectangular distribution, that is, each of the coefficients for the lag lengths indicated is constrained to be the same, and the coefficient listed in the table is the sum of these identical coefficients. "T" indicates the sum of coefficients on a distribution constrained to follow

the "tent-shaped" distribution of the third term in equation (1)in the text. "U" indicates the sum of coefficients on an unconstrained lag distribution. The dynamic simulation errors reported in the bottom two lines use coefficients estimated for the period 1954:I-1980:IV and calculated predicted values for 1981:I-1984:I, taking all variables as exogenous but lagged wage and price changes, which are treated as endogenous and recalculated each quarter as the simulation proceeds. All variable symbols are as in Fair's paper, except for the following:

$p^{EF} - p$ is the percentage change in the fixed-weight deflator for personal consumption expenditures minus the percentage change in the fixed-weight deflator for personal consumption expenditures net expenditures on food and energy.

x is the IMF effective exchange rate of the dollar.

EMW is the effective minimum wage.

LPDEV is the deviation of nonfarm private productivity from trend.

NIXON and NIXOFF are dummy variables for the Nixon price control period, 1971:III-1972:III and 1974:II-1975:I.

Construction of each of these variables is identical to the description in the notes to Gordon (1982), Table 2.

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Rejoinder

Ray C. Fair

I find Gordon's reduced form approach very unsatisfying for reasons that are stated in my paper. One doesn't know whether the variables that Gordon adds to his equation belong in the structural price equation, in the structural wage equation, or in both, and so the results are hard to evaluate. Among other things, the structural approach allows one to examine the implied behavior of the real wage, and this is an important check on the individual price and wage equations. In Model 2 in my paper, the long-run behavior of the real wage with respect to changes in both the unemployment rate and the price of imports is suspect, and in Model 1 the long-run behavior with respect to changes in the price of imports is suspect. There is room for further work here. The reduced form approach does not, however, get around this problem. The problem is simply ignored.

There is always a danger of data-mining in macroeconometric work, i.e., running enough regressions to find the result that one wants when in fact the result is spurious. A model may fit the data well and give the desired result when it is in fact a poor approximation of the true structure. The method that I use to compare the different models accounts for this possible problem since it accounts for the possible misspecification of the models. Before one can have any confidence in Gordon's results, his model needs to be put through further tests.

Is the sum of the nominal RHS coefficients in Gordon's equation really one, or has Gordon in his diligence merely found a specification that gives a value of one? The main change that seems to give a value of one is the addition of the 9th through 12th lag of the dependent variable. This is equivalent in Model 2 to adding the price change lagged five quarters to the wage equation. The results discussed in my paper show that this change is not significant. There is no evidence in my work that price

changes lagged more than four quarters belong in the wage equation. The new lagged price variable is also not significant in Gordon's equation until Gordon's other variables are added to the equation (compare columns 4 and 5 in Gordon's Table 1). The important question is thus whether these other variables belong in the equation. My feeling is that until a more structural approach is taken and until Gordon's model is subject to misspecification tests, these results are not to be trusted.

Finally, Gordon makes no mention of Model 1 except to say that inside it "is a rate-of-change equation struggling to get out, since in both the price and wage equations the coefficient on the lagged dependent variable is greater than 0.9." The coefficient estimates are, however, significantly less than one by a large margin, and the equations are really not struggling in this way. From my tests, Model 1 seems to be the best of the three, and it should not be put out of the running in the never-ending search for the best model of price and wage behavior.

4
The Role of the Central Bank in Achieving Price Stability: An International Perspective

Helmut Schlesinger

For Europe, the United States is the dominant economy in international trade, today more so again than in the decade of relatively high inflation in the U.S. At the same time, American economists have gone to the front of international economic theory, a lead that we admire greatly, even if at times with some skepticism.

As a central bank practitioner from a European country, and given what I have just said, there is a particular challenge for me in dealing from an international perspective with the subject of this symposium in a luncheon address. It is not only the topic that is the challenge, it is the luncheon too, and I am sure my speech is only going to be able to spoil its high quality. What I am able to say on the topic of price stability is of necessity colored by the experience of central bank policy and practice in my own country and certainly cannot serve to attempt generally valid answers to questions that are the subject of academic controversy. What I want to do on this occasion might be seen as a modest attempt to describe for you in a type of shortened ***tourd'horizon*** what ideas have been taking shape in my country's central bank on some of this conference's main topics.

This morning, great academic seriousness has been used to treat the questions of the causes of inflation and the costlbenefit analysis of price stabilization policy. In recent times, economic theory has again been concerning itself more strongly with these questions. A short time ago, they were dealt with in a seminar of European professors at the Bundesbank, too. These subjects constitute a continuous challenge for every central bank practitioner. Many inflation theorists tend to reproach monetary policymakers for pursuing ambitious stability policy goals "at any price." I do not want to deny the fact that the Deutsche Bundesbank is to be counted among those central banks that have always given the economic policy

goal of price stability priority in their considerations. In fact, right from the beginning, the Bundesbank understood its role to include defending the goal of price stability against all comers and actively popularizing this goal by the spoken word and in print.

The Bundesbank has a style of argument in public that, inter alia, would agree with the comment of one of its own presidents: 'In the long run, an economy cannot have 'just a little inflation: for if you start flirting with inflation, it will end up marrying you'' (Emminger). So we in the Bundesbank never seriously entertained the previously popular idea that a certain amount of creeping inflation was helpful or necessary for steady economic growth. In the same way, the Bundesbank has always been publicly against the idea of making life under inflation easier, as indexation advocates say it would be, by indexing taxes, interest rates, and wages and goods contracts. In doing this, the Bundesbank never overlooked the fact that in the course of the '70s, inflation was fed by internal and external disturbances that did not fit traditional notions of pure demand-pull inflation.

Foreign observers—and among them not least economic theorists—have probably been asking themselves from time to time how Bundesbank spokesmen can justify their confidence when spreading the gospel of price stability both at home and abroad. One frequently quoted opinion amounts to saying that the cause can be found in the traumatic consequences of two war- and government-induced inflations in Germany, or even in an inborn German tendency to conservatism and dogmatism. Now, nobody would deny that the German historical experience of inflation has been influential in determining the attitudes of our population and politicians since World War II. This fact is of no minor importance in explaining why the Deutsche Bundesbank, in the statute determining its position and activities, was required to pursue the primary goal of price stability. In addition, the Bundesbank's authority with respect to monetary policy was made independent of the government and Parliament.

However, it would be going too far to link recent attitudes at the Bundesbank only to this negative experience of inflation. The other side of the coin should really be mentioned here as well, namely that in our country, economic revival has twice been associated with the creation of new and healthy money—in 1924 and in the middle of 1948. You may object that this is all a long time ago, that today's problems have more nuances. And in fact, you would be right. But the last 10 to 15 years have seen a confirmation of the fact that practically all countries that have tried to get along with a little bit of inflation, or indexation, have run their stability policies onto the rocks.

This does not refute at a theoretical level the academic usefulness of cost/benefit thinking in inflation theory, or of contract-based indexation concepts. However, it seems to me that the global experience of inflation since the start of the '70s can be equated with a field trial in society at large, from which monetary policy can learn at least a few provisional lessons.

This experiment has shown that it is difficult indeed to steady inflationary processes once they have gotten under way, that it is not possible to adjust the economic policy autopilot to an annual rate of 5 or 10 percent, and then hope that the autopilot will stick to it. Firstly, this can be traced back to the disappearance of the illusion of money and the dynamics of inflationary expectations. The theories of adaptive and rational expectations have proceeded from this state of affairs. At the same time an inflationary climate—at least in Europe—appears to favor militant distribution conflicts. The competing claims that crop up here of the different groups in society can easily develop a home-made inflationary spiral. A spiral of this sort can easily start its ascent under an accommodating monetary policy, and for certain weak currencies, it can in certain circumstances be aggravated by the vicious-circle phenomenon induced by depreciation.

High inflation rates that fluctuate and are therefore hard to predict hamper the signalling and allocation function of relative prices and thus also the growth process, and these facts are pretty well universally accepted today. At the same time, experience in high-inflation countries suggests that their populations perceive an uncontrolled ongoing inflation as a deterioration in their quality of life, especially as to date there has been almost no success anywhere in indexing taxes, social transfers, and interest and wage income on a distributionally neutral basis. On the contrary: inflation always brings about a change in income distribution, and in the final analysis in property distribution too, and usually this is negative rather than positive for the economy. This experience of frustration has had global consequences.

Until the beginning of the '70s, the stability policies of a minority of major central banks, for example those of the Federal Republic of Germany, Switzerland, and at times Japan, too, tended to be classified as dogmatic. But after the outbreak of the second oil crisis (1979-80) at the latest, these policies were largely being imitated at the international level, as the most important central banks jointly tried to get a fresh grip on inflation, which was accelerating worldwide. Some countries, e.g. the United Kingdom and the United States, applied a particularly strong grip to throttle

the steep rise in their inflation rates down to a more tolerable level, and, without doubt, doing this meant at times taking on high-level risks with respect to employment. In general, the impression was created that countries of this type, which appeared to have lost control over inflation, had to apply monetary cold-turkey methods in the end so as to influence price expectations into dropping in a sustained fashion. In Germany, we have actually only taken this course once—after the outbreak of the first oil crisis (1974-75) after German monetary policy had temporarily lost credibility—due to ongoing and massive inflows of funds from abroad. Later on, Bundesbank policy did indeed display certain gradualistic traits, after trust in the Bundesbank's perseveranceseemed to have been restored. In the still unsettled academic controversy as to whether it is rigorous or step-wise methods of inflation-fighting that promise more success, the Bundesbank has thus adopted a pragmatic attitude: It relates what it does to the initial conditions and the climate of expectations in which its monetary policy has to be applied. I have great difficulty in seeing how any other course could possibly be taken.

This attitude also has to do with the fact that we are aware our influence has its limits and that we must not forget where they lie. Naturally, we acknowledge the widely accepted academic view that in the longer term, inflation must always be understood as a monetary phenomenon. To this extent, no central bank can evade ultimate responsibility for keeping a check on price trends. In spite of the opinion that Germany was and is more or less an island of stability, our own experience is not the least of teachers in pointing out to us the limits that obtain for monetary stabilization policy in the shorter term. This observation of mine may surprise some of you, who as professional or academic Bundesbank watchers keep track of our statements and measures from time to time. So without wanting to anticipate later conference topics, let me say a few words about the way in which we are trying to do justice to the hindrances and limits to monetary policy that we recognize when pursuing the practical implementation of our price stability goals. Those foreign observers who have been occupying themselves in some detail since the middle of the '70s with the Bundesbank's monetary policy have sometimes noted with astonishment that the Bundesbank has seemed relatively generous when setting its annual monetary growth targets. Year for year, the Bundesbank derives its monetary growth target from two basic components: assumed growth in production potential and so-called 'unavoidable" price rise. Thus, we have always cut the monetary cloak with sufficient generosity for it to allow enough room for appropriate economic growth. But on the other hand, we

have also assumed an unavoidable price rise. This has always been 0.5 to 1 percentage point below the relevant inflation forecasts, which have never been very high. And we have also expressed the belief that there can be no go and then stop in price movements, but only a gradual reduction of inflation. In this way, price goals have been formulated over the years that were usually noticeably above an inflation rate of 1 to 2 percent, a price rise that today we would characterize as a satisfactory approach to price stability.

This policy has fairly often given rise to reproaches: For example, that the Bundesbank likes talking about stability but isn't aiming for it. In actual fact, the only realistic assumption is that price rises that already appear to have worked through into business planning cannot be stopped at once. At the same time, our policy has given us a chance of reacting moderately to external price shocks—such as oil price increases and import price jumps determined by exchange rates. In doing this we have largely been concerned to avoid transplanting exogenous price disturbances via domestic adjustment inflation, without avoiding at any cost the direct effects of terms-of-trade deterioration on the domestic price level. A strategy of this type naturally presupposes a certain public trust in the course of central bank policy, and I have to admit here that prices and wages have by no means always reacted in an ideal fashion to the monetary framework set by the Bundesbank. *So* I am sure that in some cases we were tending to be too soft rather than too hard. We did indeed consider the probable costs of fighting inflation from a short-term point of view—that is, the short-term effects on production and employment. A further peculiarity of our stability policy strategy is that we do not formulate our price stabilization goals behind closed doors. Although the Bundesbank has statutory protection against direct attempts at political interference, it tries very consciously to obtain a certain degree of advance backup from other economic policy authorities and prominent groups in the community. This way of proceeding rests on the conviction that a monetary policy geared to stabilizing the price level can get satisfactory results in the long term only if the central bank's efforts are clearly recognized by all economic actors and receive their support. For some of those involved in economic activity, a monetary growth target may, however, appear to be a relatively abstract quantity that has no direct effect on the way they behave. Others could at least pretend that they do not properly understand the Bundesbank's intentions. But we also associate with our monetary growth target an unmistakable appeal to enterprises and unions to play their part, by behaving moderately in the process of price and wage formation, in allowing the real scope for growth in our economy to be exploited as far as possible without

endangering the postulate of stability. What is just as important in our policy is the constant attempt to base monetary, fiscal, and general economic policy on compatible benchmarks of stability policy. In this field, a well-established ritual has developed in Germany, and it serves to coordinate the various policy areas in a manner that the German public can recognize. In joint consultation with the federal government, the Bundesbank ensures that the price and growth conceptions of the Bundesbank and the federal government do not conflict. To date, this has always led to the government agreeing to the monetary growth targets announced by the Bundesbank.

Now, what have been the results of our stability policy? In view of the multitude of external disturbances and internal inflationary stumbling blocks that have hindered our policy since the beginning of the '70s, we are by now more or less content with the longer-term price performance of the German economy. Since the middle of the '70s, the inflation rate, measured by annual changes in the GNP deflator, has been moving without overly strong fluctuations on an underlying trend of between 3.5 and 4.5 percent, and of late has been tending towards 2.5 percent and lower. At the same time, however, even if for other reasons, real economic growth has weakened in the medium term. Moreover, we are now having to live with a level of more than 2 million unemployed, which can probably be cut back only step by step.

Nevertheless, I would not be prepared to see, in this unfavorable development on the real side of the economy, economic inflation-fighting costs that would be to the discredit of the Bundesbank's monetary policy. There are of course many deep-rooted factors that have caused a sustained weakening of economic growth from the supply side in many European countries—including the Federal Republic of Germany.[1] So we have to resist the temptation to oppose underemployment with easy and cheap money and higher government deficits. In this respect, the business situation we are faced with in Europe does not seem to be as favorable as it is in your case in the United States.

On our side of the Atlantic, we will have to pursue medium-term price stabilization goals against the background of a certain Euro-pessimism, but this pessimism should not be exaggerated. At least in the field of fighting inflation, there is a 'two-speed Europe: to use Mitterand's expression in another context. All around Germany there are countries

1. See for instance: Bela Balassa, *The Economic Consequences of Social Policies in the Industrial Countries*, Bernhard Harms Lecture, Kiel, June 23, 1984, Weltwirtschaftliches Archiv, Vol. CXX, p. 213-27.

with similar levels of price stability: Switzerland, Austria, and the Netherlands. Moreover, the United Kingdom, Denmark, and Belgium are getting noticeably closer to this trend. Efforts being made in France are also considerable, even if not so successful. The monetary policy position in this respect is a varied one; for example, monetary policy is applied with more vehemence in the United Kingdom than in some of the other countries I have mentioned. The other speed is that of the Mediterranean countries; hopes of being able to make progress here are not so high.

In the United States, monetary policy is confronted with the all-too-familiar problem of enabling a soft landing for a vital and rapidly expanding economy after a strong upswing. The fact that monetary and fiscal policymakers do not at all times appear to be seeing eye-to-eye in this regard will not facilitate solutions to your stabilization problems in America.

But I did not come here to sing you some well-known tunes from a European lament. I hope, on the contrary, that on the basis of the joint efforts of American economic theorists and those responsible in office for public policy, the American economy will soon have shot the rapids and arrived at steady growth and relatively stable prices. I think this is not impossible. If we look out of the window here at the Grand Tetons, we might think them impossible to climb, and for most of us this is true. But let me tell you a story that one of my old mountaineering friends told me: A mountain looks invincible only from a distance. When you get close to it you can see tracks up it and even foot and hand holds that you didn't expect before. In other words, there is absolutely no reason to lose courage. I am sure that the intellectual effort put into this symposium will lend its strength to this happening. So in this sense I wish the organizers continued and complete success and would like to thank you all very much for listening so patiently to a central bank practitioner from the Old World.

Credibility and Monetary Policy

Bennett T. McCallum

Introduction

According to my dictionaries, "credibility" is the property of being credible, with the latter meaning roughly the same as believable. So with this definition, a policy lacks credibility if it is one that could not reasonably be believed. It would appear that William Fellner (1976, 1979), who introduced the idea into the macroeconomic arena, chose this particular word because he believed that the U.S. aggregate demand policy of the middle to late 1970s was unsustainable and in that sense unbelievable. With the passage of time, the term has come to be used in a slightly different way, in particular, as meaning "believed" rather than "believable." In what follows, the term will be used in this latter fashion: Credibility obtains to the extent that beliefs concerning policy conform to the way in which policy is actually being conducted and to official announcements about its conduct.

It should be emphasized that this meaning is conceptually quite distinct from that pertaining to a situation in which it is expected that future rates of inflation will be small. As it happens, interest in the notion was from the start stimulated by Fellner's argument that a credible (believed) disinflation would be less costly, in terms of foregone output, than one that the public expected to be aborted. Because of this interest in disinflation, much of the discussion has been conducted under the presumption that prevailing policy is of a type that will lead to a low inflation rate in the future, and that in

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turn implies an agreement between correct beliefs and low inflationary expectations. But these concepts are obviously quite different, and to define the term in the latter way would be to abuse language as well as to create unnecessary possibilities of confusion.

A second distinction concerns phrases such as "credibility of monetary policy? Here it is important to distinguish between policy as an ongoing process—a way of making decisions and taking actions—and the resulting period-by-period actions (instrument settings) themselves. Thus, the credibility of a policy is to be distinguished from the credibility of the announcements pertaining to a particular period. While the latter is certainly a concept of some interest, economists' efforts are—for reasons explained by Lucas (1980)—usually more fruitful when focused on the analysis of policies, as opposed to specific policy actions.

The objective of the present paper is to describe and consider the most important existing ideas concerning credibility of monetary policy. Special emphasis will be given to matters pertaining to the U.S. economy and the practices and procedures of the Fed. The main discussion begins in the next section with a review of Fellner's hypothesis that the costs of a disinflationary episode will be smaller when the public believes that the disinflation will in fact be carried out. This hypothesis has been challenged recently by B. Friedman (1983), Gordon (1983), Perry (1983), and others; an evaluation of their arguments is attempted and some new results presented. In the following section, by contrast, the discussion centers on positive analyses of the monetary policymaking process. Models developed by Barro and Gordon (1983a, 1983b), Canzoneri (1983), and Cukierman and Meltzer (1984) are examined, the object being to develop an understanding of why certain features of monetary policy tend to prevail. The basic ideas of the analysis are then applied in the final section, which is concerned with various strategies for obtaining a type of policy behavior that might produce better macroeconomic results—less inflation with no more unemployment—than the U.S. has experienced in the recent past. Particular proposals touched upon include the adoption of a commodity-money standard, a balanced-budget amendment, a legislated monetary rule, a nominal GNP target, and the absorption of the Fed into the Treasury. Some conclusions are then suggested.

The importance of credibility

The basic idea of the credibility hypothesis—that the foregone-output costs of a disinflationary episode will be smaller if the public correctly believes that the attempt will not be abandoned—is familiar enough to

require only a brief sketch. If, for example, the economy's aggregate supply function (or Phillips relationship) is of the form[1]

$$(1) \quad y_t - \bar{y}_t = \alpha_1(\Delta p_t - E_{t-1}\Delta p_t) + \lambda(y_{t-1} - \bar{y}_{t-1}) + u_t,$$

with u_t a purely random disturbance, then the inflation rate, Δp_t, can be lowered without any deleterious effect on output relative to capacity $y_t - \bar{y}_t$ provided that the reduction in Δp_t is correctly anticipated by at least one period, while a cumulative output reduction of $\alpha_1/(1-\lambda)$ will occur per unit decrease in Δp_t if the latter is not anticipated. More generally, if instead of (1) the supply function is of the nominal-contract type utilized by Fischer (1977),

$$(2) \quad y_t - \bar{y}_t = \alpha_1(\Delta p_t - E_{t-1}\Delta p_t) + \alpha_2(\Delta p_t - E_{t-2}\Delta p_t) + \alpha_2(E_{t-1}\Delta p_{t-1} - E_{t-2}\Delta p_{t-1}) + \lambda(y_{t-1} - \bar{y}_{t-1}) + u_t,$$

then each Δp_t reduction must be anticipated two periods in advance to avoid all output costs, with an extension to J-period lags straightforward. These costs will, nevertheless, be smaller the smaller is the excess of expected over actual inflation rates during the episode.[2] The rather different contracts of the type employed in Taylor's models (1980, 1983a) also give rise to such effects. An interesting recent analysis using a more general framework appears in Fischer (1984).

Two or three years ago, the relevance of this credibility hypothesis for the U.S. economy was, I believe, very widely accepted by economists doing macroeconomic research.[3] More recently, however, it has been called into question on the basis of U.S. data referring to the recent (1982-83)

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1. Here $\alpha_1 > 0$ and $0 \leq \lambda < 1$ while y_t and $\bar{y}_t$ refer to logarithms of actual and "capacity" or "natural rate" values of aggregate output for period t and p, is the log of the aggregate price level. In equation (1), $E_{t-1}\Delta p_t$ merely denotes the subjective expectation of Ap, held at the end of period t-1. At various points, however, we will interpret $E_{t-1}(.)$ as the conditional mathematical expectation $E(. \mid \Omega_{t-1})$ where Ω_{t-1} is an information set including realizations of all relevant variables in periods $t-1, t-2, \ldots$. In other words, we shall in that case be assuming rational expectations. That hypothesis is neither necessary nor sufficient for the credibility hypothesis, although there are strong relationships and many pmponents of the credibility hypothesis do in fact come to the latter by way of rational expectations.

2. This statement is phrased so as to avoid taking a position on the issue of whether costs are incurred whenever $y_t \neq \bar{y}_t$, or only when $y_t < \bar{y}_t$. Thus this paper continues in the common tradition of bypassing this fundamental and important issue.

3. Note that the 'credibility hypothesis" does not imply only that policy credibility (as defined above) obtains, but also that the economy's Phillips curve is of the expectational variety. This terminology is taken fmm Fellner.

recession and slowing of inflation. In particular, Friedman (1983, 1984), Gordon (1983, 1984), and Perry (1983) have each suggested that the credibility hypothesis and its close intellectual kin, the "Lucas critique: are factually incorrect. More specifically, they have argued that conventional (i.e., non-rational expectation) Phillips curve relationships based on pre-1980 data are consistent with the disinflationary episode, and that this would not be true if the credibility hypothesis had empirical relevance.[4]

The most extreme of the positions taken in these papers is expressed by Friedman (1983, p. 14), who indicates that the unemployment-inflation figures 'are strikingly in line with the conventional estimates of the cost of disinflation surveyed by Okun." This reference, of course, is to Arthur Okun's famous summary of six econometric Phillips curves, which indicated that 'the cost of a 1-point reduction in the basic inflation rate is 10 percent of a year's GNP, with a range [across models] of 6 to 18 percent" (Okun, 1978, p. 348). In making his calculation, Friedman presumes that the episode lowered the inflation rate by 5 percentage points and estimates that the incremental unemployment during 1980-82 was about 5 point-years. These figures would imply a sacrifice ratio[5] of only about 2.5 to 3.0 (depending on the 'Okun's Law" figure used to convert unemployment into output loss), well below Okun's lower limit of 6. But Friedman also counts unemployment predicted for the years 1983-88, which totals three times as much as that for 1980-82, giving him a final value of 10-12 ('toward the pessimistic end of Okun's range") for the episode's sacrifice ratio. In a more recent look at the episode, furthermore, Friedman (1984) was able to use actual data for most of 1983. This brought the sacrifice ratio up to the 5-6 range without reliance on predictions of future unemployment.

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4. An entirely different argument calling into question the hypothesis was developed by Grossman (1983). This argument concerns equation (1) together with rational expectations (i.e., with $E_{t-1}\Delta p_t = E[\Delta p_t \mid \Omega_{t-1}]$) a specification that has often been interpreted as applying to an economy with full price flexibility. Under that interpretation, as Grossman notes, the true structural supply function (as developed in Lucas [1973]) relates to contemporaneous *perception* errors rather than anticipational errors; equation (1) is just an aggregated reduced-form expression that is appropriate in some cases. Consequently, if individuals possess useful information on contemporaneous nominal aggregates (money stock or price index values), as would seem to be the case in actuality, then previously formed expectations of Ap, are irrelevant for output determination. Credibility then becomes unimportant for price and output developments; all that matters is the path actually taken by the money stock and price level. So, Grossman in effect suggests, credibility arguments are important only for economies in which there is some stickiness in price adjustments. McCallum (1982) uses a related argument to suggest that price stickiness is in fact a feature of the U.S. economy.

5. The sacrifice ratio is the percent of a year's output lost divided by the number of percentage points (on an annual basis) that the inflation rate falls. The term was used by Gordon and King (1982).

Basically the same raw data have also been examined by Fischer (1984). As a result of a slightly different assumption regarding the natural unemployment rate (6.5 percent vs. Friedman's 6.0), and a different set of predictions about unemployment in 1984-1988, Fischer finds 'a sacrifice ratio around 5 to 6, at the lower end of the range quoted by Okun" (1984, p. 27). If, moreover, the inflation drop is viewed as 6 percent, rather than 5 percent, then the implied sacrifice ratio is below the Okun range.

Perry's (1983) study of the recent episode is based in part on a comparison of actual nominal wage changes during 1980:I-1983:II with values predicted by an equation estimated on data from the period 1954:I-1980:I. Evidence in favor of the credibility effect would consist of negative prediction errors, i.e., actual minus predicted values of the rate of wage change. In his evaluation of the results, Perry emphasizes that such errors do not show up in 1980 or the first three quarters of 1981, and that those resulting for 1981:IV-1983:II are not large compared to their standard errors. It is the case, nevertheless, that the prediction errors are negative for each of the last seven quarters that he examined, 1981:IV-1983:II. This finding, which is duplicated for the DRI model's wage equation in a study by Blanchard (1984), is qualitatively consistent with the credibility hypothesis.

Perry (1983) also reports that price-change prediction errors are predominately positive, rather than negative, for two of the three versions of the Gordon-King (1982) inflation equation. Thus, from this equation, 'there is no evidence supporting the credibility hypothesis in connection with the present policy of disinflation" (Perry, 1983, pp. 598-99). A similar finding is reported by Clarida and Friedman (1983).[6]

What should we make of all of this? Certainly there is not a great deal of evidence in the quarterly data for 1980-83 that would serve to change the mind of someone dubious about the credibility hypothesis or, for that matter, the Phillips-curve applicability of the Lucas critique. But likewise the record is not such that a true believer—even a relatively open-minded one—would be strongly inclined to alter his position. A leading reason is noted by Perry (1983, p. 600): 'No measure exists of what private decision-makers thought about policy aims in-this period. Because of this, one could argue that the promised benefits of credible disinflationary policy have not been realized because the credibility of anti-inflation policy has never been established." My own opinion gives a great deal of weight to this argument. Some reasons for doubting the Fed's resolve to eradicate

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6. Other studies of the episode have been conducted by Cagan and Fellner (1983) and Englander and Los (1983).

inflation can be inferred by consideration of Herb Stein's (1980) list of six features that a disinflationary policy should possess in order to be credible. These include:

1. A combination of various policy measures.
2. Cooperation between the president and the Fed.
3. A high degree of bipartisan support.
4. Quantitative specification of intermediate-term goals and measures, so that deviation from the program will be immediately visible.
5. Rejection of any commitment about the unemployment rate.
6. Rejection of substitute measures such as 'incomes policy.'

Among these features, only No. 6 in fact obtained during the episode in question. Of particular importance, in my opinion, was the absence of feature 4, about which more will be said below.

Indeed, from an ex post perspective, as of June 1984, it is not at all clear that the episode of 1980-82 did in fact involve a change to a new, non-inflationary policy regime. Neither private nor governmental forecasts are now predicting a continued lowering of the inflation rate from its 1983 level, and some vector autoregression models are predicting sharp increases within a few months. More fundamental is the evidence concerning policy provided by the behavior of the monetary base. In particular, the growth rate of the (St. Louis) base has averaged 8.4 percent per annum since the third quarter of 1979, as compared with 2.8 percent for the 22-year period 1947:IV-1969:IV.

Examination of one particular episode is, of course, not the preferred method of testing hypotheses. Standard econometric techniques utilize data from longer sample periods and so are less susceptible to distortion by one or two random disturbances. Thus, a preferable approach to the issue at hand would be a more general consideration of the empirical significance of the credibility hypothesis/Lucas critique. In this respect it is notable that Gordon (1984, p. 42) has contended that 'the U.S. Phillips curve appears to be one of the most stable empirical macroeconomic relationships of the postwar era, one that shows no sign as yet of being subject to Lucas's econometric critique.' This conclusion is based in part on the study by Gordon and King (1982, pp. 224-29), who find only minor evidence of parameter changes between subsamples divided at the end of 1966.

Since whether one finds evidence of relationship changes will depend on the way in which he looks, I will report the results of a brief investigation of my own. One consideration of importance is that evidence of a

parameter change will assert itself more clearly when the alternative hypothesis—alternative, that is, to a null hypothesis representing no parameter change—is not excessively general. From the analysis of Sargent (1971), one would expect that the place to look for changes in a Phillips relationship is in the value of the coefficients attached to past inflation rates. Accordingly, I have looked for changes in the values of $b_1, \ldots, b_N$ in equations of the form

$$(3) \quad \Delta p_t = a_0 + a_1 x_t + a_2 x_{t-1} + b_1 \Delta p_{t-1} + \ldots + b_N \Delta p_{t-N} + \epsilon_t,$$

where x, denotes the U.S. unemployment rate for males over 20 years of age and with p_t measured as the log of the PCE deflator. I have sought to determine whether the b_j values changed between the noninflationary 1950s and the inflationary 1970s by expressing each of these coefficients as $b_j = b_{j0} + b_{j1} d_t$, where d_t is a dummy variable equaling zero in the earlier period and 1.0 in the later period. I have followed Gordon and King (1982) in using the end of 1966 as the breakpoint for d_t. My overall sample period is 1954:I-1982:IV; seasonally adjusted quarterly observations are used.

Since the quarterly inflation rate was, over the early part of the sample, fairly well-represented as a first-order autoregression (see Nelson [1972]), let us first consider OLS estimates with N = 1. With the dummy excluded we obtain

$$(4) \quad \Delta p_t = \underset{(2.5)}{.0026} - \underset{(1.6)}{.0012 x_t} + \underset{(1.2)}{.0009 x_{t-1}} + \underset{(17.8)}{.875 \Delta p_{t-1}}$$

$$R^2 = 0.739 \qquad SSE = 0.00155 \qquad DW = 2.51$$

where SSE is the sum of squared residuals, and the figures in parentheses are absolute values of t ratios. Including the dummy—allowing the coefficient on Δp_{t-1} to be different after 1966—gives rise to the following:

$$(5) \quad \Delta p_t = \underset{(5.0)}{.0053} - \underset{(1.1)}{.0007 x_t} + \underset{(0.5)}{.0003 x_{t-1}} + \underset{(1.8)}{.230 \Delta p_{t-1}} + \underset{(5.3)}{.566 d_t \Delta p_{t-1}}$$

$$R^2 = 0.791 \qquad SSE = 0.00124 \qquad DW = 2.17$$

Obviously the dummy variable is highly significant; indeed, it carries most of the explanatory power. Other aspects of the results are not, however, satisfactory—e.g., the unemployment variables have little explanatory power. Consequently, it appears that a larger value of N is needed, that more lagged values of Δp_t are required to reflect the effect of past inflation.

Estimates with N = 4 indicate that the fourth lag is not important, so results will be reported for N = 3. With the inclusion of the post-1966 dummies, these estimates are

$$(6)\quad \Delta p_t = \underset{(4.2)}{.0046} - \underset{(2.5)}{.0017}x_t + \underset{(1.6)}{.0010}x_{t-1} + \underset{(1.2)}{.158}\Delta p_{t-1} + \underset{(1.8)}{.241}\Delta p_{t-2}$$

$$+ \underset{(1.3)}{.183}\Delta p_{t-3} + \underset{(3.1)}{.517}d_t\Delta p_{t-1} - \underset{(2.1)}{.399}d_t\Delta p_{t-2} + \underset{(1.2)}{.213}d_t\Delta p_{t-3}$$

$$R^2 = 0.819 \qquad SSE = 0.001078 \qquad DW = 1.95$$

Without the dummy terms, the SSE value is 0.001232. Consequently, the relevant chi-square test statistic has the value [(0.001232/0.001078)-1][120-9] = 15.9. As there are three constraints under the null hypothesis, the critical value for a test with significance level 0.01 is 11.3. The null hypothesis of no parameter change is therefore easily rejected.

Furthermore, it will be noted that the sum of the b_j values is 0.913 for the post-1966 subperiod and only 0.582 for the earlier subperiod. These numbers would suggest very different pictures concerning the extent of an inflation-unemployment tradeoff across inflationary steady states to someone who (incorrectly, in my opinion) believed that the estimates could be interpreted in this fashion.

While the foregoing investigation is certainly not a definitive study, its results illustrate that the Gordon-King finding is sensitive to the testing strategy employed. To conclude that Phillips relationships are not susceptible to the Lucas critique, and thus that the credibility hypothesis is invalid, seems premature at best. Let us continue this discussion, then, under the presumption that expectational effects are important in relationships describing output/inflation tradeoffs.[7]

Reasons for credibility problems

Our next topic concerns reasons why credibility tends to be low. At this point the intention is to discuss the issue at a general and slightly abstract

7. At the Jackson Hole conference, Robert Gordon reported some test statistics indicating that the difference between my results and those of Gordon and King arises primarily because my specification (3) does not include a number of additional explanatory variables that do appear in the Gordon-King study (1982, p. 218). One's conclusions concerning the relative merits of the tests must then rest, to a considerable extent, on his judgment as to the theoretical appropriateness of the inclusion of these additional variables.

level, turning in the next section to specifics concerning the United States. The discussion will be somewhat specific even here, however, in that its emphasis will be on the tendency of inflation rates—and agents' expectations of them—to exceed values planned and announced by the monetary authority.

Among studies designed to explain policy behavior of the monetary authority, the most prominent analysis relating to the subject at hand is that presented by Barro and Gordon (1983a, 1983b), who built upon insights originally developed by Kydland and Prescott (1977). In the simplest version of the Barro-Gordon (1983b) model, the monetary authority's objective function is increasing in the current inflation or monetary surprise, but decreasing in the square of the inflation or money-growth rate itself, with discounted values of similar terms for all future periods.[8] If this authority were to adopt a policy rule that chose among constant inflation rates? he would recognize that on average, surprise values would be zero so that the optimal choice would be for a zero inflation rate. For the same sort of reason, an average inflation rate of zero would be implied by the optimal rule choice when a broader class of rules is permitted.

Suppose, however, that there exists no mechanism for institutionalizing a policy rule, so that the authority proceeds in a discretionary manner, selecting current inflation rates on a period-by-period basis. In each period, then, he will take the prevailing expected inflation rate as a given piece of data (an initial condition). The current surprise value then appears to be under his control, so the optimum choice of the current inflation rate seems to be that which just balances the marginal benefit of surprise inflation against the marginal cost of inflation per se. With an objective function of the type described, this optimal value will be strictly positive, with a magnitude that is greater the lower is the cost assigned to inflation.

Rational individuals understand this process, however, so the public's expectations about actual inflation are correct on average. Thus the

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8. Other versions of the model exist. The square of actual inflation relative to some constant target rate appears in one, while Barro and Gordon (1983a) use the square of $y_t - k\bar{y}_t$ (with $k > 1$) as a penalty term rather than making the objective increasing in $y_t - \bar{y}_t$ (or the surprise term).

9. There is no need, in the Barro-Gordon setup, to distinguish between inflation and money-growth rates. Consequently, we shall for simplicity write as if the authority were directly selecting inflation values.

surprise magnitude is zero on average, over any large number of periods, even though the monetary authority views it as controllable in each period. Consequently, there is on average no benefit actually materializing to compensate for the cost of a positive inflation rate. The discretionary outcome, it is clear, features more inflation but the same amount of surprise inflation (on average) as under a rule. According to this model, then, a discretionary mode of policy behavior by the monetary authority leads to consequences that are unambiguously poorer than would obtain (for the same economy and same objectives) under rule-like behavior.

Credibility enters the picture when Barro and Gordon enrich the menu of considerations to reflect the possibility of reputational strategies. In a reputational equilibrium, the monetary authority delivers a preannounced inflation rate in each period even though this rate is below the value that would obtain under discretionary behavior, the reason being that any departure from the preannounced value would induce private agents to disbelieve announcements concerning the future and expect more inflation than promised. Under the Barro-Gordon assumptions regarding the policymaker's objectives, this rate lies between zero and the discretionary value discussed above. Thus, in each period, the monetary authority partially bypasses the apparent possibility of exploiting given expectations because of his recognition that such exploitation would lead to a loss of credibility (reputation) that would imply a more unfavorable tradeoff in the future. Taking account of reputational effects, Barro and Gordon then obtain an equilibrium solution that is a weighted average of those that would obtain under discretion and under the optimum institutionalized rule. A concern for credibility is helpful, but is not a fully adequate substitute for the possibility of an institutionalized rule.

The Barro-Gordon line of analysis accurately reflects, in my opinion, several crucial aspects of the situation that actually obtains in the U.S. economy. In particular, its emphasis on the tension created by the desirability of money growth surprises together with the undesirability of anticipated money growth, seems central to the policy problem. It provides, moreover, an explanation of why our economy experiences significantly positive inflation on average even though policymakers (as well as economists) profess to believe that no benefits are thereby induced.

Taylor (1983b) has expressed reservations about this aspect of the Barro-Gordon analysis. His argument is that, in other contexts involving similar tensions, 'society seems to have found ways to institute the optimal (cooperative) policy. For example, patent laws are not repealed each year to prevent holders of patents from creating monopolist inefficiencies [that]

would eliminate any incentive for future inventions" (1983b, p. 125). That argument seems unconvincing, however, for the Barro-Gordon analysis is designed for an economy with fiat money, and our system has only recently completed its dissociation from a commodity-money (gold) standard. Thus it should be possible, if Taylor were right, to point to the recent creation of some institutional arrangement comparable to our patent system. Indeed, the need for something of this type would seem to be the main message of the Barro-Gordon analysis. This argument of Taylor's seems to imply, moreover, that our system has been generating the optimal amount of inflation—which he disputes elsewhere (1985).

My own reservations would be just the opposite of Taylor's. Specifically, I would think that the actual situation in the U.S. would be better represented by the purely discretionary equilibrium, in the Barro-Gordon model, than by the reputational equilibrium. Establishment of the latter apparently requires specification by the policymaker of a (noninstitutionalized) rule governing preannouncements that is enforced by the cost of departing from its instructions. (See Barro and Gordon [1983b], p. 108.) But there is no existing counterpart of this rule in the U.S. system. Indeed, spokesmen for the Federal Reserve have been adamant in their rejection of any prespecified pattern of policy behavior and in their assertions concerning the desirability (or even necessity) of policymaking *flexibility*.[10] In addition, I am bothered by the assumption about expectations utilized by Barro and Gordon (1983b, p. 108).

More recently, Cukierman and Meltzer (1984) have enriched the aforementioned line of analysis by incorporating three complications not present in the basic Barro-Gordon framework: imperfect control of, and unreliable announcements about, money growth rates, plus stochastically changing objectives of the policymaker. The fluctuations in objectives, moreover, are not promptly recognized by the public. These extra ingredients permit Cukierman and Meltzer to derive a large number of interesting conclusions concerning monetary behavior; two examples are that the monetary authority will choose to have relatively looser control procedures the higher is his rate of time preference, and that looser control leads to higher average rates of money growth (and inflation). Despite the ingenuity of these enrichments, however, the basic source of an excessive

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10. See, for example, the statements in Volcker (1982, 1984). Also see the discussion of the Fed's attitude by Hetzel (1984a) and Lombra and Moran (1980).

average inflation rate continues to be the exercise of period-by-period discretion, rather than the one-time choice of a rule."

A point emphasized in the Barro-Gordon discussions is the compatibility of fixed rules with policy activism, i.e., responses to the current state of the economy. The distinction between rules and discretion is quite different from the distinction between activist (i.e., contingent) and non-activist (e.g., constant growth rate) rules. Canzoneri (1983), by contrast, has related the two distinctions by positing an environment in which desirable activist responses depend upon a state variable about which the monetary authority has private information (i.e., one not currently observable by individual agents). This makes it impossible for agents to verify, in a given period, whether the current rate of money growth differs from its average value because of an activist, rule-dictated response to current perceptions, or because the monetary authority is attempting to exploit initial conditions as in a discretionary equilibrium. But while that point is correct as stated, it does not imply that *from* a series of observations the public (i.e., individual agents) cannot tell whether the monetary authority is following a rule or behaving discretionarily, for the average money growth rates will differ. Consequently, the difference between the two distinctions seems important, despite Canzoneri's example.

The main messages that I see in all of this are, then, those stressed by Barro and Gordon. They are that (1) discretionary behavior tends to lead to excessive inflation, and (2) the operation of rules does not preclude activist stabilization responses. Reputational considerations may move the outcome in the direction of an optimal rule equilibrium, but will do so to a limited extent. What is needed to prevent excessive inflation, and expectations of the same, is the adoption of an appropriate policy rule.

It may be noted that the undesirably high inflation rates in discretionary equilibria in the Barro-Gordon framework do not necessarily correspond to imperfect credibility as defined above—that is, as existing when there is a divergence between privately expected and actual or officially announced values. There is, however, an interpretation of the discretionary

11. Cukierman and Meltzer (1983, pp. 35-35) suggest that their framework does not involve any dynamic inconsistency 'because the 'action' taken by the public [forming expectations of money growth] does not depend on the future settings" of policy variables. As the same expectation formation is the public's only "action" in the Barro-Gordon and Kydland-Prescott setups, these must also involve no dynamic inconsistency in this sense. A different concept might define dynamic inconsistency as obtaining when there exists a discrepancy between instrument settings under rules and under period-by-period decisionmaking (given the same preferences and technological constraints in each case). This sort of discrepancy would prevail in the Cukierman-Meltzer framework, if rules were considered.

equilibrium path that matches the second of these definitions precisely. Suppose that in period t the monetary authority takes $E_{t-1}\Delta p_t$ as given, but that he recognizes that *future* surprises have expected values of zero. Thus, in period t he chooses $\Delta p_t > 0$ and *plans for* $\Delta p_{t+1} = \Delta p_{t+2} = \ldots = 0$. Then when period $t+1$ comes around, the relevant initial condition is that $E_t\Delta p_{t+1}$ is given, so the authority chooses $\Delta p_{t+1} > 0$ and plans $\Delta p_{t+2} = \Delta p_{t+3} = \ldots = 0$. In each period, according to this story, the monetary authority takes actions that differ from those that he planned, last period, to take. Then if his announcements accurately represent his plans, the equilibrium will be one in which inflation in each period—or more generally on average—exceeds its previously planned and announced value. Rational private agents' expectations will, on average, equal actual values, so they will be different from planned and announced values—a situation of low credibility.

To this picture it may be objected that the policymaker is posited as behaving in a peculiar manner. In particular, he is not accurate in his predictions about how he himself will behave in the future. Dynamic inconsistency thus prevails in a different sense than that described by Barro and Gordon (1983a, p. 599). This objection is well taken, but on behalf of the story (equilibrium concept) offered, it can be said that it describes a process in which outcomes are consistently less desirable than those planned and announced by policy authorities. In particular, there is in the example at hand more than zero inflation on average even though the monetary authority is always planning and announcing that the inflation rate will be zero in the future. It seems possible that this story has some relevance for actual economies.[12] It certainly conforms in several respects—target misses, base drift, positive inflation—to the portrayal of the U.S. experience as described by Hetzel (1984c), Lombra-Moran (1980), and other knowledgable observers.

Macro policy credibility in the United States

In this section we turn our attention more specifically to the United States and, in particular, to its monetary authority, the Federal Reserve. In a discussion concerning credibility, the first thing that needs to be said about the Fed is that it appears, from the viewpoint of an outside observer,

12. It is, in my opinion, not obvious that it is wrong to assign a different extent of rationality to private agents, whose modeled actions impinge primarily on their own welfare, and policy authorities, whose modeled actions impinge primarily on others. To treat such actions differently is to admit to having a poor model of the political process—something that I am willing to do—for, with a good one, policymakers could simply be treated as maximizing their own private individual utility subject to the constraints of the political process.

that the Fed has no desire for a situation of high credibility. Of course, it would prefer that the public expects that future inflation rates will be low. But, as emphasized earlier, that is not the same as desiring a high degree of conformity in general between public beliefs about policy and the Fed's own plans.

There are various ways in which the Fed's actions and procedures suggest the absence of a desire for public understanding of the policies being pursued. One obvious example in this regard was the Fed's opposition in the 1970s to Congressional proposals for the announcement of monetary targets. Of continuing significance is the practice of announcing target ranges—with quite wide bands—for a number of different monetary aggregates. In addition, there is the ambiguity concerning the meaning of the "targets"—are they something that the Fed attempts to achieve, or do the numbers serve merely as indicators relevant to judgments about current conditions?

To these observations it might be countered that the Fed's position is appropriate since it is undesirable to have targets expressed in terms of monetary aggregates. The items of actual concern are macroeconomic goal variables such as inflation, employment, output growth, etc. Thus it is undesirable for the Fed to try to achieve announced monetary targets in the face of exogenous disturbances; instead, according to this argument, it should readily abandon monetary targets when to do so would result in better fulfillment of macroeconomic goals. Consequently, the argument concludes, the ambiguity concerning monetary targets is not evidence of any lack of desire to communicate actual goals. But if that is the position of the Fed, then it should be happy to announce target paths for the goal variables, if it wants its plans to be understood by the public. In fact, of course, the Fed is on record as opposing the establishment of publicly announced targets expressed in terms of goal variables.[13]

The absence of a desire for credibility is also suggested by the type of dialogue that often arises in response to criticism or suggestions for procedural changes. For example, officials of the Fed have frequently responded to criticism regarding money stock variability—i.e., fluctuations in M1 growth rates—with the assertion that the Fed is unable to exert control over the aggregate in question over short spans of time. Almost simultaneously, other officials of the Fed have argued in opposition to proposals for the adoption of operating procedures that would serve to improve month-

13. See, e.g., Volcker (1983).

to-month monetary control.[14] Then, in response to the criticism that arises naturally from this concatenation, it has been argued that short-run monetary control is unimportant; as long as the money supply is well managed over longer intervals there is no need, it is argued, for improved month-to-month control. But that position is hard to reconcile with the Fed's tendency to permit 'base drift," i.e., its practice of expressing each period's money stock target in terms of percentage changes from that period's starting value, without adjustments to compensate for target misses of the previous period.[15] Clearly, if misses were white noise, this practice would lead to random-walk behavior of money stock deviations from any given target path—which is not what most economists would mean by long-run control.[16] More generally, long-run control under almost any definition requires either accurate month-to-month control or an absence of base drift. It is thus difficult not to obtain the impression that the Fed places little value on long-run monetary control—an attitude that sharply contradicts the Fed's own statements about the relationship between inflation and money growth, together with its announced determination not to contribute to inflation.[17]

It might be possible to construct an argument that inflation (and thus monetary control) is not actually of much importance,[18] but that is not the issue under discussion. The point of the previous paragraph is that the nature of the Fed's multipart response to its critics is not of a type that would engender belief that the Fed is frankly conveying a clear notion of its goals and intentions.[19]

As a result of the record of the last 15 years, many economists have concluded that basic institutional reforms will be required to create a high

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14. Especially relevant in this regard was the Fed's long-lasting opposition to contemporaneous reserve requirements. One of the reasons given for the Fed's reluctance to change—the possible technical infeasibility of banks' compliance with contemporaneous requirements—was itself enough to give one doubts about the candor of the position (given that such requirements prevailed before 1968). As a climax to the matter, when the Fed finally introduced in 1984 a scheme that it describes as contemporaneous reserve requirements, it chose one that continues to feature a two-day lag between the end of computation and maintenance periods. As Goodfriend (1984) has explained, this two-day lag could—depending on whether the Fed stabilizes the federal funds rate during the two days—make the system no different for monetary control purposes than others previously found wanting.

15. From 1975 to 1978, base drift could occur every quarter; since the passage of Humphrey-Hawkins legislation in 1978 it occurs once each year, with a second occasion possible (and realized in 1983).

16. Barro (1982, p. 105) refers to this type of regime as 'one that possesses no nominal anchor."

17. See, for instance, Volcker (1984).

18. I would not try to do so.

19. Another indication is provided by the Fed's opposition to the prompt release of trading-desk directives and minutes of FOMC meetings.

degree of credibility for promises that the Fed will not permit inflation in the future. The basic aim of the proposed changes is, of course, to generate noninflationary behavior of the monetary system, as opposed to optimism unrelated to any changes in the forces that have resulted in the inflation of the past. A number of quite distinct poposals, representing different monetary standards, have been presented. Prominent among there are proposals for:

- Adoption of a gold standard or some other commodity money system.
- Passage of a constitutional amendment requiring a balanced budget for the federal government in each year.
- Legislative imposition of a monetary rule upon the Fed.
- Conversion of the Fed into a bureau of the Treasury.

A complete consideration of these proposals is clearly beyond the scope of this paper. But since each of the first three involves the adoption of some form of a rule involving precommitment, discussion of certain aspects is needed. More complete reviews have been provided by Stein **(1980)** and Friedman **(1984).**

The logical attraction of a genuine gold standard[20] is that it makes the price level—i.e., the money price of commodities in general—a relative price. There are then limits on the extent to which the price level can change over any given span of time, limits that are determined by changes in tastes and technology rather than the speed with which paper money and bank deposits can be created. Thus it seems almost certain that severe inflation could not occur while a gold standard was in operation. The system does, however, permit significant cyclical fluctuations in the price level, corresponding to relative price changes between gold and commodities in general. How severe these fluctuations would be is a matter open to

20. Friedman's (1961-1984) distinction between "real" and "pseudo" gold standards is somewhat unclear. It has been summarized by Stein (1980, p. 63) as follows: "A real gold standard is a condition in which gold and promises to pay gold are circulated and exchanged freely but in which the government does not peg the price of gold relative to the national currency . . . . In a pseudo-gold standard, the government fixes the price of gold by standing ready to buy or sell." It would seem that the existence of a national currency with a pegged gold price would constitute a genuine gold standard provided that this price is maintained permanently. The gold standard then amounts to a rule governing the behavior of currency issues, one that subordinates the currency in a way that makes it consist of "promises to pay gold." Aid in understanding Friedman's point is provided by a useful paper by Cagan (1982) that describes the forces for management of actual gold-standard systems in a discretionary manner. Cagan also describes the influences that tend to bring about the breakdown of such systems.

dispute, but most students seem to believe that the magnitude could be troublesome. Various writers have consequently proposed a monetary standard based on a composite commodity bundle, rather than a bundle consisting of gold alone. Hall (1982), for example, has suggested that a bundle composed of ammonium nitrate, copper, aluminum, and plywood (in specified quantities), would have rather small relative price changes—relative to commodities in general—in the United States of the present day.[21]

A significant difficulty with a composite commodity system is that a bundle such as Hall's would not possess the historically based, mystical attractiveness of gold. All arrangements concerning the bundle would obviously be the product of explicit attempts to consciously devise a desirable monetary system. But in the absence of the mystique widely accorded gold, there would be little reason to prefer a commodity money system in comparison to one based on fiat money. Furthermore, if the commodity standard (i.e., the 'dollar" price of the bundle) were adjustable, as Hall proposes, a monetary authority not bound by a rule would have the same type of incentive for discretionary behavior as exists under our present system.[22]

It shouldalso be mentioned that much of the apparent support for a gold standard is probably based on distorted views of what such a system entails. Friedman (1984, p. 45) has conjectured that a genuine gold standard 'has minuscule political support."

Let us turn next to the second item. While the notion of a constitutional amendment provides an attractive route for possible institutionalization of a non-discretionary policy rule, the emphasis that has been given to balanced budgets seems slightly misplaced. An example in a recent paper of mine (McCallum, 1984b, pp. 130-31) illustrates that in principle an economy without excessive monetary growth can avoid inflation even if it maintains a positive deficit that gives rise to an ever-growing stock of government debt.[23] Strictly speaking, this result requires rather extreme Ricardian assumptions involving infinite planning horizons and lump-sum taxes. But one does not have to believe in the literal empirical accuracy of these to accept the point made by this example, which is that government purchases (absorption of resources) and money creation—rather than

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21. Hall's paper includes the unorthodox contention that government purchases and sales of the bundle would be unnecessary and undesirable. I will not attempt to consider that suggestion here.

22. These problems are recognized by Hall (1982), p. 112: The commodity standard is not inherently superior to fiat money as a way to stabilize the cost of living:

23. The example is of some theoretical significance because it occurs in the context of a general equilibrium model in which all agents maximize explicitly specified objective functions and all markets clear.

deficits—are the macroeconomic policy variables of fundamental importance. Consequently, an amendment whose intent is to avoid excessive growth of nominal aggregate demand should be designated to place limits on government purchases (rather than taxation) and on money creation.

One other point to be made about any amendment whose purpose is the establishment of a policy rule concerning fiscal variables, is that it would be unfortunate if its design were to eliminate the built-in automatic stabilizers provided by a tax system that relates receipts to current income.[24]

Closely related is the much-discussed possibility of congressional imposition of a rule that would constrain and precommit the behavior of the Fed.[25] The main reasons why such a rule should be beneficial are implicit in the discussion of the previous section; here the relevant issue is whether there is any reason to expect that Congress would choose to impose such a rule. In that regard, the analyses of Hetzel (1984a, 1984b) and Kane (1980) are not encouraging. According to Hetzel, discretionary period-by-period policy behavior results from an attempt to appear responsive to the conflicting desires of various politically significant groups, the intensity of whose desires fluctuates from month to month and year to year. The effect of this hypothesis is reinforced by Kane's scapegoat theory, according to which members of Congress want the Fed to have a substantial amount of discretion so that each member can attempt to place blame on the Fed, ex post, for unpopular developments. Each of these lines of reasoning seems to suggest that the likelihood of Congress imposing an operationally well-defined rule on the Fed is lower than the likelihood of the Fed adopting such a rule of its own volition.

Recently, Friedman (1983, 1984) has mentioned the possibility of legislation that would "end the independence of the Fed by converting it into a bureau of the Treasury Department" (1984, p. 43). He suggests that while this arrangement would be "by no means ideal . . . it would be a great improvement over the existing situation, even with no other changes" (1984, p. 45). The basis for this judgment is that bringing the Fed inside the administration would provide it with a "bottom line" that would serve as a check on the bureaucratic inertia that prevents reform (1983, pp. 114-18). The bottom line in question would, however, result from potential voter dissatisfaction rather than the type of financial incentives faced by a

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24. This concern would be unnecessary if the economy were perfectly Ricardian. The viewpoint being taken is that the Ricardian model provides a good starting point for analysis of macroeconomic phenomena, but that its conditions are unlikely to obtain in full.

25. Also possible is a constitutional amendment restricting monetary behavior (Friedman, 1984, pp. 41-42).

private business firm. In view of the type of performance that has been forthcoming from Congress and recent administrations, it is unclear that better results would obtain. It would appear that the monetary authority would, if placed in the Treasury, be faced somewhat more directly with the same type of conflicting and fluctuating pressures that it is now subject to indirectly. If such pressures are in fact an important reason for discretionary behavior, this arrangement would be unlikely to lead to improved performance. The case of the Bank of Israel is relevant in this regard.

Before concluding this discussion of proposed institutional reforms, a few words should be added concerning one that has received a great deal of attention recently, namely, that the Fed engage in 'nominal GNP targeting."[26] This proposal has been discussed, by both friends and foes, as if it were something dramatically different from money stock targeting. Consequently, I would like to suggest that they are in fact highly similar. Some essential features of similarity are as follows:

- Both assign the monetary authority an objective stated in terms of a nominal variable.
- In both cases, this variable is not itself an ultimate goal variable or an instrument that can be manipulated directly by the Fed.
- Thus in both cases specification of the target does not amount to an operational rule.
- Such a rule can be easily constructed, however, by specifying adjustments to the growth rate of the monetary base or the Fed's portfolio that would automatically take place whenever the GNP or money-stock variable is above or below its target path.
- For the avoidance of inflation, that target path needs to be defined in level (rather than growth rate) terms or, equivalently, base drift must be scrupulously avoided.

Of course the operating characteristics of a system based on nominal GNP targets will be different from those of one based on M1 or M2 money supply targets. But, given institutional arrangements under which the money stock is not directly controllable, this difference is one of a technical nature that does not involve major issues of principle or ideology. More important issues, in my opinion, involve the presence vs. absence of

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26. See, e.g., Gordon (1983), Hall (1984), and Taylor (1985). The scheme described in McCallum (1984a) uses nominal GNP target departures as input variables to a fixed but semi-activist rule prescribing growth of the monetary base.

operational rules for manipulating a controllable instrument and the presence vs. absence of base drift.

Conclusion

It remains to bring together some of the diverse themes presented above concerning credibility of monetary policy,[27] an attempt that will be made here. In the first section it is maintained that evidence purporting to contradict the validity of the credibility hypothesis—i.e., the importance of expectations for output-inflation tradeoffs—is unconvincing at best. Given the strong theoretical basis for this hypothesis, it then seems reasonable to base analysis involving macroeconomic policy on specifications in which inflation-rate expectations play a central role. In the following section, the Barro-Gordon analysis, which builds upon precisely this sort of a specification, is reviewed, together with elaborations and related arguments. The main message is that attempts by the monetary authority to optimize on a discretionary period-by-period basis tend to result in more inflation, and no less unemployment, than would prevail under a mode of operation that involves a fixed, but perhaps activist, monetary rule. A successful anti-inflationary policy would then seem to require the adoption of rule-like behavior, the central feature of which is abstention from attempts to exploit each period's historically given initial conditions.

A discouraging aspect of this conclusion, mentioned in the last section, is that discretionary behavior appears to reflect a response to political pressures of a type that may impinge more directly upon Congress and the executive branch than upon the Fed. Consequently, it seems unlikely that steps to end period-by-period monetary policymaking will be forthcoming from Congress or any part of the executive branch. Nor does it seem likely that constitutional amendments of an effective type can be relied upon.

There are reasons for believing, then, that the best hope—discouraging experiences notwithstanding—lies in the possibility of adoption of something closer to rule-like behavior by the Fed itself. In that regard, it should be noted that the Barro-Gordon analysis does not imply that such an outcome is infeasible; it merely *assumes* that discretionary or reputational equilibria will be established in the absence of mechanisms for binding precommitments. But while the Fed cannot literally precommit its future actions, it can adopt procedures that would make departures from a pre-

27. Certain portions of the discussion are equally applicable to a discussion of the credibility of fiscal policy. The model used to analyze the reasons for credibility problems would not, however, appear to be appropriate for such a discussion. Issues involving the interaction of monetary and fiscal policy have been recently discussed by Blinder (1982).

selected rule costly to itself. If, for example, the Fed were to adopt an operational rule such as that described by Hall (1984, p. 68) or McCallum (1984a, p. 390),[28] then a host of activities and procedures involving rapid and accurate collection and processing of the requisite data would grow up and become established. Public statements and lectures explaining the benefits of the rule—and perhaps even the infeasibility of departing from it—would be given by Federal Reserve Board members, advisers, and system economists. Departures from the rule would come to require justification, and proposals for departures would inflict embarrassment on those individuals who made them. In time, the whole gamut of forces for bureaucratic inertia emphasized by Friedman (1983) would come to work on behalf of adherence to the rule.

But would this sort of behavior not deprive the Fed of the political benefits of period-by-period discretionary policymaking emphasized by Hetzel (1984b), namely, those obtained by appearing responsive to the multiple, shifting objectives of various politically significant groups? There is of course some danger involved, but there are also dangers associated with the attempt to be responsive. In particular, there is the danger that the groups in question will come to recognize that the Fed cannot deliver the desired outcomes. Actions involving redistribution can help one group but only by hurting others, while extra attention during one part of the business cycle requires below-normal attention during other phases. Thus, the type of behavior under discussion produces only the appearance of being responsive to all of the various interest groups.[29]

Furthermore, there is an important danger involving the ***independence*** of the Fed, i.e., its existence as an entity dictated to by neither Congress nor the executive branch. In a democratic system of government, the ultimate justification for this sort of independence would seem to be based on the presumption that it will promote far-sighted behavior,[30] modes of operation that avoid the pursuit of transitory benefits that entail poorer performance on average over long time spans. But the choice between discretionary and rule-like behavior amounts to the choice between a way

28. It is crucial in this regard that the rule be operational, i.e., specified in terms of a controllable instrument variable, in order to minimize possible self-deception. Adoption of an intermediate target variable, be it MI or nominal GNP, does not constitute adoption of a rule.

29. Another problem with Hetzel's argument is that it seems to presume that rules must be of a non-reactive type, i.e., unresponsive to current conditions. Thus he says, 'The requirement of balancing multiple goals among which priorities change . . . creates the demand for flexibility, and absence of precommitment" (1984b, p. 18).

30. Volcker (1983) refers to the "independent status of the Federal Reserve that makes a longer-term view possible."

of doing business that is always focused on the immediate present and one that takes a longer perspective. Discretionary behavior is then, in this view, fundamentally inconsistent with the raison d'etre of an independent monetary authority. The decision not to adopt rule-like procedures for monetary policy, in other words, constitutes neglect of the Fed's institutional mission. One would expect prolonged neglect of this type to lead to public calls for institutional reform, a conclusion that derives some support from the experience of the past few years.

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Commentary

Alan Blinder

I would like to begin by quoting Ben McCallum's words at an earlier conference:

> "My reaction to the paper. . . is one of great enthusiasm. What a discussant wants most in a paper, after all, is something with which he can wholeheartedly disagree. And for me the . . . paper is unusually rich in such items."
>
> —B.T. McCallum, March 1984

Actually, I don't disagree with everything in this paper. For example, neither Ben nor I like the gold standard. But, of the four main points I find in Ben's paper, I disagree with all.

They are:

- We should not be convinced by evidence showing that the recent disinflation is more or less in line with earlier Phillips curve estimates.
- Central banks lack credibility because, in their effort to cause unanticipated inflation, they wind up causing excessive anticipated inflation—for reasons outlined by Barro and Gordon.
- The Federal Reserve System does not want its policies to be credible.
- The Fed should get around these time-inconsistency/credibility problems by adopting and adhering to a fixed rule.

The evidence from the recent disinflation

By now, quite a few people have noticed that, given the unemployment we experienced, the recent disinflation in the U.S. was more or less in line with what earlier econometric estimates of Phillips curves suggested—in apparent contradiction to the credibility hypothesis.

To the studies cited by Ben, I would add a fascinating paper by Ando and Kennickell (1983) that shows not only that the equation in the current MPS model (which is little different from the equation estimated in 1973) tracks the last decade very well, but that even the version estimated by DeMenil and Enzler back in 1970 does not do all that badly.

I can also add the following personal observation.

For some years now, I have been using the simple rule of thumb that each point of unemployment, henceforth U, above 5.8 percent (my estimate of the natural rate) reduces p by 0.5 points. (This corresponds to a "sacrifice ratio" of 5-6.) Periodically during the disinflation of the past three years, I have checked the accuracy of this rule, and been constantly amazed by its accuracy.[1]

Using the four years from 1980 to 1983 as the disinflation period, the rule of thumb says that inflation should have fallen by 5.4 points between early 1980 and early 1984. No matter what price index you use, this is not far from the actual decline. If you then factor in the amazing climb of the dollar, it seems surprising that inflation has not declined further.

Yet somehow McCallum claims that "the evidence purporting to contradict . . . the credibility hypothesis . . . is unconvincing at best." Why? Because he estimates an old-fashioned Phillips curve—with no supply shock variables—over 1954-1982, and finds that the coefficients on lagged inflation are higher post-1966 than pre-1966.

I find McCallum's alleged evidence on credibility rather incredible.

The credibility hypothesis is a very *specific* application of the Lucas critique, which says that you will get more disinflationary bang for your unemployment buck if you pursue a tough anti-inflation policy. In terms of a theoretical expectations augmented Phillips curve,

$$\dot{p} = \text{const.} + E(\dot{p}) - aU,$$

it says that a gets bigger.

But Ben simply identifies the credibility hypothesis with the *general* Lucas critique and looks for any parameter shifts. Now, the one parameter shift that we all know took place—thanks largely to the annual Phillips curves estimated by Bob Gordon—is that the coefficients on lagged inflation (interpreted as a proxy for expected inflation) rise as you extend the sample beyond the late 1960s into the early 1970s, and then stop rising.

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1. A published example appeared in the Boston Globe on Feb. 9, 1982, under the title "Unemployment up means inflation down."

McCallum finds this. But so what? He is testing for a shift of the wrong parameters in the wrong time period. He should be looking for changes in the U coefficients during the disinflation of the early 1980s.

What would such a test show? To find out, I ran some regressions of my own.

- First, I (approximately) replicated his equation 6 and then extended the sample one year—to 1983:IV. The differences were trivial.
- Then, following McCallum's procedure, I tested for shifts in the unemployment coefficients—starting the dummy in 1980:III, roughly when disinflation began.
- Results: The two dummy variables got roughly equal and opposite coefficients, each with a t statistic about 0.5 in absolute value. The F-statistic for the joint hypothesis that both were zero was $F = 0.16$.
- If we accept the point estimates at face value, the U coefficients in my version of McCallum's equation are

$$-.0006U(t) - .0011\Delta U(t)$$

until 1980:II and

$$-.0006U(t) - .0002\Delta U(t)$$

after. So the point estimates say that there was no change in the level effect and a large reduction in the ephemeral effect of rising unemployment.
- Next, I ran the equation only through 1980:II and looked at post-sample prediction errors.

Looking first at one quarter-ahead residuals, 9 of the 14 are negative (as the credibility hypothesis suggests). But that's not much more than 50-50, and none of them are larger than one standard error. The only large residuals are positive, making the average prediction error slightly positive.

Similarly, a 14-quarter dynamic simulation of the model leaves the price level only 0.9 percent too high by 1983:IV.

- Conclusion: If the right questions are asked, McCallum's specification gives the same answer as the others: The disinflation was just about what should have been expected, given the behavior of U.

This means either that credibility is not very important for the slope of the Phillips curve, or that the Fed did not gain credibility despite the deep recession.

But there is one pretty glaring fact that argues against the second interpretation.

Starting in October 1979, Chairman Volcker publicly and repeatedly identified inflation-fighting with money targeting. He then put us all through a small depression to lower inflation, all the while stressing the importance of controlling M growth. Then, in October 1982, he suddenly abandoned money targeting and let the M's soar, while pledging that this policy change did not mark abandonment of the battle against inflation.

If he lacked credibility, long-term nominal rates would have shot up. Instead, they fell, suggesting that Paul Volcker has both chutzpah and credibility.

Thus the evidence strongly suggests that the credibility hypothesis, sensible as it is, is not of great empirical importance.

Why central banks lack credibility

In the next section, Ben is very happy with the Barro-Gordon explanation for high inflation and low credibility. I am not. One set of objections is practical, the other theoretical.

On the practical level, I think we must seriously entertain the notion that many of the surprises in M are just as surprising to the Fed as they are to us, i.e., that they are not deliberate policy moves.[2] Short-run M surprises may be of little importance anyway. Mishkin's (1982) results suggest that they mean nothing special for output—and hence fail to reap the benefits assumed in the Barro-Gordon analysis. Furthermore, since we all know that M affects P with a long and variable lag, short-run money surprises mean virtually nothing for inflation.

If the Fed's actions are not the source of unanticipated inflation, maybe not even of unanticipated M, and if unanticipated M is not very important anyway, then the Barro-Gordon analysis may not be a good guide for practical policymaking.

On the theoretical level, the way Barro-Gordon handles reputation and credibility is—as they themselves admit—ad hoc. It is only one of many possibilities.

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2. This idea rings true, and is similar to that of the Cuckierman and Meltzer paper that McCallum cites.

Davis Backus and John Driffill (1984) have ingeniously applied the theory of reputation due to Kreps and Wilson (1982) to the Barro-Gordon model, and reached rather different conclusions.

According to Backus and Driffill, lack of credibility stems from the fact that the public is not sure about how serious the government is about fighting inflation. The government tries to build an anti-inflation reputation by being tight-fisted, while the public learns in a Bayesian manner. (Does this sound familiar?)

As a result, they show, the government may well stick to a tough anti-inflation policy for many periods—especially early in its term.

Thus, even within the Barro-Gordon framework, the government may—for a long time—opt for zero inflation, not for the high inflation posited by Barro-Gordon.

Does the Fed want credibility? How can it get it?

Ben then constructs a revealed preference argument that the Fed does not wish to be credible.

His evidence is that the Fed:

- refuses to announce clear and explicit target paths for ultimate goal variables like p and $\dot{y}$.
- equivocates on how important control of M growth really is, and permits base drift when it redefines its 'cones."

I agree with Ben that the Fed's pronouncements do not "engender belief that the Fed is frankly conveying a clear notion of its goal and intentions." But I don't think this is because the Fed loves inflation or wishes to be disbelieved.

First of all, if velocity follows a random walk, then allowing long-run base drift is perfectly consistent with a long-run P level target. On the contrary, rigid adherence to a predetermined path for M would make P drift away from its target path.

More importantly, however, it seems to me that the reason the Fed refuses to announce its goals for $\dot{y}$ and p is because these goals place far more weight on low inflation, and far less weight on high employment, than the goals of the body politic. Since it is impolitic to fess up, the Fed sets up smokescreens—just as its professed conversion to monetarism in 1979 was a smokescreen for pushing interest rates up.

Notice that this interpretation of the Fed's fondness for baloney is the absolute opposite of McCallum's. In his view, the Fed dissembles because

it is surreptitiously promoting inflation. In my view, it dissembles because it is surreptitiously promoting unemployment.

Should the Fed commit itself to a rule?

In his concluding section, Ben takes the optimality of a fixed rule for granted and suggests using a feedback rule for manipulating the monetary base as a way to keep nominal GNP on a preassigned path.

I'm not convinced—for several reasons.

- While a $\dot{Y}$ rule is no doubt better than an M rule, holding to a predetermined path for Y is a very unforgiving policy when there are supply shocks. If $\dot{Y}$ is fixed, then $\dot{y}$ must fall by as much as p rises. This seems suboptimal to me.
- Ben's main argument for preferring a rule to discretion amounts to a preference for far-sighted over short-sighted policies.

No doubt, far-sighted policies are better than short-sighted policies, and discretionary policy is sometimes myopic. But I don't think this is inevitable. For example, discretionary policy, not constitutional rules, has kept commercial development to a minimum in the Grand Tetons. And the same can be said for environmental policy in general.

Besides, given limited knowledge about how the economy works, I doubt that we can design a rule that we'll be happy to live with for a long time. So when to change the rule will always be a discretionary decision.

- This brings me to my last point.

Policy rules with feedback, computed in the Tinbergen-Theil framework, used to be thought of allegorically—as approximate descriptions of reasonable behavior, around which there would always be deviations. An optimal rule was not meant to be written into law and followed religiously; it was meant to give guidance to policymakers. Thus I always thought of a feedback rule as a stylized representation of discretionary policy.

The time-inconsistency literature has changed this perspective. Suggested feedback rules are now meant to be taken *literally*—as formulas that obviate the need for human intervention. McCallum clearly advocates a rule as a way to tie policymakers to the mast so that they cannot exercise discretion.

While I recognize that time inconsistency is a problem, and realize that to err is human, I am troubled by this new perspective. For I think it loses

touch with reality, and thereby contributes to the growing irrelevance of economic research to economic policy.

As Jim Tobin (1982) put it on this platform two years ago: "Policy rules are a myth of economic theorists' simplified models. It is in practice impossible, politically [and] economically . . . to prescribe in advance for all contingencies the behavior of future presidents, legislators, and central bankers. It is. . . not credible that responsible officials will not react to the circumstances of the day as they and their constituents perceive them. It is in practice impossible to draw a line between responsive 'feedback' rules and discretion."

In a word, I fear that if academic economists insist on playing intellectual parlor games about how best to replace the Federal Reserve Board and the president by a Fortran statement, we will lose what little credibility we still have.

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Monetary Strategy with an Elastic Price Standard

Robert E. Hall

When the Open Market Committee met in mid-1973 to make the policy decisions that would influence the economy in 1974, the situation seemed serene. The price level stood at 98.5 as measured by an index with 1952 = 100. Unemployment was 5.8 percent, close to its normal level of 6 percent. The Committee forecast that the 1974 price level would rise to 99.7 percent, a development the Committee welcomed because they had a strategy of holding prices at 100. The forecast for unemployment was 6 percent.

Late in 1973 and for the first half of 1974, OPEC hit the economy with an unexpected price shock of unprecedented magnitude. The price level rose to 102.4 and unemployment reached 6.6 percent. After settingpolicyfor 1975, the Committee forecast that unemployment would remain at 6.6 percent and the price level would rise to 104.8. This forecast put the economy on track as far as the Committee's strategy was concerned, for that strategy permitted the price level to rise above the target of 100 by 8 points for each point by which the unemploymentmte exceeded 6 percent. In their view, this stmtegy permitted the economy to roll with the punch when a shock struck.

More bad news hit in 1975. Unemployment turned out to be far worse than the Committee or any other forecaster thought: It averaged 8.4 percent. But the price level rose to only 104.2. Strong stimulus was put in place in 1975 so that forecast unemployment for 1976 was down to 6.5 percent. The price forecast for 1976 was 104.0, so that once again the elastic target was satisfied.

The Committee debated vigorously about the degree of stimulus necessary to bring unemployment down by this much in a single year: As it happened, they chose slightly too much stimulus. Unemployment was actually 6.0 percent in 1976 and the price level was 104.0.

The economy proceeded smoothly through 1977 and 1978. Unemployment in 1978 was 6.4 percent, and the price level was down to 103.2. The Committee's forecast was for continuing gentle deflation until the price level returned to its original level of 100.

In 1979 and 1980, OPEC struck again, before the lingering effects of the first shock were completely worked out. Again, policy let the economy roll with the punch. The situation was much more favorable this time because the price shock was not accompanied by an adverse demand shock; in fact, there was a favorable surprise about unemployment in 1981. Unemployment reached 6.7 percent in 1980 but was back down to 6.2 percent in 1981. Again, the price level absorbed most of the shock in the short run. It reached 109.7 in 1981. In 1982 and 1983, the Committee slowed the economy a bit with contmctionary policy that mised unemployment to 7.2 and 7.4 percent. The price level fell gradually and reached 107.8 in 1983.

As of mid-1984, the Committee plans to continue slightly slack conditions in order to bring the price level back to the long-run target of 100 in 1952 prices. At no time in the 30-year history of the elastic price standards has the price level gone above 110. Only once has the unemployment rate exceeded 7.5 percent.

Unhappily, a report on the history of postwar monetary policy doesn't read like this at all. Instead, the price level in 1983 was 372 on the basis of 1952 = 100. And unemployment did not do nearly as well either—it actually exceeded 7.5 percent in five different years. The reason for the poor performance of monetary policy was the lack of a strategy. My main point in this paper is that almost any monetary strategy would have given performance similar to this fictional account. I give a menu of policies, out of which the anti-inflation hawk can choose one and the anti-unemployment dove can choose another. What is most interesting is that the hawkish policy would have given a better record for *unemployment* and the dovish policy a better record for *price stability* than we got from actual policy.

It is not enough to formulate the strategy of monetary policy as bringing about price stability. Few economists endorse the unlimited manipulations

of monetary instruments as necessary to ensure complete price stability, without regard for the state of economic activity. Rather, the strategies promoted by economists implicitly or explicitly accept some fluctuations in the price level so as to cushion real activity. Price stability in the longer run is hoped to be the outcome of these strategies.

Professional opinion has settled on two compromise strategies. The first is constant monetary growth. When the portfolio of the Federal Reserve grows at a predetermined rate and does not react to events in the economy, shocks to supply and demand can raise and lower prices in a cushioning way in the short run, but in the long run the price level is supposed to remain close to constant. Unhappily, the promise of price stability will go unfulfilled if there are long-term shifts in the demand for the Fed's liabilities. Moreover, if these shifts occur quickly, as they did in the early 1980s, they can be destabilizing to real activity as well.

The second strategy, constant growth of nominal GNP, has enjoyed growing popularity among macroeconomists as the defects of constant money growth have become apparent. Again, prices are allowed to fluctuate in the short run under a nominal GNP rule, but will tend toward a stable level in the longer run. Except possibly for transient errors in executing constant nominal GNP growth, the shifts in monetary velocity that are so troublesome for a money growth rule are benign under the nominal GNP rule. The only threat to longer-run price stability under the nominal GNP rule is an unexpected shift in the growth of full-employment GNP, which will bring a change in inflation in the long run of opposite sign and the same magnitude.

My point here is to advance the discussion beyond a comparison of the two major existing proposals. I will formulate a monetary strategy where the two goals of long-run price stability and short-run employment stability are stated more clearly than they are in the constant money growth rule or in the nominal GNP growth rule. Specifically, I will examine an elastic price target. Under this strategy, the Fed is instructed to stabilize the price level at a particular value. However, the strategy is elastic in the short run because the Fed is given some leeway in achieving the target depending on the amount of unemployment. When a price shock hits, the Fed does not have to clamp down on the economy right away to get the price level back to the target. Instead, when unemployment rises, the allowable price level rises as well. When the economy begins to recover and unemployment falls toward its normal level, the Fed has to take action to get the price level back down to the target. Because the economy always tends toward an equilibrium with normal unemployment, the Fed ultimately has to

achieve the price target. But the linkage to unemployment cushions the economy in the desirable way in the short run.

The formal statement of the elastic price strategy is clean and straightforward: Monetary policy is on track when the deviation of the price level from its constant target level is eight times the deviation of unemployment from its normal level. Policy is too tight if the price deviation is less than eight times the unemployment deviation; it is too expansionary when the price deviation is more than eight times the unemployment deviation. The elasticity of 8 in this statement is a matter for policymakers to choose; hawks may want an elasticity as low as 2 and doves may go as high as 10. Later in the paper I will provide some data that will show the alternative consequences of the choice of elasticity.

When the elasticity is chosen to be about 2.5 or 3.0, the elastic price strategy gives results that are quite similar to monetary targeting or nominal GNP targeting. Thus both policy strategies are somewhat elastic. However, optimal policy may well involve a higher elasticity. According to estimates that appear later in this paper, the standard deviation of unemployment would have been about 1.1 percentage points under an elastic price strategy with an elasticity of 3, and only 0.8 percentage points with an elasticity of 8. Of course, the improved stability of unemployment under a higher elasticity would come at the cost of worsened performance for price stability—with an elasticity of 3, the price level would have had a standard deviation of 2.7 percent around the target, as against 3.4 percent with the elasticity of 8.

The elastic price standard is not an arbitrary choice as a strategy for monetary policy. Under rather general and plausible conditions, it is very close to optimal to aim policy to achieve the elastic price standard. The choice of elasticity depends on the relative social costs of inflation and unemployment, but otherwise the form of the optimal monetary policy is almost exactly that given by the elastic standard.

The need for a monetary strategy

So many other authors have argued so persuasively, in my view, on the importance of precommitment to an anti-inflationary monetary strategy that I do not want to dwell on the point here. Bennett McCallum's paper for this symposium has added to the case that the adoption, once and for all, of a credible policy for stabilizing prices will itself make the job of price stabilization less costly. Further, I respect the case made by Kydland and Prescott (1977) and Barro and Gordon (1981) that reconsideration of

the goals of monetary policy each year invites that problem of policy inconsistency: Without precommitment, the payoff each year from creating a new inflationary surprise leads to a policy that is more inflationary than the optimal policy. To get to the optimum, policy choices must be made once and for all and embedded in a formula.

For many years, the case for a monetary policy strategy as a fixed rule was argued exclusively by monetarists. Precommitment to a rule was virtually synonymous with adoption of the monetarist recommendation of predetermined money growth. But the logic of precommitment applies to monetary strategies in general, not just the particular strategy of fixed growth of some measure of the money stock.

What we are looking for in a monetary strategy

The basic long-run goal of monetary policy is to provide stable prices. But shifts in monetary policy influence real economic activity in the short run. Consequently a monetary strategy has to balance the two objectives of price stability and smooth real growth. The two specific quantitative dimensions of economic performance that I will examine are variability in the price level and in the unemployment **rate.** In both cases, I will depart somewhat from conventional analyses, so some justification for looking at these two measures is in order.

Price variability

I will be concerned with the price level, not its rate of change. The goal of monetary policy, in my view, is not to keep the rate of inflation around zero; it is a little more ambitious—to keep the price level on target. Every time the price level shifts thanks to some random shock, the difference in objectives becomes important. Under inflation stabilization, policy does not try to bring about negative inflation after a burst of positive inflation. Instead, it attempts to prevent further inflation. The burst of inflation leaves its mark permanently in the form of a higher price level. Under price stabilization, policy pushes the price level back down to its target. Over long periods, the price level can drift up or down under inflation stabilization, whereas it cannot drift under successful price 'stabilization. Both types of policy will keep the average rate of inflation at zero.

My advocacy of price stabilization derives from my beliefs about why price instability is costly to the economy. The purchasing power of the dollar is a basic unit of measurement to the public. Many important economic decisions, especially those made by the general public, are stated in terms

of the dollar. A drifting price level interferes with good economic planning, especially personal planning. Let me give two examples:

- Private pensions almost always pay out a fixed dollar amount. When the price level drifts upward, the purchasing power of the pension is front-loaded. Retired people have trouble making side arrangements to equalize purchasing power over the years of retirement. Because the public doesn't fully understand price level drift, pension arrangements designed to offset it are rarely offered, and are unpopular when they are offered. A pension with stable purchasing power will necessarily pay less in the first year than a fixed dollar pension, if the price level is drifting upward.
- Mortgages involve payment streams that are roughly constant in dollars over their terms. The burden of the payments is far greater in earlier years if the price level is drifting upward. Even though mortgage payments are now frequently indexed to interest rates, no progress has been made at all in equalizing the real burden of payments over time.

Although a policy of inflation stabilization would solve some of these problems, price level stabilization would be even better. It is well within the power of monetary policy to promise a 30-year-old worker today that the purchasing power of the dollar at the time of his retirement 35 years later will be within 10 percent of what it is today. No such statement can be made under inflation stabilization.

Unemployment variability

Unemployment is socially undesirable, at least within the range likely to be experienced under a monetary strategy of price stabilization. On the margin, every reduction of unemployment appears to be good. Shouldn't the goal of a monetary strategy be the minimization of unemployment, not the reduction in the variability of unemployment?

The answer is that monetary policy is powerless to influence the average level of unemployment in the long run. As Milton Friedman (1968) argued persuasively almost 20 years ago, no amount of monetary expansion can bring a permanent economic high. A simple comparison of unemployment and inflation among the world's economies makes the point starkly. Countries with rapid money growth and high inflation have, if anything, higher unemployment than those with stable prices.

Given that monetary policy is forced to accept about 6 percent unemployment, on the average, and given the reasonable proposition that the

marginal social costs of unemployment above that level exceed the marginal gains below that level, the objective of policy should be low variability of unemployment. Assigning this limited objective to monetary policy does not in any way require the belief that 6 percent unemployment is socially optimal. Policies that bring permanent reduction of unemployment through improved labor market performance have a substantial social payoff. It is just that monetary policy is not one of those policies.

The policy frontier

Monetary strategies oriented toward limiting the variability of prices and unemployment can be classified along an axis of hawkishness and dovishness. **A** hawkish policy moves aggressively to offset every price disturbance, tolerating wide swings in unemployment as needed for price stability. It achieves a lower level of price variability at the cost of a high level of unemployment variability. A dovish policy keeps unemployment close to *6* percent and lets the price level swing more widely to absorb economic shocks. Its price variability is higher but its unemployment variability is lower. The idea that policy can be analyzed in terms of variability of unemployment and the price level has been developed by John Taylor in an important series of papers (1980, 1981, 1982).

I should be clear that not every policy is either hawkish or dovish. Some policies are just bad. It is perfectly possible for a policy to make unemployment fluctuate as much as it does in a hawkish policy and yet for prices to depart from target as much as they do in a dovish policy. In fact, actual policy had exactly that character over the postwar period, as I will show later in this paper.

I will define the ***policy frontier*** as the set of policies that give the lowest combinations of unemployment and price variability. A policy on the frontier has the property that no other policy can deliver both lower unemployment variability and lower price variability. **A** more hawkish policy can reduce price variability, but only by raising unemployment variability.

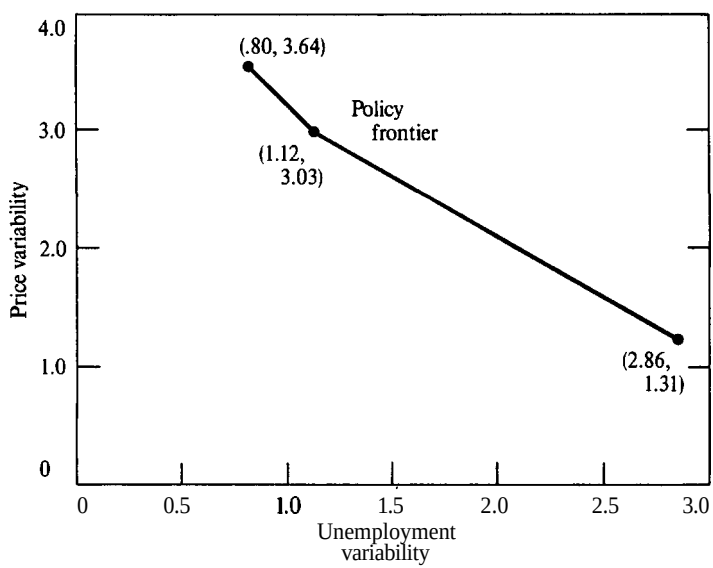
Figure 1 shows the policy frontier for the U.S. economy derived later in the paper. The horizontal axis is unemployment variability, measured as the standard deviation of the departure of unemployment from 6 percent. The vertical axis is price variability, measured as the standard deviation of the percent departure of the price level from a constant target. The policy frontier curves up and to the left, with dovish policies at the upper end and hawkish ones at the lower end. The curve of the frontier means that the more dovish policies have to incur more and more price variability per unit of reduced unemployment variability.

The choice of a point on the frontier is a matter of politics and social preferences, about which economists have little to say except as citizens. My principal message is to point out the existence of the frontier and to stress that it takes a coherent monetary strategy to get to the frontier. The policy of the past decades put us far above the frontier, with substantially more unemployment variability and almost infinitely more price variability than a point in the middle of the frontier in Figure 1.

Elastic price targeting and the policy frontier

Monetary strategies based on elastic price targets have a close relation to the policy frontier:

FIGURE 1
The Policy Frontier



Note: The policy frontier shows the most favorable combinations of unemployment and price variability. The horizontal axis is the standard deviation of the unemployment rate, in percent, and the vertical axis is the standard deviation of the percent departure of the price level from target. Three points on the frontier are derived by simulation in the next section.

Economic structure and the execution of policy

Two important relationships govern the policy frontier. The first is the aggregate demand schedule that controls the influence of monetary policy

on real activity. There is about a one-year lag before monetary expansion lowers unemployment reliably. I will also assume that policymakers know roughly how much money growth is needed to lower unemployment by one percentage point over the year starting a year after the growth occurs. Another important aspect of aggregate demand is the predictability of unemployment a year forward. Errors in forecasting will generate errors in achieving the elastic target, which have implications for the amount of unemployment and price variability.

The second important relationship is the price adjustment process, or Phillips curve. More economic slack, as indicated by higher unemployment, depresses inflation. The slope of the Phillips curve is a critical parameter for the policy frontier—the lower the slope, the farther the frontier is from the origin. Unresponsive inflation means that more unemployment must be incurred to get prices back on target after a shock. I take the slope of the Phillips curve to be about one half percentage point of reduced inflation, in the course of a year, for a one percentage point increase in unemployment, maintained for a year. This slope is in line with recent empirical estimates for the U.S.

The Phillips curve is perturbed from time to time by inflationary shocks. Occasionally, wages rise more than labor market conditions would normally warrant, and prices rise by more than indicated by the Phillips curve. More important, however, is the increase in inflation associated with jumps in oil prices and in other determinants of the overall price level. These shocks are critical for the design of monetary strategy. More than anything else, the strategy must be formulated to deal intelligently with the burst of inflation and higher unemployment set off by each shock. Although the two oil shocks of the 1970s are the most conspicuous inflationary disturbances of the postwar period, other shocks, positive and negative, occurred as well, and there is every reason to think that new shocks will continue to complicate monetary policy in the future.

Subject to these two important relationships, monetary policy operates according to the strategy of the elastic price target. Specifically, the goal of policy is to influence prices and unemployment so that the price level, p, is as close as possible to the elastic target. The percentage departure of the price level, p, from its ultimate target, p*, is an elasticity A times the departure of unemployment from its nominal level of 6 percent:

$$100\,(p - p^*)/p^* = A \cdot (u - 6)$$

The Fed's operating procedures under the elastic price target

It is neither practical nor desirable to dictate to the Fed exactly how it should proceed under the elastic targeting strategy. Rather, Congress's

instructions to the Fed should emphasize the result: close achievement of the elastic target. As financial markets evolve and the Fed learns how best to operate to achieve the target, procedures will change and performance will improve.

I think the Fed's internal procedure would proceed in the following way: Each month, it should formulate a quarterly forecast for the forthcoming two years. The forecasts should combine the results of formal models with the judgments of experienced forecasters. Reliable outside forecasts should receive some weight as well.

With the forecast in hand, the Fed should examine the one-year period starting two quarters in the future. For example, in August the next calendar year should constitute the criterion period; in April, it should be the twelve months starting in October, and so on. The forecasts for the price level and unemployment in the criterion period should be compared to the elastic target. If the forecast price level exceeds the target as adjusted by the forecast unemployment rate, then policy should be tightened. If the outlook is for a price level below target, policy should be turned expansive. After policy is changed, new forecasts should be prepared and the elastic price target checked again for the criterion period. The forecasting-policy resetting exercise should be continued until the elastic target is satisfied exactly in the forecast for the criterion period.

Although the elastic target is stated in terms of the price level, it is likely that the changes that occur as policy is shifted are more in forecast unemployment than in the forecast price level. For example, with an elasticity of 5, if the forecast price level is 338, 2.4 percent above the target of 330, and the fore°cast unemployment rate is 6.2 percent, the price level is 1.4 percent above where it should be according to the elastic target (five times the unemployment gap is 1.0 percent, as against an actual price gap of 2.4 percent). Projected policy might then be changed by lowering reserves by 0.6 percent, which would translate into an increase in forecast unemployment of 0.26 percentage points and a decrease in the forecast price level of 0.1 percent. The new forecast is right on target—the price level is now forecast at 2.3 percent over the ultimate target while unemployment is 0.46 percentage points over 6 percent, and 2.3 is five times 0.46.

How policy influences the price level and unemployment

The policy moves needed to keep on target should be made fairly quickly. It takes about a year for monetary policy to have its strongest impact on unemployment and even longer for the price level. Over the one-year span, both variables in the elastic target are controllable by monetary

policy, so it is reasonable to ask policy to achieve the target in terms of a forecast a year ahead. Economists disagree over the relative influence of monetary policy on the two variables, but agree strongly that one or the other is strongly controllable a year hence. One of the great virtues of the elastic price target as a monetary strategy is that its effectiveness is agreed upon by all major schools of thought.

With respect to the price level, monetary policy acts quickly and effectively on certain types of prices, but slowly on others. Auction prices of raw materials decline immediately when monetary contraction brings higher interest rates. More importantly, monetary contraction causes the dollar to appreciate against other currencies, which immediately lowers the dollar prices of many goods traded in world markets. Monetary control of prices of tradeables holds both for imports and for some types of exports. With a longer lag, monetary policy influences wages and therefore prices throughout the economy.

Monetary influence over the unemployment rate is an important feature of Keynesian economics and is agreed upon by the great majority of practical macroeconomists. The influence builds to a peak about a year after a policy move and then subsides to zero. Monetary policy cannot influence the average level of unemployment in the long run. But in the short run, a monetary contraction raises interest rates and depresses investment demand for housing, plant and equipment, and consumer durables. Employment in construction and durables declines and unemployment rises throughout the labor market. In addition, higher interest rates cause dollar appreciation; higher U.S. prices to the rest of the world and lower import prices to the U.S. divert demand away from U.S. producers and so raise unemployment through another channel.

As a general matter, monetary policy is entirely capable of pushing the economy in the direction necessary to achieve the elastic price target. Moreover, this conclusion holds if Keynesian economists are right that wages and prices are sticky and it holds equally if prices are fluid and move quickly to clear markets. The conclusion is also strongly supported by the forecasting models in use in the Fed today.

Choice of the monetary policy instrument

I have avoided taking a position on exactly how the Fed should carry out each month's monetary policy; this is a question of tactics more than strategy. Any reasonable choice of policy instrument is compatible with the strategy of adjusting the instrument as necessary to make the forecast

price and unemployment levels satisfy the elastic target in the forecast. In current monetary institutions, the choices are

- A short-term interest rate
- Reserves
- The monetary base

All of these are directly under the Fed's control, in that simple operating instructions for the open market desk can achieve the agreed upon level of the instrument without any error. A broader monetary aggregate like M1 cannot serve as a policy instrument for it is not directly under the Fed's control.

The advantage of using the interest rate as an instrument is well known: Unexpected shifts in the demands for reserves and currency are automatically offset and have no disturbing effect on the rest of the economy. During the financial transition of the early 1980s, there was much to be said for an interest-rate instrument. Moreover, the interest-rate instrument overcomes the troublesome problem of seasonal variations in reserve and currency demand. But the use of the interest rate increases the sensitivity of the economy to disturbances in spending. Because the interest rate would not rise automatically when consumption, investment, or other types of spending rose, the stabilizing effect of interest-rate fluctuations would be lost. A greater burden would fall on the forecasting and policy adjustment process at the Fed to respond to spending shifts.

Choosing reserves as the instrument would reverse the situation. Disturbances in spending would be cushioned by interest rates, but shifts in demand for reserves caused by movements of depositors among accounts with difference reserve requirements would be propagated into the overall economy. Then the forecasting and policy adjustment process would have to pay close attention to these shifts. The prospect for future destabilizing shifts is substantial, as only a thin line separates accounts with 12 percent reserve requirements from those with 3 percent or zero.

The monetary base is probably the least desirable instrument. The demand for currency is probably even more erratic than is the demand for reserves.

How close should we expect the Fed to come to meeting the elastic price target?

Under the operating procedure I have proposed, the Fed would concentrate on meeting the elastic price target prospectively over the forthcoming

year starting in about two quarters. The current quarter and the next quarter would be water under the bridge so far as monetary policy was concerned. Naturally, surprises would occur that would make the Fed's forecast incorrect and cause it to miss its target. Because the Fed could label any policy failure as a forecasting error created by a surprise occurring too late to be offset by policy, Congress and the public need some sense of the likely magnitude of reasonable departures from the target.

Because of the forecasting step in the policy strategy, it is simple to state as a general matter how large the mistake should be in achieving the elastic target: The departure from the elastic target should be no larger than the errors in forecasts in the price level and unemployment made one year in advance. Specifically, the number of percentage points by which the price level departs from the elastic target should be equal to the percent error in the year-ahead price level forecast plus the elasticity, **A,** times the percentage-point error in the unemployment forecast.

If the Fed is consistently missing the elastic price target by more than the forecasting errors of good outside forecasters, then policy is not working properly. Or, to put it another way, if the Fed's forecast, which always says that the elastic target will be achieved in the forthcoming year, is consistently different from outside forecasts, and the outside forecasts are more often right, then the Fed is not carrying out its job appropriately.

Congressional review of monetary policy ought to proceed as follows. Every six months, the Fed should present its forecast for the year starting two quarters later. At the same hearing, outside forecasters should testify about the outlook for the same period. If the outsiders systematically agree that the Fed will probably miss the elastic target, then the Fed would be called back to explain the discrepancy. Because the Fed is better informed about monetary policy (a key determinant of the outlook), it is possible that its forecasts will be consistently superior to other forecasts. For this reason, it should not be a requirement that the consensus forecasts always satisfy the elastic price target exactly.

The policy frontier for the postwar U.S. economy

Suppose the Fed faithfully carries out the forecasting-policy adjustment process recommended in this paper, so that an honest forecast always has the elastic price target satisfied exactly in the forthcoming year. The effect of that policy is to make the economy roll with the punch from both inflationary shocks and errors in forecasting demand. Of the two sources of disturbances, it is inflationary shocks that cause the more significant problems for monetary policy. To keep the story simple, I will describe how the

strategy of the elastic price target handles the response to an anticipated inflation shock. The story is not very different if the shock is a surprise; it only takes longer for policy to start its gradual response.

The immediate effect of an upward inflation shock is to raise both unemployment and the price level. Consider a shock that would raise the price level by 1 percent if unemployment remained constant. Because of the response of policy, the shift raises unemployment by $1/(A + 0.5)$ percentage points. The 0.5 is the slope of the Phillips curve. Because A is in the denominator, the higher is A, the lower is the jump in unemployment. For example, if $A = 3$ (roughly nominal GNP targeting) then there will be 0.29 extra percentage points of unemployment per percent of price shock, but if $A = 8$, the increase is only 0.12 extra percentage points of unemployment. In later years, the bulge in unemployment subsides at a rate of 0.51 $(A + 0.5)$ percent per year. With $A = 3$, the rate of decay is 14 percent per year; with $A = 8$, it is 6 percent per year.

Because the policy response to an inflation shock is to raise unemployment to counteract the inflation, the actual increase in the price level is less than the shock. However, reasonable policies let the price level absorb the great bulk of a shock. A 1 percent price shock raises the price level by $A/(A+0.5)$ percent. With $A = 3$, this is 86 percent; with $A = 8$, it is 94 percent. The price level rises by less than the amount of the shock because of the deflationary effect of the increase in unemployment that goes with the shock. The bulge in the price level disappears over time at the same rate as does the bulge in unemployment.

The postwar era under the elastic price target strategy

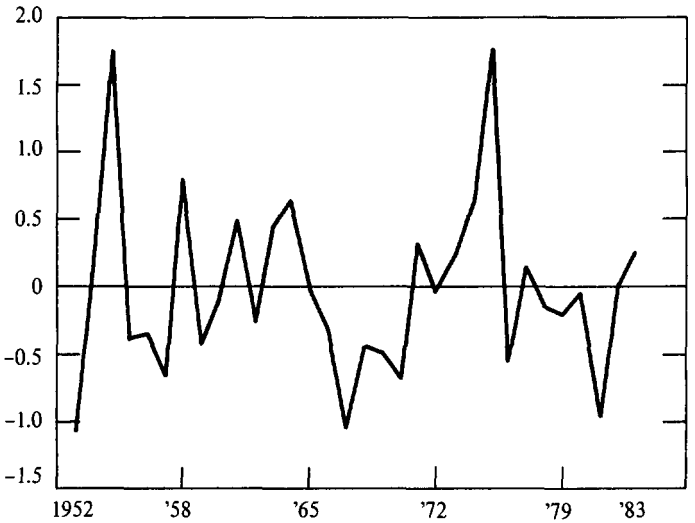
A monetary strategy based on an elastic price target would have delivered unambiguously better performance over the past 30 years than did actual policy. Unemployment variability could have been substantially less, and price variability could have been vastly less under an elastic price target for ***any*** reasonable elasticity, including nominal GNP targeting.

The first step in demonstrating this proposition is to isolate the aggregate demand forecasting errors, the price shocks, and the errors in forecasting the price shocks. For the AD forecasting errors, I ran a simple annual forecasting equation for the unemployment rate, with lagged unemployment, prices, monetary base, and interest rates as predictors. The residuals from this regression, shown in Figure 2, are representative of the forecast errors that would have been made under the process described earlier in the paper. Each recession shows up as a spike in the figure—neither this equation nor experienced forecasters are able to call the sharp increase in

unemployment that occurs in the typical recession. Notable also in Figure 2 is the prolonged period of negative forecast errors for unemployment in the mid-1960s.

FIGURE 2
Forecasting Errors for the Unemployment Rate

Percentage points of unemployment



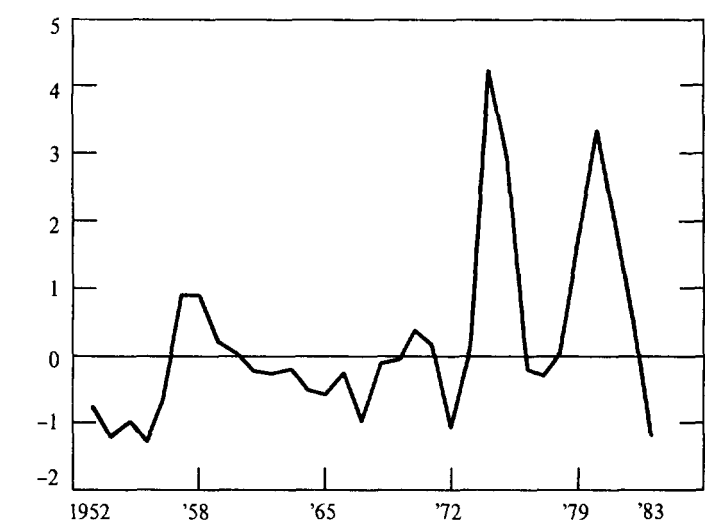
Note: Because the unemployment rate cannot be forecast perfectly accurately, even the best policy involves deviations from the elastic target. These deviations also feed into the way prices depart from the long-run target and the way that unemployment departs from its normal level of 6 percent.

Derivation of inflation shocks is a trickier issue. Most economists subscribe to the view that once inflation becomes established in the economy at a certain level, the Phillips curve shifts upward by the amount of the established inflation. Under an elastic price target, or any other sensible strategy for price stabilization, established inflation is unlikely to develop, since the public will come to have faith in monetary policy's ability to keep average inflation at zero. But to extract estimates of year-by-year inflation shocks from the actual historical data from a period of mistaken policy, some account must be taken of the growth of established inflation during the postwar period. I will estimate the shocks by subtracting the component of inflation attributable to demand and the amount of established inflation from actual inflation.

By calling almost every movement in inflation a change in established inflation, inflation shocks can be made to seem minimal. Because my purpose here is to show that elastic price targeting can handle large price shocks, I want to avoid any procedures that might understate the historical shocks. My estimates of established inflation are accordingly conservative. For the period of generally low inflation from 1948 through 1965, I took established inflation to be its average for those years, 1.5 percent per year. For the period of inflationary policy, 1966 through 1978, I took it to rise in equal increments from 1.5 percent to 6.8 percent, its value in 1978. For 1979 through 1983, I took established inflation to be at the constant level of 6.8 percent.

Figure 3 shows the estimates of inflation shocks obtained by subtracting this estimate of established inflation from actual inflation and also taking out the effects of demand by adding 0.5 (u-6). The most salient features are the two sharp spikes for the oil price shocks of 1974 and 1979-80. Other positive shocks occurred in 1957-58 and 1970-71. Negative shocks occurred in 1952-56 and 1972 (probably the effect of price controls). Figure 3 is no more than a good guess about the price shocks that would have

FIGURE 3
Inflation Shocks

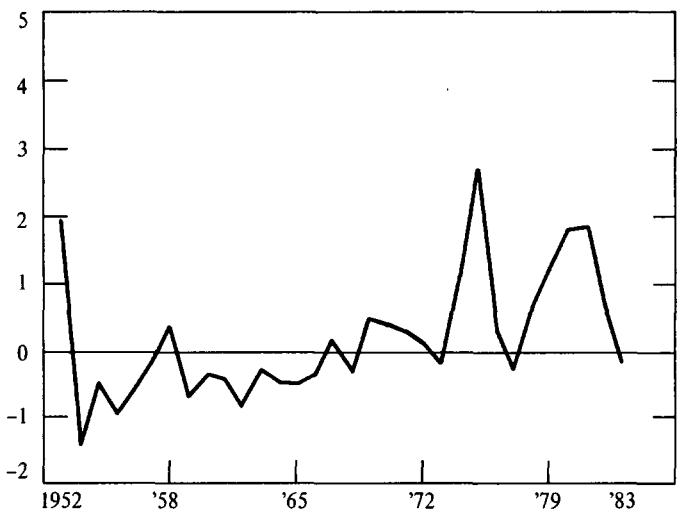


Note: The economy does not track the Phillips curve exactly. The two biggest departures occurred in 1974 and 1979-80 when oil prices rose sharply. It is the shocks themselves, not just the surprise part, that create most of the problem for stabilization policy.

occurred under a policy of price stabilization. However, the results of this paper are not sensitive to the precise series used for the price shocks.

Prediction errors for the price level also figure in the errors in achieving the elastic price target, but they are subsidiary if the elasticity is at all high, simply because errors in unemployment are multiplied by the elasticity but those for the price level are not. To get a feel for the predictability of the price shocks I regressed my series for the shocks against the same list of lagged predictors that I used for the unemployment rate. Only the lagged price level turned out to have predictive power; it explains just under half of the variance of the price shock. Figure 4 shows its predictions for the postwar period. The prediction errors for the price level are the difference between the actual price shock and the predicted price shock minus the slope of the Phillips curve times the prediction error in the unemployment rate.

FIGURE 4
Predictions of Inflation Shocks



Note: When an inflation shock is predicted, policy can start to respond to it sooner. About half of the variability of the inflation shocks in Figure 3 are predicted here. However, even perfect prediction of the shocks would not dramatically improve performance.

What would have happened under the elastic price target

My simulations of the U.S. economy under the elastic price target strategy assume that policy achieved the elastic target except for the forecast

errors just derived. Because these errors are based on crude annual equations, they are not a stringent standard of performance. Actual policy probably could have done quite a bit better. Of course, my simulations have to assume that the forecast errors occurred because of exogenous shocks to the economy, and that these shocks would have been the same under the proposed monetary strategy as they were under the actual strategy. I think this assumption is a reasonable approximation. It is wishful thinking to assert that events like the oil price shocks would not have occurred under a superior U.S. monetary policy.

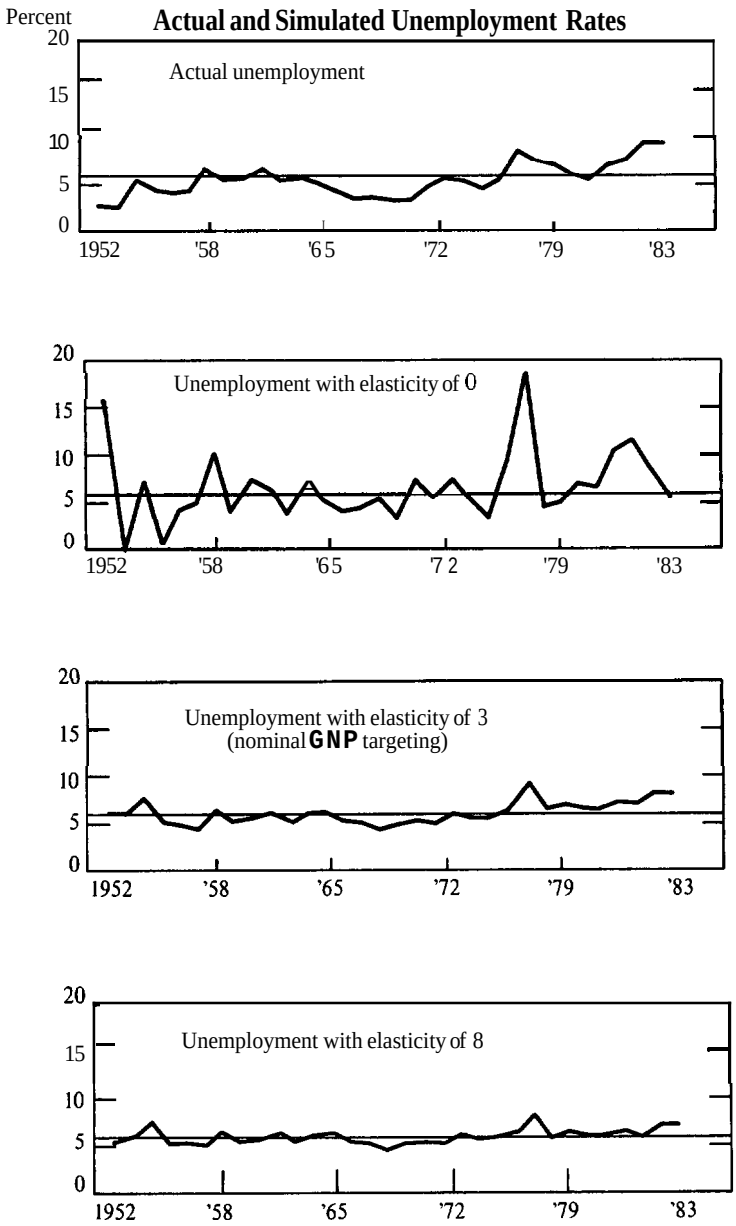
Aside from the forecast errors that brought departures from the elastic target, the only other property of the U.S. economy necessary to know for the simulations is the slope of the Phillips curve. The effect of the strategy is to keep unemployment above 6 percent (except for random forecasting errors) whenever the price level is above target and below 6 percent when it is below target. When unemployment is consistently above 6 percent, there is downward pressure on prices as the Phillips curve does its job. Gradually, the price level returns to its ultimate target. As it does so, unemployment must also approach 6 percent, through the operation of monetary policy and the elastic target.

In the simulations, the gradual return to the long-run target is not generally visible, because new shocks constantly push the economy away from the target. What is visible, however, is the tendency for the price level to stay near the target and for the unemployment rate to stay near 6 percent in spite of the battering of the economy by random shocks. Even though prices are quite sticky and policy is very gingerly about getting prices back to target by incurring excess unemployment, the price level stays much, much closer to constancy in even the most dovish of the simulations than it actually did over the postwar period. Most remarkably, the variability of unemployment is also considerably less, even though the policy is much more successful in stabilizing prices.

Figure 5 shows the simulated unemployment rates under various regimes. At the top is the actual unemployment rate. The horizontal line marks the 6 percent rate I take as the normal amount of unemployment. The plot shows the basic defect of postwar policy—unemployment was pushed too low in the 1960s so that it had to be held far above 6 percent in the 1970s and early 1980s. The combination gave much too much unemployment variability.

The second panel in Figure 5 shows how unemployment would have behaved had monetary policy been dedicated single-mindedly to price stabilization. Wild swings in policy would have brought extreme variation to

FIGURE 5
Actual and Simulated Unemployment Rates



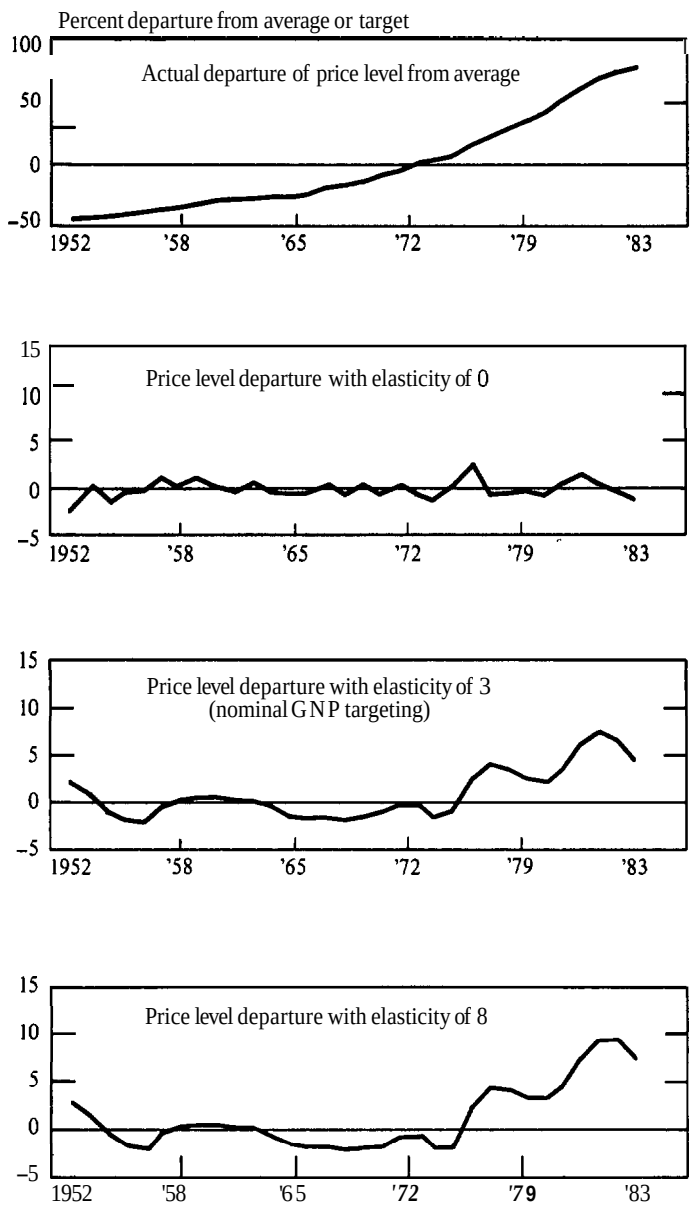
unemployment. Unemployment would have briefly reached true depression levels during the two oil price shocks. This plot shows vividly the dangers of a hawkish policy in an economy with sticky prices. Advocates of pure price stabilization must be very confident that the price adjustment process is much quicker than the one assumed in this simulation.

The two bottom panels show how the unemployment rate would have evolved under two variants of the elastic price standard. The one with an elasticity of 3 is a close approximation to nominal GNP targeting. Movements in unemployment are similar to the ones that actually occurred, but with smaller amplitude. The policy would have made the mistake of too low unemployment in the 1960s, thanks to a sequence of surprises about aggregate demand, but the mistake would have been much smaller. The burst of unemployment in 1975 would have been worse under targeting with an elasticity of 3 than it was actually (9.4 percent as against 8.5 percent). The years 1974 and 1975 saw the confluence of the largest inflation shock of the postwar period (4.2 percent in 1974) and the largest demand forecasting error (1.8 percentage points of unemployment in 1975). On the other hand, nominal GNP targeting would have given lower unemployment in 1982-83 than actually occurred. Responding to the second oil shock was less difficult because it was not accompanied by a big positive demand forecasting error. Further, sensible policy, as expressed by the elastic price strategy, would not have been struggling against the high inflation that actually occurred in 1979-82.

The unemployment record with an elasticity of 8 is quite a bit better than under nominal GNP targeting. In the worst year, 1975, unemployment would have risen only to 8.4 percent. The prolonged period of high unemployment from 1976 onward that actually occurred, and would also have occurred under nominal GNP targeting, would have been largely eliminated with the higher elasticity.

Figure 6 shows actual and simulated price levels. The top panel is the actual path of the price level from 1952 to 1983. The departures from constancy are so large that this panel has to have a different scale from the others. The next panel shows that an aggressive price stabilization policy would have kept the price level close to constant. The worst departure would have been in 1974, 2.7 percent over target. This and the other oil price shock would have been extinguished immediately through the use of monetary policy so constrictive as to return the price level back to target the very next year. Under the price stabilization policy, 1975 would have been a year of deflation, not of inflation.

FIGURE 6
Actual and Simulated Departures of Price Level from Target

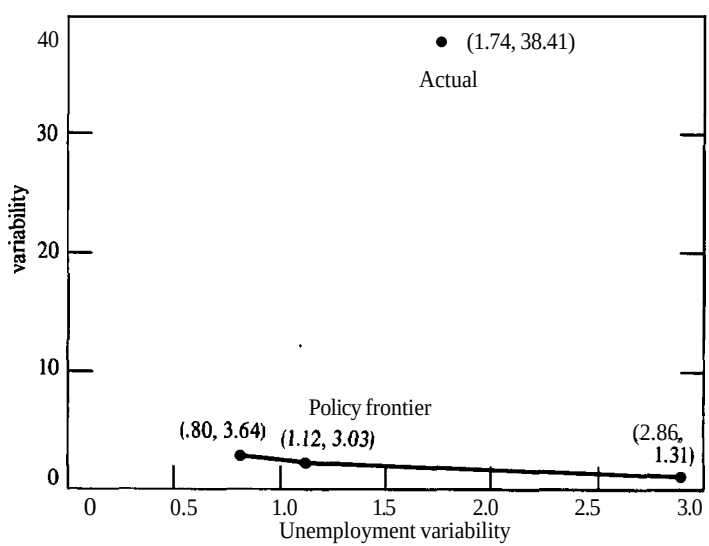


The bottom two panels of Figure 6 show the implications for the price level of the elastic price target strategy. With an elasticity of **3,** price would have remained close to the constant target level until 1974. Under the first oil shock, the price level would have risen to 3 percent over target in 1974, peaked at **4.2** percent over in 1975, and then begun a gentle decline. The process would have been interrupted by the second shock, which pushed prices to 6.2 percent over target in 1980 and to a peak of 7.6 percent over target in 1981. Then a new decline would have begun, taking the price level to only 4.7 percent over target in 1983.

The price story with an elasticity of 8 is much the same, except that the swings have greater amplitude. The price level would have peaked at 9.7 percent over target in 1981 and would have reached 7.8 percent over target in 1983.

What I want to stress most about these simulations is the superiority of either of the elastic strategies to actual policy. Figure 7 shows dramatically how completely perverse actual policy was. The policy frontier plots the standard deviations of unemployment and the price level for the three policies simulated in Figures 5 and 6. They are the same points shown in Figure 1, but here the scales are changed in order to accommodate another point, labeled actual, which shows the actual standard deviations of unem-

FIGURE 7
The Policy Frontier and Actual Economic Performance, 1952-83



Note: Actual policy brought more unemployment variability and vastly more price variability than necessary.

ployment and the price level. The actual point is far, far above the frontier. Actual policy did not make sense by any set of preferences about unemployment and price variability. In particular, the two elastic strategies *dominate* actual policy, in the sense of offering both lower unemployment variability and much lower price variability.

Conclusions

What is most important about monetary strategy is to have one. Any policy on the frontier of unemployment and price variability that is not fiercely hawkish will give better performance by far than we had under the meandering policy of the past 30 years.

Nominal GNP targeting is one policy on the frontier. With some justice, it has been criticized as overly hawkish, in that it calls for substantial unemployment in an aggressive response to an inflation shock. An elastic price target with an elasticity of 5 or 8 strikes me as closer to the optimum. But this paper has shown that differences among sensible policies are small compared to the difference between historical policy and any sensible policy.

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Commentary

Raymond Lombra

In the two years since we last convened in this majestic setting to discuss monetary policy, real output has expanded rapidly and inflation has slowed significantly. However reluctant policymakers are to take credit for the economy's exceptional performance and pat themselves on the back in public, the critique presented by Bob Hall must come close to provoking a response. Serving as a force for moderation and so as not to foster the widespread notion that economists rarely agree on anything, especially policy issues, my plan is to focus on the core of Bob's paper, around which I believe most economists and policymakers may be able to rally. Moreover, leaving most of the technical nitpicks to Stigler's conference handbook should help engender a constructive dialogue more in concert with the intoxicating beauty and dignity of our surroundings.

The economy's evolution over the past two years has proceeded within a policy strategy often characterized as "pragmatic, eclectic, and flexible." However well such a strategy appears to have worked, many, including the farsighted prime movers behind this conference—Roger Guffey and Thomas E. Davis—have become increasingly concerned about the absence of a reliable, strong, well-understood ***anchor*** for policy. Skilled sailors know that reliable anchoring entails good holding ground, proper equipment, and informed technique. Designed to absorb the shocks of winds and currents as they change direction and velocity, an essential ingredient of successful anchoring is adequate "scope"—the ratio of anchor line to the depth of the water. The lower the ratio (i.e., the less scope), the tighter is the tether linking the boat and the anchor. Although quite serviceable in calm waters, such a configuration is not very tolerant of shocks. As a result, the anchor can easily slip or break loose, becoming dysfunctional. In contrast, adequate scope builds in sufficient flexibility to absorb shocks. At the other extreme, a huge ratio (i.e. very large scope) comes to

approximate a vessel drifting aimlessly. The message of this analogy, and indeed of Hall's paper, is that a policy anchor and a policy strategy with a moderate degree of built-in flexibility are not mutually exclusive.

There are four general characteristics of Hall's specific policy proposal that I would like to highlight and discuss.

Precommitment

An increasing number of academics agree in principle with the notion that policymakers should announce a specific, credible, understandable, defensible trajectory for monetary policy covering the short to intermediate term (say, six months to two years). Differences do, of course, exist concerning the specifics of such a strategy—e.g., which variable(s) to focus on, how frequently to review the policy stance, the necessary and sufficient conditions for revising policy, etc. Without down-playing the importance of such nuances, these differences should not be allowed to obscure the agreement regarding precommitment.

Although exhibiting a superficial attachment to precommitment, as exemplified by the Fed's twice-yearly policy dances with Congress under the aegis of the reporting requirements embedded in the Humphrey-Hawkins Act, most policymakers view meaningful precornmitment as economically and politically naive, and possibly even injurious to the nation's economic performance. Trumpeting the overriding need for judgement, a flexible, pragmatic, and eclectic—that is, sensible—policy allegedly emerges. Manifested by ever-changing emphases accorded the various monetary aggregates; changes in the relevant bases, ranges, and definitions; and shift-adjustments, few would confuse the Fed's approach with the type of precommitment advocated by Hall and others.

We live in an uncertain world; on this we all presumably agree. And, as many have said, the future is unknowable but not unimaginable. Yet, as the past 20 years so vividly demonstrate, and as Brunner and Meltzer have forcefully argued, we should be profoundly humble about our ability to distinguish between, much less anticipate, permanent and transitory shocks to the economy.

While I have an abiding respect for the work of Steve Axilrod, his large and talented staff, and indeed for the staffs throughout the Federal Reserve System, the Fed's flexible approach to policy is predicated on a degree of confidence in their collective abilities to sort things out—a confidence that, in my judgement, is not wholly justified. Moreover, the alleged short-run economic benefits of flexibility, which are almost by definition transitory, need to be weighed against the long-run costs. Policy adjustments,

reversals, and errors erode credibility and complicate intertemporal decision-making in the private sector. At a deeper level, Fed attempts to reoptimize at each month's FOMC meeting must face rather than finesse the problem of "dynamic (time)inconsistency" first discussed by Kydland and Prescott (1977), and now immortalized by Rick Mishkin's two-year-old son.

By viewing flexibility as diametrically opposed to precommitment, it can be argued that the Fed overestimates the economic benefits and underestimates the economic costs of its pragmatism. As economists, however, we should not underestimate the political benefits generated by the Fed's vague, incomplete strategy (Lombra [1984]); precommitment and specificity go hand in hand with enhanced accountability for principals and their agents! The short-run political shock absorber comprising current arrangements and the inevitable tension between political and economic forces go a long way toward explaining the gulf separating many economists from policymakers on the notion of precommitment.

Focus on nominal magnitudes

Two propositions underlie recommendations that monetary policy should focus on nominal magnitudes. First, the longer the run, the larger the price effects of policy actions and the smaller the real effects. Second, the ability of economists to forecast the short-run effects of particular policy actions on real output, employment, and prices is limited. Thus, with policy approximately neutral in the long run and central bank independence supposedly providing a shield permitting policymakers to take the long view in conducting policy, a focus on nominal magnitudes—on price stability, to be precise—is advanced as appropriate, prudent, and welfare-enhancing.

The profession's forecasting performance has been chronicled and analyzed in a series of important articles by McNees. Table I extracts some data from his most recent evaluation (McNeesand Ries [1983]).

The size of the mean absolute errors and root mean square errors appear nonnegligible. Moreover, the mean error measure, an indicator of bias, suggests that while the forecasts of nominal GNP are on the mark on average, this reflects a tendency to overestimate real output and underestimate inflation. Such indications, which are broadly consistent with similar evaluations of the Fed staffs forecasts (Lombra and Moran [1980], Karamouzis and Lombra [1984]), suggest that attempts to pin down the slope and position of the short-run Phillips curve and handle expectations adequately have not been wholly successful. If a Hall-like proposal can be shown to be

TABLE 1

One-Year-Ahead Forecast Errors
1971-83

	Error Measure		
Variable	Mean Error	Mean Absolute Error	Root Mean Square Error
Nominal GNP	0.0	2.2	2.8
Real GNP	0.5	1.6	2.1
Implicit Price Deflator	-0.7	1.4	1.8
Unemployment Rate	-0.3	0.7	0.9

Notes: From McNees and Ries (1983), Table 3. Error measures are calculated from the median of forecasts by the ASA-NBER survey, Chase, DRI, Wharton, and BEA. Errors for the first three variables are calculated as predicted minus actual growth at compound rates. Unemployment rate errors are the difference between predicted and actual unemployment rate levels.

flexible enough to handle such forecasting difficulties, the fact that it gives primary emphasis to nominal magnitudes, and should have a salutary effect on expectations suggests it dominates alternative strategies predicated on estimated empirical relationships between policy instruments and real variables which, in the language of Leamer (1983), are characterized by whimsy and fragility.

A forward-looking policy

Hall's proposal conditions policy on forecasts for unemployment and the price level a year ahead. Current outcomes and expected outcomes over the next six months are treated as water under the bridge. Existing empirical work (Lombra and Moran [1980]) and my experience within the system suggest the formulation of policy has often taken almost the opposite tack: incoming data on *past* outcomes drive policy discussions and adjustments. To be sure, discussions do include simplistic extrapolations of trend-cycle indications in the data, with a dash of regression to the mean experience thrown in, as lip service is paid to the staff's forecasts. However, the perception that short-run forecasts are unreliable precipitates heavy discounting and an overriding focus on current conditions.

Many, myself included, have long felt that a policy that is in many respects backward-looking will often prove unduly procyclical. It is certainly true that short-run forecast errors are not small and that forecasts tend to deteriorate as we move from a one-quarter to a four-quarter horizon. However, the overwhelming portion of forecast errors is usually concentrated

in the first two quarters, and little further deterioration is evident over a four- to eight-quarter interval (McNees and Ries [1983], Table 2). In all likelihood, the dynamic behavior of prices and output, discussed above, contributes to longer-run forecasts being in some sense more reliable than short-run forecasts; the longer the run, the more prices will have adjusted. Even more fundamentally, the fact that forecasts are almost always wrong does not imply that they contain no usable information and that policymakers are free to ignore them. Of course, with high discount rates in the political arena reinforcing the existing economic uncertainties concerning the near-term outlook and the short-run transmission mechanism for monetary policy, such behavior is hardly surprising.

The pitfalls of Fed-style pragmatism

Has the Fed moderated, aggravated, or initiated economic fluctuations? The never-ending character of this debate and the intransigence of relative positions on the role of monetary policy testify to the limitations of our analytical and empirical tools and offer strong support for Keynes' dictum that in economics it is virtually impossible to ***convict*** someone of error, and extremely difficult to ***convince*** someone of error.

The Fed sees policy as a stabilizing force, more often than not deftly responding to emerging disturbances. Many academics, particularly monetarists, see policy as often aggravating economic fluctuations. Accusing policymakers of being deaf rather than deft, and suffering from both myopia and amnesia, Fed bashing has seldom been in short supply.

The correlation between the Fed's plans and its performance has, in my judgement, been variable, difficult to predict, and not particularly high on average. Moreover, policymakers strain credulity beyond reasonable limits by contending that virtually all departures of the record from the rhetoric result from bad luck, fiscal policy, unanticipated nonpolicy shocks, financial innovation, and the like. While Hall's Figure 7 and accompanying discussion surely exaggerate the degree to which policy has exacerbated economic fluctuations, it does appear that, despite good intentions, Fed flexibility and pragmatism often produce policies that become part of the problem rather than the solution.

I recognize that what looks like a policy error ex post from an economic perspective might have looked quite different ex ante. I would also contend that the last 20 years have seen their share of successful policy episodes. Further, it must be granted that alleged economic policy errors have occasionally been associated with short-run political successes. However, assuming a high discount rate and a multi-dimensional objective function

defined over political and economic outcomes, inattention to the longer-run economic effects of policy emerges naturally. The result is a focus on the short run, wherein knowledge deficiencies about the transmission mechanism and the source and duration of shocks are particularly acute.

That the absence of an anchor for policy may be a part of the problem can probably only be seen by standing back from the day-to-day fire fights that permeate policymaking. I have long felt that Reserve Bank presidents and their staffs, being somewhat less involved in shorter-run policy operations, have displayed a comparative advantage in gaining a perspective on policy; over the years many have asked, What precisely are we doing? How are we doing it? And is there a better way? In this spirit, the collective wisdom advanced during this conference raises fundamental as opposed to technical questions about the conduct of policy. Fed bashing aside, I doubt the current configuration of the Bluebook and Greenbook and the accompanying policy strategy in place are an adequate response to such questions.

Lest I be accused of being too easy on the author, let me make some specific observations and suggestions motivated by reading Bob's provocative paper. While I am not sure how seriously to take some of the details, I accept and am sympathetic to the spirit of the exercise he conducts. However, it was startling to read a paper written in 1984 where price and policy expectations are not prominent. Wouldn't the Fed's choice of parameter A effect the economy's wagelprice-setting mechanism? One need not buy short-run neutrality to believe the system depends on the policy rule. In the empirical section, I would counsel against relying too heavily on results generated by what Bob Weintraub used to call a Tinker-Toy model. Why not utilize the one-year-ahead forecasts provided by McNees and Ries (1983), and the errors and biases embedded therein, to put the elastic price standard through its paces? Although still vulnerable to a modified Lucas critique, the results would be less model-dependent and somewhat more realistic. Lastly, I wonder how to treat fiscal policy within such an exercise. Is it reasonable to assume fiscal policy will be invariant to the stance of monetary policy? I think not, and suggest the implications of such an interdependence for the variance of the price level and unemployment need to be explored.

Constrained optimization is what policymaking is all about. Logically, then, we need to be fairly precise about the nature of the constraints and the objectives if we are to produce useful policy evaluations and prescription. Reflection and research on such issues suggest to me that, specifics

aside, approaches like those advanced by Hall that are predicated on pre-commitment are forward-looking, and focus on nominal magnitudes go a long way toward avoiding the pitfalls of Fed pragmatism. It is often said that sailing is like standing in a cold shower and tearing up $20 bills. It strikes me that an inflationary and periodically destabilizing monetary policy can also be so characterized.

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7
The Value of Intermediate Targets in Implementing Monetary Policy

Benjamin M. Friedman

Despite the growing experience with their use, both in the United States and abroad, the role of intermediate targets of monetary policy remains a source of confusion and controversy. Although some advocates apparently regard stable growth of one or another monetary aggregate as an end in itself, by far the more typical view in favor of such intermediate targets is that they somehow enable the central bank to achieve more effectively its objectives for the nonfinancial economy, usually including price stability or real growth, or both. It is in making that 'somehow" more precise, and thereby making the appropriate role (if any) of intermediate targets operational, that the difficulty lies.

The ambiguity stems from the fact that measures like money or credit are not under the immediate control of the central bank. In the United States, the deposits that constitute the main bulk of any of the familiar monetary aggregates are created by more than 40,000 financial institutions, and how much money there is at any time depends on the decisions not only of these institutions but of millions of individuals and businesses that own deposits. Broader asset aggregates like total liquid assets depend on the decisions of an even wider range of institutions, as do liability aggregates like domestic nonfinancial credit. The Federal Reserve System can influence any of these measures, to be sure, but it cannot directly control them in the sense that it can control, for example, the nonborrowed

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reserve base or the federal funds rate. Hence these measures are at most targets, not instruments, of monetary policy—intermediate steps between the instruments that the central bank can control directly and its ultimate nonfinancial policy targets.

The object of this paper is to assess quantitatively the potential value of specific intermediate targets for monetary policy in the United States. The basic premise motivating this analysis is that a financial variable like money or credit—or, for that matter, a market interest rate—has potential value as an intermediate monetary policy target only to the extent that movements in that variable convey information about the nonfinancial economic developments that constitute the reason for having a monetary policy in the first place. Moreover, to warrant such a variable's use as an intermediate target, the pertinent information its movements contain must not be readily available elsewhere. The questions addressed in this paper are whether any familiar financial variables in fact contain such potentially valuable information and, if so, which ones and how much.

In addition to the specific conclusions provided as answers to these questions, a key contribution of this paper is the method of analysis it introduces. In particular, the paper suggests and implements a method for using structural economic models, restricted by the relevant economic theory, to answer questions that the previous literature has addressed primarily with nonstructural, unrestricted representations of economic behavior. The specific quantitative conclusions reached in this paper about the potential value of intermediate targets in the monetary policy process result from the application of this method to one macroeconometric model that is especially small and simple. The method of analysis suggested here, however, is applicable more generally, to models small and large, simple and complex.

The first section outlines the basic concept of the intermediate target as a way of gathering and processing relevant information in implementing monetary policy. The next section presents the small macroeconometric model of the United States to be used in the quantitative analysis. The third section applies this model to evaluate the potential usefulness of familiar financial variables as intermediate targets when the chief nonfinancial focus of monetary policy is the growth of nominal income. The following section undertakes an analogous evaluation focused separately on real income growth and price inflation. And a final section briefly summarizes the principal conclusions of this analysis and re-emphasizes some of its limitations.

Intermediate targets as information variables

Why should a central bank, in conducting monetary policy, take account of the movements of money or credit?[1]

After nearly a decade of formal reliance on monetary aggregate targets for monetary policy by the Federal Reserve System, and the adoption of analogous targets by an increasing number of central banks around the world, even to pose such a question may at first seem like so much inspecting the interstices of the obvious (hardly an unknown activity in the social sciences). Yet the question is a serious one. In the circumstances under which most central banks today actually conduct monetary policy, the relevance of movements in money or credit is far from self-evident. Still less self-evident is why central banks should elevate measures like money or credit to the level of intermediate policy targets, thereby creating the presumption that, in implementing monetary policy, they not only may but indeed will respond to the movements of these variables.

At least part of the reason why this issue receives relatively little serious attention in current discussions of monetary policy is probably the fault of the professional economics literature, which more often than not relies on hypothetical constructs that either rule the question out altogether or in the end make the answer—within those constructs—genuinely self-evident. At the theoretical level, for example, most models simply treat the money stock as an exogenous variable, directly subject to control by the central bank. In such models there can be no question of the central bank's ***responding*** to movements of the money stock, because by assumption the central bank ***initiates*** all such movements. Similarly, most theoretical models include only one monetary asset, and in some models that asset is the only available form of wealth holding.[2] Such models, of course, cannot address the question of to which movements the central bank may want to respond when there are two or more monetary aggregates that covary imperfectly. At the empirical level, much of the current discussion simply assumes away the great body of evidence documenting the instability of any simple specification of the relationship between nonfinancial economic activity and any measure of money.

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1. This section relies in part on arguments developed at a formal level in Brunner and Meltzer (1967), Tobin (1970), Poole (1970), Kareken, et al. (1973), and Friedman (1975).

2. It is astonishing that some economists, having hypothesized models including a single form of wealth holding, proceed to label that single asset 'money" and then draw logical inferences on which they then base recommendations about actual monetary policy.

The circumstances under which the Federal Reserve actually conducts U.S. monetary policy are quite different. No monetary or credit aggregate is directly subject to central bank control. Instead, the Federal Reserve controls the growth of nonborrowed reserves, or perhaps a short-term interest rate like that on federal funds. There is not just a single monetary asset. Instead, the market offers a great variety of forms of deposits (and, similarly, an enormous variety of forms of borrowing), and the number of potentially definable monetary (or credit) aggregates is limited only by imagination and data collection machinery. No simple money-income or credit-income relationship is consistently reliable over short time horizons. Moreover, given the pace and extent of changes in patterns of U.S. financial intermediation, there is little ground for strong confidence in such relationships over longer horizons either.

Why, then, under these circumstances, radically different from those so often either explicitly assumed in the professional economics literature or casually assumed in discussions of current policy, should the Federal Reserve take account of the movement of money or credit in implementing monetary policy? The potential role of such variables in the policy process stems from the possibility that their movements may provide information, which is otherwise either unavailable or difficult to process, about the nonfinancial targets that the central bank seeks ultimately to affect.

The starting place for making monetary policy is a set of objectives for the nonfinancial economy. In part because of the targeting and reporting requirements imposed on the Federal Reserve by Congress, but also because much other planning takes an annual form, the typical procedure in the United States involves the tentative identification each year of a desired rate of economic growth for the year ahead, in both real and nominal terms.[3] The Federal Reserve then determines, and publicly reports to Congress, the target rates of money and credit growth that are likely—as seen in advance of the fact—to be consistent with that economic growth. Finally, the Federal Reserve determines, and implements via open market operations, the growth of nonborrowed reserves (or the federal funds rate level) that is likely—again, as seen in advance of the fact—to be consistent with the targeted growth of money and credit.[4]

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3. Because of lags (inertia), of course, not all desired growth rates of either prices or real income are feasible. The discussion here assumes a choice from within the feasible range.

4. Before October 1979, the Federal Reserve's operating instrument was typically the federal funds rate. Thereafter it was the growth of nonborrowed reserves. Wallich (1984) has stated that from late 1982 on it was borrowed reserves.

As of the beginning of the year, therefore, the Federal Reserve in principle outlines a mutually consistent set of growth rates for real income, prices, money, credit, and nonborrowed reserves, and it uses open market operations to implement the one element in this package under its direct control. The question at issue here is what further usefulness—if any—the money and credit aggregates possess. If actual money or credit growth deviates from the corresponding targeted pace, should the Federal Reserve respond? And if so, why, since the ultimate policy objective is to affect not money or credit growth but real economic growth and price inflation?

Responding to aberrant movements in money or credit growth is a useful policy under these conditions only if such movements forewarn subsequent (or contemporaneous but as yet unobservable) movements of real income or prices. For example, money growth greater than targeted—that is, greater than expected in advance to be consistent with the desired growth of income and prices—may indicate that later on either real income or prices (or both) will advance more strongly than expected. If so, responding to this excessive money growth by reducing the growth of nonborrowed reserves will set in motion forces of adjustment—involving in the first instance higher short-term interest rates, but in addition much broader aspects of asset yield and price relationships—to help restrain the excessive nonfinancial economic activity. Similarly, if money growth less than targeted forewarns coming economic weakness, responding by increasing reserve growth will set in motion forces acting to bolster activity levels. The rationale for responding to either faster or slower credit growth than targeted is analogous.

This familiar monetary policy procedure, based on targeted growth rates for money and credit (or, more commonly, money only) suffers from two potential drawbacks. The first, of course, is that aberrant movements of the targeted aggregate may not indicate future economic strength or weakness after all. Instead, they may merely reflect shifts in the portfolio preferences of either financial institutions or the general deposit-holding and liability-issuing public. In that case, policy responses in the form of changes in reserve growth (or in short-term interest rate levels) will be counterproductive, pushing nonfinancial activity away from, rather than toward, its intended course. Whether or not the Federal Reserve should respond to such unexpected movements of money or credit therefore depends, in the first instance, on what information about future economic activity these movements convey. A large and long-standing empirical literature has examined this question, primarily using "nonstructural"

methods that rely on no specific economic model.[5]

The second potential shortcoming in the use of monetary and credit aggregates as intermediate policy targets is that whatever information about future activity levels these aggregates do convey may simply duplicate information readily available from other convenient sources. Given the large element of inertia in short-run fluctuations of economic activity, surely the first place to look for information about income growth in the near future is in the recent movements of income itself. In other words, the relevant question is not just whether a potential intermediate target provides information about future income growth but whether it provides information not already contained in recent movements of income itself. A large empirical literature has addressed this question too, again primarily using nonstructural methods.[6] It is also possible to frame this question in a much broader way by asking whether yet other readily available data may also contain the same information that movements of money or credit convey, but the policy implications of empirical findings in this broader context are less straightforward because of the difficulty inherent in strategies explicitly relating monetary policy responses to large numbers of different variables.

The task undertaken in this paper is to address these questions about the information contained in potential intermediate targets of monetary policy, using a small 'structural" macroeconometric model of the United States. The key advantage of basing the analysis on a structural model, in comparison to the more prevalent use of nonstructural methods in the recent literature, lies in the presumably superior representation of expected economic behavior, and hence the superior division of the respective movements of variables like income, money, and credit into corresponding expected and 'surprise" components, that the structural model provides. The answer to any question about the information contained in unexpected movements in money or credit can be only as valid as the underlying distinction of expected versus unexpected movements on which it relies. By relying on nonstructural (usually vector autoregression) models for this purpose, the recent literature implicitly assumes that the best available representation of the expected movement of any variable is an unrestricted linear projection from past values of itself and other variables, and identifies any difference between this projection and the corresponding actual movement as unexpected. A structural model instead uses the

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5. Traditional references include Friedman and Schwartz (1963) and Andersen and Jordan (1968).

6. See, for example, Sims (1972, 1980) and Friedman (1983 and forthcoming).

relevant economic theory to restrict the representation of a variable's expected movement, and hence also to identify the unexpected part of its actual movement.

A further advantage of basing the analysis on a structural model is that structural models typically make clear the relationships among the operating instruments, potential intermediate targets, and nonfinancial objectives of monetary policy. Empirical findings therefore have a ready interpretation in terms of the policy process, and specific results correspond in a straightforward way to rules for central bank response. By contrast, evidence generated without using any structural model is at best difficult to translate into policy implications.

The countervailing disadvantage of the structural approach, of course, is that the particular structural model used may rely on theory that is irrelevant or invalid. In that case the restrictions imposed may make the model's representation of expected economic behavior, and hence the corresponding distinction of expected versus unexpected movements in any given variable, not superior but inferior to their unrestricted, nonstructural analogs. Similarly, if a model does not adequately represent the relevant macroeconomic behavior, policy rules suggested by its properties may be misdirected and even counterproductive. Given its compactness and simplicity, the model used here is clearly illustrative rather than definitive.

The next section presents a small macroeconometric model, and the following two sections go on to analyze its implications for the information value of potential monetary policy targets. An important caveat is in order, however, before proceeding to that task. Even the finding that aberrant movements of money or credit contain information about future economic activity, and that such information is not readily available elsewhere, does not warrant taking account of this information by establishing money or credit as an intermediate target in any strict sense. The Federal Reserve should respond to such information, to be sure, and it may even be useful to establish a form of targeting procedure to institutionalize the presumption that it will do so. In general, however, the appropriate policy response is different—under most realistic circumstances, more modest—than that required to return money or credit fully to the corresponding targeted path.[7]

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7. One reason for the more modest response, analyzed by Poole (1970) and Friedman (1975), is that in general such an abberant movement reflects some combination of unexpected economic strength or weakness and unexpected shifts in portfolio preferences. A second reason, analyzed by Brainard (1967), is that policymakers do not know with certainty the correct values of the parameters describing the economic effects of policy actions.

A macroeconometric model

Table 1 shows estimates, based on U.S. quarterly data spanning 1961:I-1979:III, for the six-equation Pirandello Model first presented in Friedman (1977) and subsequently updated in Clarida and Friedman (1983). The model includes empirical estimates for relationships describing aggregate demand, aggregate supply, money demand, money supply, and the term structure of interest rates, plus a nominal income identity.[8] For convenience, all equations are linear in logarithms, and no variable is lagged more than once. Hence the model is a simple linear first-order difference equation system.

The reason for limiting the model's estimation to data through 1979:III is that there is evidence of a break after that date in all five of the estimated relationships? To the extent that the conditions newly characterizing the immediate post-1979:III period continue to prevail, the model is therefore a description of historical behavior only. More recently, however, the Federal Reserve System appears to have moved away from the new policy procedures adopted in October 1979.[10] The model may therefore be applicable to current behavior as well, even though not to that of the few years immediately following 1979:III.

The model's aggregate demand equation includes an interest rate, or IS curve, effect (here based simply on a nominal long-term interest rate), as well as a fiscal policy effect and a terms-of-trade effect. The aggregate supply equation relates price setting to real economic activity and also to the terms of trade. The money demand equation has the standard real LM curve specification. The money supply equation combines a nonborrowed reserves multiplier effect with a borrowed reserves response associated with the discount rate and an excess reserves response associated with the short-term market interest rate." The term structure equation, which provides a link between the long-term interest rate in the aggregate demand

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8. The only change in specification from the original 1977 model is due to the use of M1 rather than M2 as the monetary variable. The estimates shown in Table 1 are from the appendix to Clarida and Friedman (1983).

9. By contrast, there is no evidence of a break after 1976:II, the endpoint of the sample originally used in Friedman (1977). *See* the comparison of F-statistics in Table 5, Clarida and Friedman (1983).

10. *See* again Wallich (1984).

11. The coefficients of the two interest rate terms in the money supply equation are not significant individually but are highly significant jointly. The test statistic for the null hypothesis that both coefficients are zero is $\chi^2(2) = 16.2$.

TABLE 1
Equations of the Pirandello Model

(1) ***Aggregate demand***

$$AX_t = .0064 - .1026\,\Delta r_{Lt} + .1024\,AE_t - .0688\,\Delta I_{t-1} + .4397\,\Delta X_{t-1}$$
(4.8) (−2.9) (2.0) (−2.2) (5.0)

SE = .00780 $\bar{R}^2$ = .49 ρ = −.4

(2) ***Aggregate supply***

$$\Delta P_t = .0895\,\Delta X_{t-1} + .0542\,\Delta I_{t-1} + .8700\,\Delta P_{t-1}$$
(3.4) (3.9) (25.2)

SE = .00347 $\bar{R}^2$ = .88 ρ = −.1

(3) ***Money demand***

$$A(M-P)_t = .1192\,AX_t - .0406\,\Delta r_{St} + .8703\,\Delta(M-P)_{t-1}$$
(1.9) (−3.9) (7.7)

SE = .00676 $\bar{R}^2$ = .53 ρ = −.5

(4) ***Money supply***

$$AM_t = .0034 + .2118\,AR_t + .0097\,\Delta r_{St} - .0234\,\Delta r_{Dt} + .7627\,\Delta M_{t-1}$$
(2.3) (2.1) (0.6) (−1.3) (8.6)

SE = ,00481 $\bar{R}^2$ = .53 ρ = −.2

(5) ***Term structure***

$$r_{Lt} = .0472 + .1441\,r_{St} - .0579\,r_{S,t-1} + .1376\,\Delta(L-S)_{t-1} + .9100\,r_{L,t-1}$$
(1.4) (1.1) (−0.5) (2.3) (37.0)

SE = .020 $\bar{R}^2$ = .98 ρ = .4

(6) ***Nominal income identity***

$$\Delta Y_t = AX_t + \Delta P_t$$

Notes: Equations are estimated using Fair's (1970) method for simultaneous equations with lagged dependent variables and serially correlated disturbances.
Sample period is 1961:I-1979:III.
Numbers in parentheses are t-statistics.
All variables are in logarithms.
Predetermined variables are **E, I,** L, **R, r_D,** and **S.**

Definitions of Symbols:
E = high-employment federal expenditures
I = import price deflator
L = outstanding long-term federal debt
M = money stock (M1)
P = GNP price deflator
R = stock of nonborrowed reserves
r_D = discount rate
r_L = Baa corporate bond rate
r_S = three-month Treasury bill rate
S = outstanding short-term federal debt
X = real GNP
Y = nominal GNP.

equation and the short-term interest rate in the money demand and money supply equations, combines a form of the standard expectations hypothesis with a debt management policy effect.[12] The nominal income identity is straightforward.

As estimated here, these six relationships determine six variables: the growth rate of nominal and real income, prices, and money, and short- and long-term interest rates. Exogenous variables include monetary policy (nonborrowed reserves and the discount rate), fiscal policy (high-employment government expenditures), debt management policy (the maturity composition of outstanding government debt), and the dollar price of imports.

An alternative way of specifying the stochastic structure of the model is to assume that the direct instrument set by the Federal Reserve's open market operations is not the growth of nonborrowed reserves but the short-term interest rate. In that case, the short-term rate would be an exogenous conditioning variable, while nonborrowed reserves would be one of the six variables jointly determined by the model. Because the Federal Reserve is free to choose either nonborrowed reserves or the short-term interest rate as its operating instrument, and because there is some ambiguity about how Federal Reserve policy has actually operated in the past, it is interesting to know the model's implications for key policy questions under either specification. The two sections below therefore report parallel sets of results along just these lines. Changing the assumed stochastic structure of the relationships among the model's variables in general changes the corresponding estimated coefficients, however, so that the alternative sets of results based on an interest rate instrument rely on a different set of coefficient estimates (not shown) than the ones based on a reserves instrument shown in Table 1.[13]

The Pirandello Model's compactness and simplicity result, of course, from the imposition of many restrictions on the data. Those restrictions

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12. The coefficients on the two short-term interest rate terms in the term structure equation are not significant individually but are highly significant jointly. The test statistic for the null hypothesis that both coefficients are zero is $\chi^2(2) = 10.4$.

13. As an historical matter, of course, only one (at most) of these two descriptions of the monetary policy process can be correct for the model's estimation period. It is in general not valid to draw inferences from a model estimated assuming a stochastic structure different from that which characterized actual behavior during the estimation period. The relevant question here is which of the two policy instruments was exogenous during that period.

necessarily limit—although, apparently, to a surprisingly small degree—the model's ability to represent actual macroeconomic behavior.[14] The corresponding advantage purchased by those restrictions is not just convenience, but the facility that the resulting model's form provides for explicitly analyzing policy questions like the ones addressed here.

Intermediate targets for nominal income

A familiar, albeit simplified, representation of the process of choosing and implementing monetary policy targets begins by positing a desired growth rate for nominal income for some period ahead, then translates that desired income growth into the implied growth of the money stock, and in turn translates that money growth into the implied growth of nonborrowed reserves. The two translation steps involved could be as simple (simple-minded?) as merely allowing for average trend movements, first in monetary "velocity" and then in the "money multiplier," or they could incorporate sophisticated econometric and/or judgmental predictions of the dynamic money-income and money-reserves relationships. Carrying out this task using the model shown in Table 1 would stand somewhere in between.

Given such a model, and given the values of the four exogenous variables other than nonborrowed reserves over the relevant time period, it is straightforward to determine what rate of reserves growth the Federal Reserve System should implement in order to make the conditionally expected nominal income growth over this period equal to any chosen rate. The model also indicates what rate of money growth to expect over this period, given the implemented reserves growth as well as the assumed values of all other predetermined variables—including, importantly, the serially correlated disturbances to the model's five stochastic relationships.

As the first entry in the middle column of Table 2 shows, the standard deviation of the model's forecasting error for nominal income growth an indefinite number of quarters ahead (that is, the final-form residual corresponding to a forecast for a period sufficiently far in the future to eliminate altogether the role of information about the model's endogenous variables) is 1.19 percent.[15] In the absence of any other information external to

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14. See the discussion in Clarida and Friedman (1983). For a comparative analysis of the model's predictive behavior see Mahoney et al. (1983).

15. The final form of the simple model used here is just its solved-out autoregressive representation. If the structural model is written as $\underline{y}_t = A\underline{y}_t + B\underline{y}_{t-1} + C\underline{x}_t + \underline{u}_t$ where $\underline{y}$ and $\underline{x}$ are vectors of endogenous and exogenous variables, respectively, and $\underline{u}$ is a vector of disturbances, to the structural relationships, then the model's final form is

$$\underline{y}_t = \sum_{i=0}^{w} [(I-A)^{-1}B]^i (I-A)^{-1} C\underline{x}_t + \sum_{i=0}^{\infty} [(I-A)^{-1}B]^i (I-A)^{-1} \underline{u}_t.$$

(Continued on next page.)

TABLE 2
Standard Errors for Nominal Income Residual Autoregressions

Included lags	*Model from Table 1*	*Model with credit*
None	0.0119	0.0122
1	0.0104	0.0108
1,2	0.0102	0.0107
1,2,3	0.0101	0.0106
1,2,3,4	0.0100	0.0104
2,3,4	0.0105	0.0109
3,4	0.0106	0.0110
4	0.0109	0.0112

the model, therefore, nominal income growth at a long horizon out would be within about a ± 1¼ percent range of the forecast value two-thirds of the time. The remaining entries in the column also show that the availability of observations on ***recent*** income growth helps somewhat in predicting ***future*** income growth. Making the forecast of future income growth conditional also on observations of recent income growth reduces this range to about ± 1 percent for periods up to four quarters ahead. In other words, the model's final-form residuals are serially correlated, so that taking account of whether income growth has been higher or lower than expected in the recent past (that is, allowing for previous final-form residuals) reduces the model's forecasting error in comparison with the corresponding uninformed forecast. Because allowing for this additional information in general changes the model's conditional forecast of income growth, it also in general changes the reserves growth necessary to make the conditional expectation of income growth equal the same chosen rate as before.

What, then, is the potential role for the rate of money growth—or any other intermediate policy target—in the policy process? If observed money growth different from prior expectations also provides information that bears on future income growth, then a forecast of future

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The final-form forecast (the expected movement in $\underline{y}$) for any period is then

$$\hat{\underline{y}}_t = \sum_{i=0}^{\infty} [(I - A)^{-1}B]^i (I - A)^{-1} C\underline{x}_t$$

and the corresponding final-form residual (the unexpected movement) is

$$\underline{\epsilon}_t = \underline{y}_t - \hat{\underline{y}}_t = \sum_{i=0}^{W} [(I - A)^{-1}B]^i (I - A)^{-1} \underline{u}_t$$

Because estimation of the model provides values of $\underline{u}$ only from 1961:I on, the calculation of $\underline{\epsilon}$ (and therefore all results based on $\underline{\epsilon}$ reported in Tables 2-7 below) begins in 1964:I, thereby avoiding possible problems associated with truncation of the infinite sum. (An alternative procedure would be to calculate $\underline{\epsilon}$ from $\underline{x}$ values extending back before 1961:I, but data are not available for all of the exogenous variables for enough prior quarters.) Analogous results for calculations beginning in 1966:I show no essential difference.

income growth conditional on recent money growth will likewise be superior to the corresponding uninformed forecast. In addition, as in the case of information contained in recent income growth, allowing for the information contained in recent money growth in general changes the reserves growth necessary to make the conditional expectation of future income growth equal the same chosen rate as before, and hence in general warrants a policy response in the form of a different rate of reserves growth.

The initial question to ask, therefore, is whether money growth in fact contains such potentially useful information. Moreover, as the discussion in the first section explains, establishing a presumption that the Federal Reserve will respond to whatever information is contained in money growth, rather than simply responding to observed income growth, makes sense only if the information contained in money growth is not also contained in income growth itself.

The first column of Table 3 reports standard errors for a series of equations relating the model's final-form income growth residuals to lagged values of the corresponding final-form residuals for money growth and, in all but the first two equations, lagged values of the income growth residual itself. For a model as simple as the one used here, it would be possible to infer these standard errors (or their equivalents) directly from the properties of the model's estimated coefficients, but the point of using instead regressions like those underlying Table 3 is to illustrate a method of analysis that is readily applicable to more complex models as well. The first two values shown indicate, in comparison to the standard error of 1.19 percent reported for the uninformed forecast in Table 2, that movements of money growth do contain information about future income growth. Even so, comparison with the other standard errors reported in Table 2 shows that this information is little greater than that contained in recent movements of income growth.

The issue, however, is not whether money growth contains more or less information than income growth, but whether money growth contains *additional* information not contained in income growth. The next two values shown in the first column of Table 3 are standard errors for equations relating nominal income residuals to lagged values of the money growth residual and the income growth residual itself, entered with comparable timing. Comparison with the corresponding standard errors based on lagged income growth alone, shown in Table 2, indicates that the additional information contained in money growth is significant statistically

TABLE 3
Standard Errors for Nominal Income Residual Regressions With Information from Endogenous Financial Variables (Reserves Exogenous)

	Information variable (Z)			
Variables in regression	ΔM	Δr_S	Δr_L	ΔC
Z_{-1}	0.0102**	0.0117	0.0103**	0.0092**
Z_{-1}, Z_{-2}	0.0097**	0.0117*	0.0101**	0.0092**
$Z_{-1}; Y_{-1}$	0.0098**	0.0104	0.0098**	0.0093**
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; Y_{-1}, Y_{-2}, Y_{-3}, Y_{-4}$	0.0094**	0.0098	0.0096*	0.0091**
$Z_{-1}, Z_{-2}; Y_{-2}$	0.0096**	0.0109	0.0099**	0.0091**
$Z_{-1}, Z_{-2}, Z_{-3}; Y_{-3}$	0.0096**	0.0109	0.0097**	0.0092**
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; Y_{-3}, Y_{-4}$	0.0094**	0.0103	0.0095**	0.0092**

* Z variables significant at 0.05 level.
** Z variables significant at 0.01 level.

but not economically.[16] A reduction in the standard error of the informed forecast from 1.04 percent to 0.98 percent (or from 1.02 percent to 0.94 percent) is hardly ground for establishing money growth as an intermediate policy target.

These comparisons are not necessarily apt, however, if data on money growth become available before data on income growth. It may still be useful for the Federal Reserve to react to the information contained in money growth if the' information contained in income growth, which it duplicates, is unavailable. Even with a further one- or two-quarter lag imposed on the income growth residuals but not the money growth residuals, however, there is still apparently little additional information contained in money growth. The last three values shown in the first column of Table 3 are standard errors for regressions relating nominal income growth to lagged money growth and to lagged income growth itself with just such

16. The significance levels reported in Table 3 (and in Tables 4, 6 and 7 below) are for the t- or F- statistics pertaining to the information variables (for example, unexpected money growth) in the regressions indicated. These significance levels strictly rest on the assumption that the remaining unexplained residual variation in these regressions is not serially correlated. This assumption is apparently plausible in most cases. For example, of the Durbin-Watson values for the seven regressions in the first column of Table 3 (the seven regressions based on unexpected money growth), only one indicates serial correlation that is statistically significant at the .05 level. The significance levels reported in Tables 3, 4, 6, and 7 also strictly rest on the assumption that the model's exogenous variables, including policy variables, are not affected by feedback from the endogenous variables. This assumption, of course, is more dubious.

differential lags. Once again, the additional information contained in money growth is statistically significant, but hardly enough to matter economically.

Money growth is not the only financial variable that may contain potentially useful information in this context, of course, and in principle the Federal Reserve may instead choose to alter the growth of nonborrowed reserves in an analogous way in response to some other readily observable financial variable. The model used here, with nonborrowed reserves taken to be the direct operating instrument of monetary policy, generates forecast values (and hence, after the fact, final-form residuals) not just for money growth but also for short- and long-term interest rates. The second and third columns of Table 3 present results, analogous to those based on money growth in the first column, for tests of the information about future nominal income growth contained in either of the two interest rates.

These results provide no ground at all for the Federal Reserve's responding to movements in short-term interest rates, and they suggest that the case for responding to long-term rates is about comparable to that for responding to money. The standard errors for the equations including the short-term rate residuals, shown in the second column, are uniformly larger than those of the corresponding equations including the money growth residuals, and the information contained in short-term rates is typically not statistically significant. The standard errors for the equations including the long-term interest rate residuals are only marginally larger than those of the corresponding equations including money growth, and the information contained in long-term rates is always statistically significant. The reduction in standard error, however, is again never sizeable enough to make the indicated responses very interesting in a policy context.

The three financial variables that are endogenous in this model—money growth and short- and long-term interest rates—do not constitute the entire universe of potentially useful intermediate target variables for monetary policy. The final column of Table 3 reports analogous results for tests of the information'aboutfuture nominal income growth contained in movements of aggregate credit growth. These results are based on a model identical to that shown in Table 1, except that the financial quantity used in the third and fourth equations is total domestic nonfinancial credit, so that these equations become, in effect,'credit demand" and 'credit supply" equations.[17] The resulting model is highly similar to that shown in Table 1,

17. This procedure is clearly inferior to the more ambitious undertaking of respecifying these equations to represent the demand for and supply of credit more appropriately.It does, however, render the results more directly comparable with those based on the model including money.

as the properties of the final-form income growth residuals reported in the right-hand column of Table 2 indicate. In addition, the results (not shown) of regression tests for the information content of the short- and long-term interest rate residuals in this altered model are very similar to the corresponding results shown in the second and third columns of Table 3.

The results based on this altered model, reported in the final column of Table 3, indicate that the credit aggregate apparently offers the best prospect of any of the candidates considered here as a potential intermediate target for monetary policy. The standard errors for the equations including credit growth residuals are uniformly smaller than those for the corresponding equations including the residuals for any of the other three variables, despite the slightly larger bases of comparison shown in the right-hand column of Table 2. Moreover, the ***additional*** information contained in recent movements of credit, beyond what is already contained in nominal income itself, is typically greater than that contained in any of the other three variables. With a single parallel lag on both credit and income, for example, the reduction in standard error is from 1.08 percent to 0.93 percent. With four lags and a two-quarter delay on the receipt of income data, the comparable reduction is from 1.10 percent to 0.92 percent.

Finally, it is also interesting to consider the value of potential intermediate targets for monetary policy when the Federal Reserve conducts open market operations by setting the short-term interest rate rather than the growth of nonborrowed reserves. The first three columns of Table 4 present results, analogous to those shown in Table 3, based on an alternative version of the Pirandello Model estimated with the short-term interest rate taken as exogenous and reserves growth, along with money growth and the long-term interest rate, endogenous. The final column of Table 4 presents further analogous results based on this alternative model estimated with credit in place of money. The results show that, if the Federal Reserve's direct operating instrument is the short-term interest rate, only the long-term interest rate (among the four variables considered here) consistently exhibits potentially useful information about future movements of nominal income.

Intermediate targets for real income and prices

The above analysis proceeds from the simplying assumption that it is possible to summarize the Federal Reserve System's objectives for the nonfinancial economy in terms of desired growth of nominal income. This practice is broadly familiar, both because it sidesteps the arbitrariness inevitably involved in weighting two or more ultimate policy objectives, and

TABLE 4
Standard Errors for Nominal Income Residual Regressions With Information from Endogenous Financial Variables (Short Rate Exogenous)

	Information variable (Z)			
Variables in regression	ΔM	ΔR	Δr_L	ΔC
Z_{-1}	0.0109	0.0109	0.0090**	0.0106*
Z_{-1}, Z_{-2}	0.0106	0.0107	0.0091**	0.0100**
$Z_{-1}; Y_{-1}$	0.0108	0.0109	0.0089**	0.0107
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; Y_{-1}, Y_{-2}, Y_{-3}, Y_{-4}$	0.0102	0.0105	0.0092**	0.0103
$Z_{-1}, Z_{-2}; Y_{-2}$	0.0105	0.0106	0.0092**	0.0101
$Z_{-1}, Z_{-2}, Z_{-3}; Y_{-3}$	0.0103	0.0105	0.0092**	0.0101
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; Y_{-3}, Y_{-4}$	0.0102	0.0104	0.0092**	0.0102

* Z variables significant at 0.05 level.
** Z variables significant at 0.01 level.

also because some economists have hypothesized that monetary policy can only affect nominal income without affecting the division of nominal income between real and price elements.

Familiar as it is, however, focusing only on nominal income is not fully satisfactory for purposes of a discussion of intermediate targets for monetary policy. The most immediate reason is that the choice of an appropriate growth rate for the money stock, the most traditional intermediate target variable, is not invariant to the real-price composition of the associated nominal income growth. Although it is standard to assume a unit price elasticity of the demand for money, empirical evidence consistently indicates an income elasticity of (M1) money demand well below unity.[18] Hence the money growth that would be consistent with any chosen nominal income growth is greater as the underlying rate of price inflation is greater and the corresponding real growth smaller. More fundamentally in the policy context considered here, the appropriate central bank response to information about future price inflation in general differs from the appropriate response to information about future growth of real economic activity.

It is also interesting, therefore, to look beyond the information that potential monetary policy target variables contain about nominal income to see what information they contain about, at the least, real income and prices. Table 5 provides a basis for the relevant comparisons by showing

18. For recent years only, there is also some evidence of a non-unit price elasticity.

TABLE 5
Standard Errors for Real Income and Price Residual Autoregressions

	Model from Table 1		*Model with credit*	
Included lags	ΔX	ΔP	ΔX	ΔP
None	0.0093	0.0059	0.0101	0.0054
1	0.0091	0.0039	0.0098	0.0038
1,2	0.0091	0.0037	0.0099	0.0037
1,2,3	0.0091	0.0036	0.0099	0.0036
1,2,3,4	0.0091	0.0036	0.0099	0.0036
2,3,4	0.0092	0.0040	0.0100	0.0039
3,4	0.0092	0.0041	0.0100	0.0040
4	0.0093	0.0044	0.0100	0.0043

standard errors of the Pirandello Model's final-form residuals for real income growth and price inflation (and the corresponding residuals of the model with credit) analogous to those shown in Table 2 for the model's nominal income residuals.[19] The residuals for price inflation exhibit substantial serial correlation, but the real income residuals do not.

The upper panel of Table 6 presents standard errors, analogous to those in Table 3, for equations relating the model's final-form real growth residuals to lagged values of the final-form residuals for the model's endogenous financial variables and, in most cases, to lagged values of the real growth residual itself. The results show that movements in both money growth and credit growth, and especially in the long-term interest rate, consistently provide statistically significant information about future real income growth beyond that contained in recent values of real income growth. Comparison to Table 5 shows, however, that the associated reduction of the real growth forecasting error due to observed money growth or credit growth is too small to warrant much attention in a policy context. By contrast, that due to observed long-term interest rates—for example, from 1.00 percent to 0.82 percent with a two-quarter lag on real income data—is small but perhaps worth a policy response.

The lower panel of Table 6 presents standard errors for equations analogously relating the model's final-form residuals for price inflation to lagged values of the other residuals and lagged values of the inflation residual itself. These results show that movements in both money growth and credit growth, and in the short-term interest rate, consistently provide statistically significant information about future inflation beyond that contained in recent inflation. Here it is questionable, however, whether

19. The final-form residuals used as the basis for these calculations are again for the model estimated with reserves exogenous.

TABLE 6
Standard Errors for Real Income and Price Residual Regressions With Information from Endogenous Financial Variables (Reserves Exogenous)

	Information variable (Z)			
Variables in real income regressions	ΔM	Δr_S	Δr_L	ΔC
Z_{-1}	0.0087**	**0.0093**	0.0082**	0.0094**
Z_{-1}, Z_{-2}	0.0086**	**0.0094**	0.0080**	0.0094**
$Z_{-1}; X_{-1}$	0.0087**	0.0092	0.0083**	0.0094*
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; X_{-1}, X_{-2}, X_{-3}, X_{-4}$	0.0088*	0.0092	0.0083**	0.0096*
$Z_{-1}, Z_{-2}; X_{-2}$	0.0087*	0.0094	0.0081**	0.0095*
$Z_{-1}, Z_{-2}, Z_{-3}; X_{-3}$	0.0087*	0.0094	0.0082**	0.0096*
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; X_{-3}, X_{-4}$	0.0087*	0.0092	0.0082**	0.0095*
Variables in price regressions				
Z_{-1}	0.0053**	0.0057*	0.0056*	0.0035**
Z_{-1}, Z_{-2}	**0.0051****	0.0056*	**0.0057**	0.0035**
$Z_{-1}; P_{-1}$	0.0038*	0.0037**	0.0039	0.0033**
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; P_{-1}, P_{-2}, P_{-3}, P_{-4}$	0.0034**	0.0033**	0.0036	0.0033**
$Z_{-1}, Z_{-2}; P_{-2}$	0.0038**	0.0038**	0.0042	0.0034**
$Z_{-1}, Z_{-2}, Z_{-3}; P_{-3}$	0.0036**	0.0035**	0.0041	0.0033**
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; P_{-3}, P_{-4}$	0.0035**	0.0034**	0.0039	0.0034**

* **Z** variables significant at **0.05** level.
** **Z** variables significant **at 0.01** level.

the resulting reduction of the model's inflation forecasting error due to the information in any of these financial variables—at most, from 0.41 percent to 0.34 percent for the short-term interest rate and with a two-quarter lag on inflation data—is of value in a policy context.

Finally, Table 7 presents standard errors for both real income growth and price inflation residuals that are analogous to those shown in Table 6 but based on the alternative version of the Pirandello Model estimated under the assumption that the direct operating instrument of monetary policy is the short-term interest rate. Here the long-term interest rate stands out in consistently providing statistically significant information about future real income growth. Credit growth, and, to a slightly lesser extent, money growth and reserves growth, all provide statistically significant information about future price inflation.

Conclusions and caveats

The basic premise underlying the analysis in this paper is that any financial variable has potential value as an intermediate target for monetary policy only if observed movements of that variable contain information about the likely future movements of whatever aspects of nonfinancial economic

TABLE 7

Standard Errors for Real Income and Price Residual Regressions With Information from Endogenous Financial Variables (Short Rate Exogenous)

	Information variable (Z)			
Variables in real income regressions	ΔM	AR	Δr_L	ΔC
Z_{-1}	0.0101	0.0102	0.0079**	0.0095**
Z_{-1}, Z_{-2}	0.0102	0.0100	0.0079**	0.0094**
$Z_{-1}; X_{-1}$	0.0096	0.0096	0.0079**	0.0093*
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; X_{-1}, X_{-2}, X_{-3, X-4}$	0.0095	0.0095	0.0079**	0.0094
$Z_{-1}, Z_{-2}; X_{-2}$	0.0100	0.0103	0.0079**	0.0095*
$Z_{-1}, Z_{-2}, Z_{-3}; X_{-3}$	0.0099	0.0099	0.0080**	0.0094*
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; X_{-3}, X_{-4}$	0.0097	0.0097	0.0079**	0.0095
Variables in price regressions				
Z_{-1}	0.0043	0.0042**	0.0044	0.0041**
Z_{-1}, Z_{-2}	0.0039**	0.0038**	0.0044	0.0034**
$Z_{-1}; P_{-1}$	0.0037	0.0036*	0.0037	0.0035**
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; P_{-1}, P_{-2}, P_{-3}, P_{-4}$	0.0032**	0.0035*	0.0037	0.0032**
$Z_{-1}, Z_{-2}; P_{-2}$	0.0039	0.0038'	0.0040	0.0034**
$Z_{-1}, Z_{-2}, Z_{-3}; P_{-3}$	0.0034**	0.0035**	0.0040	0.0032**
$Z_{-1}, Z_{-2}, Z_{-3}, Z_{-4}; P_{-3, P-4}$	0.0032**	0.0036*	0.0040	0.0033**

* Z variables significant at 0.05 level.
** Z variables significant at 0.01 level.

activity the central bank seeks ultimately to affect. Further, keying monetary policy responses to observed movements of any such variable is sensible only if the relevant information it contains is not also contained in other readily available sources—in the first instance, from observed movements of nonfinancial activity itself.

The empirical results presented in this paper, based on a small quarterly macroeconometric model of the United States, indicate the absence of compelling evidence in favor of singling out any single variable as "the intermediate target" of monetary policy. Of the variables considered here—including money (M1), credit, a long-term interest rate, and whichever of either reserves or a short-term interest rate the Federal Reserve System does not set directly by open market operations—most do contain at least some statistically significant information about the future growth of nominal income, real income, or prices. In most cases, however, this information is significant statistically but not economically. In other words, the reduction in forecasting error gained from using it is typically too small to be of great moment in a policy context.

The paper's principal conclusion, therefore, is to cast doubt on the practice of designating specific financial variables as intermediate targets of monetary policy. To the extent that such targets are necessary for other reasons, however—for example, to facilitate Congressional oversight of the Federal Reserve's policy decisions—the strength of this conclusion varies from one potential intermediate target to another. Among the variables considered here, credit growth and the long-term interest rate appear to offer the best prospects of providing information that would be useful in formulating and implementing monetary policy. For example, when the direct operating instrument is growth of nonborrowed reserves and the ultimate policy objective is stated in terms of nominal income, the reduction in forecast standard error associated with the information contained in credit growth is 0.18 percent. Even so, specific results like this one for credit growth are not invariant to the assumed operating instrument and ultimate nonfinancial objective, nor to the assumed pattern of data availability, so that any positive implications for the use of intermediate targets for monetary policy are at best highly conditional.

Several further caveats about the findings reported here are also worth repeating. First, the analysis in this paper focuses only on the question of information contained in single financial variables. It therefore omits entirely the possibility that the movements of two (or more) such variables, in conjunction, may provide potentially valuable information not contained in either alone. Because the Federal Reserve currently specifies either target ranges or monitoring ranges for four financial aggregates, this possibility certainly bears investigation. Empirical findings along such lines would also have implications for the difficult question of how the Federal Reserve should respond when two of its designated target variables give conflicting signals.

Second, it is important to re-emphasize that the appropriate monetary policy response to the information contained in unexpected movements of any designated financial variable is in general ***not*** to take actions that would return that variable to its previously expected path—that is, to treat it as an intermediate target in the traditional sense. Unless there is a one-for-one relationship between observed movements in the financial variable and likely future movements of the relevant aspects of nonfinancial economic activity, the appropriate policy response is instead to use the information that the financial variable provides by taking action expected to return not it but nonfinancial activity to the previously targeted path.

Finally, the analysis reported here relies on an econometric model that is extremely compact and simple. The model apparently does a surprisingly

good job at capturing some of the main features of macroeconomic behavior, but it necessarily omits many more. The method of analysis suggested in this paper for using a structural model to address questions for which the previous literature has relied on nonstructural models, however, is more general. The applications here to one small, simple model need be no more than an illustration. A parallel analysis based on a more powerful, and presumably more trustworthy, model would be a straightforward extension of this research.

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Commentary

Stephen M. Goldfeld

This paper is a logical extension of some of Ben Friedman's valuable work in monetary economics. That work has several strands. First, it has clarified the nature of intermediate targeting and demonstrated that the informational assumptions implicit in a two-stage targeting procedure can be extreme. Friedman has shown this theoretically and, using an early version of the Pirandello model appearing in the present paper, has quantitatively evaluated the inefficiency in two-stage targeting.

Given the importance of informational assumptions in this work, it is not surprising that a second related strand of Friedman's research has been to evaluate the informational content of a broad range of financial variables. A basic approach in this regard has been to ask whether surprises or innovations in a particular financial variable or set of variables can contribute to an explanation of current or subsequent movements in variables like GNP and prices. It is based on this research that Friedman has become one of the leading advocates of the informational value of a credit variable. As Friedman has previously emphasized, finding an informational role for a financial variable does not mean that intermediate targeting on that variable is an optimal, or even a good, policy, since there may be many variables that provide information. Furthermore, as a third strand of Friedman's research has sought to demonstrate, the relationships among financial and nonfinancial variables may not exhibit the requisite temporal stability needed to justify the religious targeting on some financial variable.

Taken as a whole, then, the various strands of Friedman's past research have cast considerable doubt on the merits of intermediate targeting. His present paper attempts to add another nail to the coffin. Not surprisingly, it bears a strong resemblance to some of Friedman's earlier research. There is, of course, a novel element in the paper, and this lies in the nature of the econometric technique used to provide the latest nail. However, despite its

novelty, I have serious reservations about the usefulness of the procedure. Indeed, to put it simply, I think it is unnecessary to use the procedure and dangerous to do so. Moreover, Friedman does not carry out the procedure in a way that it is consistent with the econometric model he presents. I will try to make the basis for these claims clear as we proceed.

Friedman starts with the informal idea that intermediate targeting makes sense only if aberrant movements in the target variable tell you something that you don't know about the future course of the economy. He further takes the view that one tests this by looking at "surprises" in some likely target variable and seeing if these explain future surprises in GNP or real GNP or whatever. A key element in this is how one goes about defining surprises and how one carries out the relevant tests of significance. As Friedman points out, these questions have been traditionally examined by nonstructural methods. The earliest incarnation of this is the approach embodied in the so-called St. Louis equation. More recently, the technique of vector autoregression has been applied to these issues.

In the present paper, Friedman adopts something of a mixed strategy, relying on a small structural econometric model but then using the model in a way that has some spiritual similarities to the vector autoregression approach. Quite obviously, the conclusions one is entitled to draw from this exercise depend on the reasonableness both of the model and of the procedure that uses the model to answer questions of interest. I will say a bit about the model later, but for the moment I want to concentrate on the novel Friedman procedure. Unfortunately, this involves a bit of notation.

To begin with, let us focus on a case where there is one target variable denoted without much imagination by the symbol M and one goal variable, y. The basic idea is first to decompose y and M into systematic and surprise components. This is done in equations (1) and (2) where e_{yt} is the income surprise and e_{Mt} is the money surprise, and where the t-subscript denotes time.

$$(1) \quad y_t = \hat{y}_t + e_{yt}$$

$$(2) \quad M_t = \hat{M}_t + e_{Mt}$$

If one had values for the income and money surprises, one could then regress the income surprise on both lagged values of the money surprise and lagged values of itself. Friedman would then judge the informational value of the money variable by the contribution the lagged money surprises make to such a regression.

The problem, of course, is to get values for the surprises. Friedman suggests estimating a structural econometric model and then solving this model for the so-called final form that expresses the endogenous variables of the model as a function of all current and past values of the exogenous variables. The final form is then used to calculate the predicted values, $\hat{y}_t$ and **M,.** The surprises can then be calculated from equations (1) and (2), and these then can be used to evaluate the informational value of the money variable.

While this two-step procedure sounds superficially plausible, upon closer examination it is not that appealing. It is easiest to see this if we consider the logic of the Friedman approach in a simplified setting. More specifically, let us consider a one-equation model in which we assume that y, is related to its past value and one exogenous variable x, as in

$$(3) \quad y_t = ay_{t-1} + hx_t + u_t$$

For the moment, we also assume the parameters in equation **(3)** are known. By lagging equation **(3)** repeatedly and substituting for lagged y's on the right hand side, we can derive the final form of this model given by

$$(4) \quad y_t = h\sum_{i=0}^{\infty} a^i x_{t-1} + \sum_{i=0}^{\infty} a^i u_{t-i}$$

We see that the first term on the right hand side of (4)is a prediction of y, based on current and past values of the exogenous variable, so this is the needed $\hat{y}_t$. By (1), the second term is the surprise denoted by e,. We then have

$$(5) \quad y_t = \hat{y}_t + e_t$$

as required. Furthermore, given the definition of e,, it is easy to verify that

$$(6) \quad e, = ae_{t-1} + u,.$$

We are now in a position to make some preliminary observations about the Friedman procedure in this simple setting.

First we note that equation (6)is what Friedman would propose to estimate. But what we see is that (6)involves only one parameter of interest, a, and this parameter also appears in the underlying model, equation (3).Put another way, if we have (3), there is no need to do any second-step regression to get (6);we can simply write it down. What this also suggests is that

there is a one-to-one correspondence between the underlying model and the form of equation (6). As we shall see momentarily, this is true in general.

Now, of course, even with a simplified model like (3), we will in general not know the parameters a priori, so one would have to estimate (3) to determine them. However, once having estimated (3) there is no reason to estimate (6), since we already have an estimate of the parameter, a. Moreover, if one did choose to estimate (6) by least squares after estimating (3), one would not obtain an estimate of a with good statistical properties. Furthermore, the conventional tests of significances would not be applicable to this regression. In short, estimation of equation (6) is both redundant and fraught with statistical difficulties.

Before turning to a more general model, it is worth making one additional observation for this simple case. In particular, despite my disparaging remarks about estimating equation (6), in some cases it may be possible to learn something from its estimation. Consider, for example, the case when the true model is given by (3), but the investigator mistakenly assumes a is zero. If one goes through the Friedman procedure, one might well conclude that e_{t-1} matters in explaining e_t. One would then have a clue that one should reexamine the initial specification. In this case, the Friedman procedure would function like a crude version of the Durbin-Watson test. The same sort of thing would be true if the misspecification involved omitting a second order lag from (3) that was then included in (6). More generally, misspecifying the dynamics of the initial model will have implications for what looks important in (6). The message here, however, is that estimating the surprise equations is subject to yet another frailty—namely that it will be sensitive to the proper specification of the underlying model.

Armed with this background, we can quickly move through the general case where we deal with a multi-equation structural econometric model. As we know, such a model implies a reduced-form model. This is, in fact, what equation (3) is and, by analogy with (3), we can write the reduced-form model as

$$(7) \quad Y_t = AY_{t-1} + HX_t + V_t$$

where Y_t now represents a vector of endogenous variables and A is a matrix of parameters rather then a single parameter as in (3). Some algebra also yields the generalizations of equations (4) to (6) which are implied by (7).

In particular, we have

$$(8) \quad Y_t = (HX_t + AHX_t + A^2HX_{t-2} \ldots) + (V_t + AV_{t-1} + A^2V_{t-2} + \ldots)$$

which can be rewritten as

$$(9) \quad Y_t = \hat{Y}_t + E_t.$$

Here E, represents a vector of surprises, one for each of the endogenous variables in the model. Finally, we can manipulate the definition of E, to obtain the generalization of (6) given by

$$(10) \quad E, = AE_{t-1} + V,.$$

A comparison of equations (7) and (10) reveals, as before, that there is a one-to-one correspondence between the model and the surprise equations, and that the latter involve the same parameters as does the original model.

To illustrate the nature of (10), it may help if we consider a specific example. The following two-equation model, which is hardly meant to be anything other than an algebraic example, will suffice.

$$y_t = ay_{t-1} + bM_t + gM_{t-1} + \text{exogenous variables} + u_{yt}$$

$$M, = cy_t + dM_{t-1} + fy_{t-1} + \text{exogenous variables} + u_{Mt}.$$

While we have written this model in structural form (both endogenous variables, y, and M_t, appear in each equation) and have not spelled out the exogenous variables, this information is sufficient to derive the equations for the income surprise:

$$(11) \quad e_{yt} + \frac{(a+bf)}{1-bc} e_{y,t-1} + \frac{(g+bd)e_{M,t-1}}{1-bc} + \frac{u_{yt} + bu_{Mt}}{1-bc}$$

Equation (11) is the equation of interest in the Friedman procedure that is consistent with the initial model. Straightforwardly enough, it says that the lagged money surprise will help explain the income surprise whenever g is nonzero (M_{t-1} affects y, directly) or b and d are nonzero (M_{t-1} affects M, which, in turn, affects y_t).

What this brings out is the important point that all the substantive questions of interest about the informational content of a potential target variable are contained in the original model. In order to answer the kinds of questions that interest Friedman, one needs only to estimate the original model and then carry out the appropriate tests of significance based on the estimates. One could, for example, test hypotheses

about the coefficients in equation (11) from the estimates of the basic model. Moreover, because of the statistical difficulties alluded to earlier, estimation of (10) or (11), after one has first estimated the model, is a statistically invalid way of drawing the sorts of inferences that are at issue. In short, there is no need to use the Friedman procedure and many reasons not to.

Equations (10) and (11) also bring out another troublesome aspect of the Friedman procedure. As already emphasized, the form of these equations is implied by the underlying model. In general, this means that the income surprise equations should include the lagged surprises for all the endogenous variables in the model. Moreover, whether one includes first- or second- or third-order lags of these variables is determined solely by the lag structure of the original model. In estimating his surprise equations, Friedman violates both of these principles. More particularly, he includes lags of only two variables, whereas he has a six-equation model. Furthermore, he carries out his procedure with varying lag lengths, ignoring the fact that this sort of arbitrariness is ruled out by his own model.

Although my main concern is with the logic of the basic Friedman approach, as noted earlier, the reliability of the underlying model is also a potential issue. One feature of the model that deserves note is the apparently rather slow response of the money supply to an injection of reserves. Indeed, the actual magnitudes involved seem quite implausible, suggesting there may be some difficulty in using the model to evaluate monetary policy. A related issue concerns the choice of the exogenous policy variable. The model is estimated with either the short-term interest rate or nonborrowed reserves as an exogenous variable. The appropriate choice may not be either one or the other and should depend on what policies were pursued in the sample history.

Model details aside, there are also some issues of timing implicit in the Friedman paper that are worthy of note. The time unit of the basic analysis is quarterly, but data on reserves and money are available almost continuously. Since the Fed probably finds it hard to sit on its hands in the face of what appears to be new information, some realistic aspects of targeting may be lost with a quarterly focus. By using the latest revised data, another practical element in targeting is brushed aside. In particular, since there are often substantial revisions in money and GNP data, to evaluate targeting in a realistic way may require use of initial estimates of these variables. To paraphrase the words of Senator Howard Baker at the time of Watergate, we may need to ask, "What did you know and when did you know it?" Finally, there is a somewhat extreme timing aspect to the way Friedman chose to define his surprises. In particular, by use of the final form of his

model, the surprise is defined relative to a prediction based only on current and past values of exogenous variables. That is, no past values of the endogenous variables are used in making the predictions. While it is possible for someone to forecast in this way, it seems an unlikely description of any realistic forecast. As a consequence, the surprises implied by this procedure may be of limited interest.

Overall, then, while I have considerable sympathy with Friedman's punch line on the shortcomings of intermediate targeting, I am not persuaded that the evidence provided by his two-step procedure is of much value. Rather, it seems to me that Friedman needs to state precisely the hypotheses that he is interested in. These hypotheses could then be tested by estimates obtained from his structural model. While it might be possible to argue that Friedman's two-step procedure provides an approximation to the correct procedure, in view of the potentially serious statistical difficulties with his estimated surprise equations, it is his burden to make this case with some evidence.

Overview

James Tobin

Here we go again! This conference continues the chronic debate among economists and central bankers on fundamental issues of monetary policy: on the goals, capabilities, strategy, and tactics of demand management, on what we have or should have learned from the 1980s, the 1970s, and indeed the whole postwar period. The issues are familiar: rules versus discretion in policymaking; reactive versus fixed settings of instruments and targets; the importance, feasibility, and requisites of credibility of announced policies; the choice of instruments and targets; the unemployment/price tradeoff menu over short and long runs; the values to be placed on the choices offered.

Several papers by guest economists give the central bank hosts quite a beating. The Fed is accused of 'time inconsistency' specifically overaccommodating inflationary shocks and pressures in the short run, sacrificing its long-run goals and credibility to political expediency. These economists perceive the central bank's tasks and choices to be much simpler than the Fed itself has viewed them. Martin, Burns, Volcker, ***et al.*** will with some justice detect Monday-morning quarterbacking in these criticisms.

Logical program but ***uncompleted synthesis.*** The choice and order of the topics speak well for the logical thinking of the economists who organized the program. On the first morning we heard about the causes of inflation, then about its costs to society, and finally about the costs in unemployment of avoiding inflation. Our second session concerned how to conduct monetary policy so as to achieve price stability, at least in the long run, with minimum unemployment cost. Bob Hall used one of economists' favorite expository graphs, displaying a frontier of feasible choices of the two "bads," unemployment and inflation. From Fischer's paper we might perhaps distill a social indifference map to show us how to find the optimal choice within Hall's tradeoff menu.

Perhaps to the disappointment but hardly to the surprise of the organizers, so satisfying a synthesis did not quite jell. The separate pieces, *a fortiori* the discussion, meshed imperfectly. For example, most participants dissented from the optimistic monetarist views of Mishkin and McCallum on the causes of inflation and the unemployment costs of disinflation. But the critics did not agree with each other. Fair, Hall, and Gordon would all draw the feasible frontier differently, and they would not even use the same axes. Fischer's paper gave only qualified support to Hall's view, apparently shared by Mishkin and McCallum, that zero inflation is a desirable, as well as feasible, long-run goal. Pragmatic discussants like Nordhaus, Gordon, and Blinder would gladly settle for fairly stable single-digit inflation.

Forward commitments in monetary policy: the issues. Much of the debate at this symposium concerned the possibility and desirability of advance commitments in monetary policymaking. As a guide to this debate, I would distinguish several of its dimensions:

- How permanent should numerically specific commitments be? Forever, e.g., 3 percent per year growth in something for all time? Or periodically reconsidered and changed, like the Fed's targets for the aggregates?
- In what time series should commitments be expressed? Macroeconomic goal variables like unemployment, real GNP, prices, and inflation? Intermediate monetary and financial indicators like the monetary aggregates, credit, or interest rates? Instruments directly under central bank control, its balance sheet, its discount rate, or the federal funds rate? Magnitudes almost directly controllable, total or unborrowed reserves, or the monetary base?
- What role, if any, should actual observations and forecasts play in determining the actions to which the policymakers are committed? Should policy be blind to new information, on the grounds that determined disregard of current events and outlooks contributes to credibility? Or should policy respond to such information in preannounced ways? Or should policymakers retain discretion to cope with unforeseen, perhaps unforeseeable, circumstances?
- Should the objectives, strategies, and tactics of the central bank be explicitly and promptly announced? Or does judicious use of confusion help monetary policy achieve its social goals?
- What should be the constitutional status and political responsibility of the monetary authorities? Independent, or answerable to the executive or the legislature?

Rules versus discretion. Three papers—by McCallum, Mishkin, and Hall—advocate rules, imposed or self-imposed, well publicized and understandable, numerically definite, and permanently binding. McCallum and Mishkin seem to prefer non-responsive rules, blind to observed outcomes and forecasts; anyway they see no advantage in reactive policies. Their reasons are mainly ***apriori*** theoretical rather than empirical. Hall, however, sees great superiority in a reactive rule. His "elastic price standard" is a very interesting suggestion, ingeniously documented by 30 years of macrodata—altogether a refreshing contribution to this well-worn subject.

Targets and instruments. This overviewer was gratified to find in the papers by Ben Friedman and Hall such emphatic recognition that operating instruments must somewhere in policy strategy and tactics be related to goal variables of ultimate value. Friedman shows, not for the first time, the virtual uselessness and irrelevance of intermediate monetary aggregates as targets. The aggregates have no objective importance and carry little information not otherwise available; yet they, just like variables of macroeconomic importance, can be controlled only indirectly, by reactive manipulations of instruments. In glossing over this fact, McCallum and Mishkin illustrate Friedman's complaint that economists facilely and fallaciously assume that the M's of their simple models are directly controllable or that actual central bank instruments have all the properties of those model M's. Our profession seems to be reaching consensus and clarity on these points. So perhaps the grip of mechanical monetary-aggregate monetarism on policymakers, politicians, journalists, and markets, which has already been loosened, will at last be broken.

The substitution of nominal GNP—or even better, Bob Gordon's candidate, final sales—for monetary aggregates would be an improvement, because it would allow the Fed to offset velocity shocks without risking credibility. (If cosmetics would smooth the transition, the new targets could be called "velocity-adjusted aggregates.") But Hall's results show that a permanent rule fixing numerically the target path of nominal income could be a harsh recipe for handling OPEC and other price shocks. It mandates a 1 percent loss of annual output for every 1 percent excess of price index over target. A more accommodative response, followed by tightening gradually to remove the price bulge, seems indicated by Hall's simulations. Of course, nominal income could be used, like the aggregates now, as a periodically changeable numerical target. For example, each annual application of Hall's elastic-standard policy could be expressed and announced as a nominal income, or final sales, target for a year ahead.

Reactive rules as discretion. A permanent numerical rule for any nominal quantity, instrument or target, will in this overviewer's view some day become intolerably disastrous, with probability approaching one, because of big or cumulative shocks. This is true of the monetary base, M1, credit, nominal GNP, what have you. Even a permanent reactive rule, like Hall's formula, can get into trouble. It is as hard to specify in advance policymaker's responses to all contingencies as it is to write those Arrow-Debreu contracts so beloved of economic theorists. Some hawks condemn Volcker for 'blinking" when the going got tough in the summer of 1982. I agree with Bill Nordhaus that the Fed's announced policy of October 1979 did not—could not—say what the Fed would do in case of Third World debt crises, big negative velocity shocks, and domestic financial troubles. I agree with Alan Blinder that economists' conceptions of commitments to complex feedback rules are allegorical or stylized descriptions of 'discretion." My personal view is that the Fed has to have discretion to deal with contingencies, like those of 1982, within its general commitment to macroeconomic goals shared with Congress and the Administration.

Credibility. There is something in the idea, but in my opinion less than McCallum and Mishkin think. There is something in it when the message gets through—not just to the financial community, a skeptical audience obsessed with credibility, but to business managers, workers, and unions who actually decide or negotiate prices and wages. As Dr. Schlesinger's informative address reminds us, the German authorities aim at the critical audience. When the Bundesbank tells management and union leaders the implications of its monetary policy for the year, it is carrying out simultaneously a 'credible-threat" policy and an 'incomes policy."

In the decentralized wage- and price-setting institutions of the U.S. and U.K., threats by Volcker and Thatcher seem to have brought little or no amelioration of the time and cost of disinflation. Threats to everybody in general but to nobody in particular are evidently not very effective. McCallum, like other partisans of 'credible-threat" strategy, says that strategy wasn't really tried. Well, we never have perfect experiments in macroeconomics. Blinder's quotation from a previous symposium somehow struck me as right on the mark! Policymakers in a representative democracy can never tie their own and their successors' hands as securely as the advocates of permanent rules would like. Economists who would engrave their concepts and numbers in the Constitution 'have a lot more confidence in the stability of economic structure and in their understanding of it than history justifies.

Monday morning quarterbacking. *A* theme common to McCallum, Mishkin, and Hall is that the United States could easily have enjoyed a much better price or inflation record, along with an unemployment record as good or even better. For the first two of these authors, this is deemed an obvious truth. Greatly to his credit, Hall seeks to demonstrate it empirically.

Most likely we could have done better, but there are several reasons to believe that even Hall overstates the case:

1) Note that Hall's own simulations make unemployment no lower, generally higher, under his rules than actual unemployment every year before 1979, except 1975 under the 'dove" policy—as nearly as I can tell from his Figure 5. The improved outcomes come mostly since 1979. Hall's simulations say that we recently suffered much too high unemployment for the disinflation achieved.

2) Hall assumes that policymakers like William McChesney Martin could have known in the 1950s and 1960s what Hall knows now from a structure estimated on data through 1983. Hall knows, for example, that the "natural rate of unemployment"has been 6 percent all along, but no observations available to Martin or Burns or Heller told them that. Shouldn't Hall have calculated his simulations from 'rolling" regressions and forecasts, using no data not available to policymakers each year? Moreover, uncertainty and fluctuation of the level of the 'natural rate" are surely major problems in demand management, omitted from Hall's model.

3) Hall plots in Figure 7 actual results far above his variance frontier. The price variance is greatly exaggerated by taking it around its mean rather than its trend. Given that Hall's preference for price stability over inflation stability rests on his concern for fairness to long-run nominal savers, the measure he should use is the variance of the ***ex post*** real long interest rate.

4) The sharp price deflations in Hall's simulations may be harder to achieve and more devastating to aggregate demand than the model, estimated without such observations, contemplates. We cannot be sure the short-run Phillips curve does not become very flat at zero growth of nominal wages.

5) Actual inflation, especially bulges that accompany OPEC-like shocks to real wages and profits, may leave in their wake more upward wage and price pressures that Hall's Phillips curve allows. He optimistically assumes that public confidence in his policy would wipe the terms for expectational and institutional inertia out of his wage and price equations.

6) I strongly suspect that errors of monetary control and forecast are bigger than those of Hall's model, and I just cannot follow his argument that we need not worry about the precision of the relation of the Fed's instruments to aggregate demand and prices because the "black boxes" of the forecasting profession will handle the problem.

Tradeoff menu. The orthodox view that there is no tradeoff to policy in the long run is, I gather, accepted by all authors but Fair. McCallum and Mishkin think that the long run is pretty short. Fair challenges orthodoxy; he says he has found a long-run tradeoff. It seems, however, to be between price level and unemployment, rather than between inflation and unemployment.

Fair's tradeoff seems to be the upward slope of the conventional aggregate supply curve, used in Mishkin's diagrams. Evidently the Fair Model (No. 1 in his paper)—although it has price inertia from the inclusion of lagged wages and prices in his equations—has no built-in ***inflation*** inertia. That is, the contractual, institutional, and expectational lags in wage and price formation would not prevent the rate of price increase from subsiding even at low maintained rates of unemployment. Evidently the model has no steady state with an inflation rate other than that consistent with the time trend in the money wage equation, a price inflation rate that will vary inversely with the productivity trend. In the Fair model, above-trend inflation occurs while the price level is adjusting to shocks or policies; when adjustment is complete, it stops.

Fair may have shown the econometric superiority of his model over the two opponents he sets up. I do not see what this demonstration implies about the existence or duration of a Phillips tradeoff. While I concur with Fair's preference for a structural approach to wages and prices, I find it hard to believe that the mechanisms of inflation inertia and expectations have not changed over the sample period, and hard to accept a 'natural" inflation rate determined by an unexplained trend in nominal wages.

Mishkin proclaims the truth—in all macro theories--of Milton Friedman's dictum that inflation is always and everywhere a monetary phenomenon. Well, who could doubt it? Inflation is by definition a general rise in commodity prices in terms of the ***monetary*** unit. A rise in MV/Q is tautologically a rise in P.

The famous dictum may be a useful antidote to the naivete or willful blindness of many politicians and some economists. In small open economies with underdeveloped securities markets, government deficits are automatically monetized. They depreciate the exchange rate and generate domestic inflation, often hyperinflation. The malady is jointly fiscal and

monetary. This is not a description of the inflation problem in the United States. Here, unguarded repetition of the dictum too often conveys the message that inflation is easy to prevent and to cure, if only politicians and central bankers would be resolute and farsighted.

That message is terribly misleading. All serious macroeconomists agree that monetary policies and quantities have important effects on aggregate demand. They do not all agree, as this symposium illustrates, that monetary policies and events affect solely prices and have no effects on output and employment. That inflation is a monetary phenomenon does not exclude wage- and price-setting institutions as additional 'causes" of inflation, in that they impose severe real costs as side effects of monetary anti-inflationary medicine. As Nordhaus pointed out, the shape of the "AS" curve, shifted as it frequently is by supply shocks, depends on the degree of monetary accommodation. That degree has been the big policy issue of recent years, and the critical issue of this conference. Reminding us of Friedman's aphorism contributes nothing to its resolution.

The social value of price stability. Fischer provides an updated catalogue of the costs of inflation. Its relation to the other papers is to guide the assignment of social values to price stability and high employment, to help us draw indifference curves tangent to policy frontiers. Fischer points out how the costs of inflation depend on society's institutions—tax laws, interest ceilings, indexations—and their adaptability. Of course, changing some of these institutions would also, by making prices more or less volatile, for example, alter a Hall or Taylor variance frontier.

When Fischer and other authors list or estimate 'costs of inflation," I wish they would more consistently tie them to actual feasible policy choices. When inflation is a joint product of other disasters, it should not be charged with the unavoidable costs of those disasters. It should be charged only with the extra costs, if any, attributable to handling them in an inflationary way. The Weimar republic had to pay reparations, and we had to pay tribute to OPEC. These were not 'costs of inflation." Confusion on this point, along with failure to understand that inflation raises the incomes you receive as well as the prices you pay, may be sources of popular anti-inflation sentiment. Fischer's costs are not in aggregate enough to explain their strength.

Certainly the 'money triangle" is not the source of popular passion. As Shiller remarked, Fischer did not point out here, though he has done so elsewhere, that depriving the Treasury of seignorage would necessitate additional explicit taxes, with their own distortionary costs. This would be true whether the loss of seignorage resulted from price stability or

deflation, or from paying interest on base money. The latter is therefore not such a cheap way of countering 'shoe leather costs: as Fischer's present paper seems to say.

Hall bypasses cost-benefit analysis of inflation. He just wants a constant yardstick. The yard is a stable measure of distance, and the dollar should be a stable measure of purchasing power. The analogy is defective, especially for long periods of time, because of all the index number problems that economists know about but prefer to forget. The strongest argument for price stability is that it provides a safe vehicle for accumulation of purchasing power. This can probably be better done by adding indexed bonds, entailing some sacrifice of expected return for the reduction of risk, to the menu of financial assets, rather than by making price stability a requisite of macroeconomic policy. Wholesale indexation, however, is another matter. It would substitute a new yardstick for our present monetary unit, and all our difficult problems would recur in a different and perhaps even less tractable form. Real wage stickiness would probably be worse than nominal wage inertia.

Most of the personal disappointments of economic life are due to deviations of relative wages and prices from expectation. Relative price movements are inevitable byproducts of economic change and technological progress; sometimes acceptance of their consequences for the overall price level facilitates adjustment. Some nominal anchor to the price system is needed, no doubt. But it is better provided, as both Schlesinger and Fischer stressed, by the reputation of the macroeconomic policymakers, earned through experience, for responsible and judicious use of their discretion, than by formal commitments to rules.

Overview

Allan H. Meltzer

My assignment is to give an overview of the principal issues raised at this conference on price stability and the contributions of the individual papers to these issues. The principal issues have been the choice between rules and discretion in setting the path for a return to price stability and the preferred type of rule if discretionary actions are avoided. These issues bring to the fore the role of anticipations and the related issue of credibility, since the costs of returning to price stability are almost certainly lower if the return is anticipated and if policy actions are perceived as consistent with the goal of stable prices.

The issue of rules versus discretion is an old one. Policymakers, or their staffs, are inclined to dismiss rules casually by arguing that judgment is superior to a rule requiring constant money growth if there are shifts in the demand for money. This argument does not do justice to the analytic issues, and it fails to consider the type of monetary arrangements recommended in much of the recent academic literature on the subject.

My interest in monetary arrangements began 20 years ago when Karl Brunner and I analyzed the working of the Federal Reserve System and proposed changes for the House Banking Committee (Brunner & Meltzer [1964]). At the time, discretionary policy consisted of choosing a level of free reserves—member bank excess reserves minus member bank borrowing—every three weeks. The U.S. was on the Bretton Woods standard. In practice, as everyone eventually learned, this standard did not restrict monetary policy or maintain price stability. Despite their commitment to fixed exchange rates and a fixed gold price, the Federal Reserve retained discretion, and it permitted the rate of money growth to be determined by its choice of the level of short-term interest rates or free reserves. Principal responsibility for the fixed dollar exchange rate was left to other countries. Most chose to maintain fixed exchange rates, so the

discretionary policy decisions in the United States produced inflation in all the principal economies of the world. Although we did not forecast this outcome, we urged the Federal Reserve and Congress to change their procedures by adopting a monetary rule, and by permitting exchange rates to fluctuate if necessary to maintain the proposed rule.

The particular rule we chose called on the Federal Reserve to set the growth rate of the monetary base once every six months so as to achieve that rate of money growth consistent with the goals of the Employment Act of 1946. These goals are maximum employment consistent with price stability. We rejected, explicitly, the idea of setting the growth rate of the base once and for all (Ibid., p. 85). In today's jargon, we favored a contingent rule specified in terms of the growth rate of the monetary base. We proposed that the growth rate of money (M1) be used as an indicator of the future effects of monetary policy.[1] To facilitate implementation of the proposed rule and to reduce variability, we recommended several changes in operating procedures.

Our choice of the particular rule was based then, as it would be now, on a judgment about the comparative costs of activism and passivity. In the choice of monetary rules, as in other activities, there are type one and type two errors. Central banks typically err on the side of activism, but they can remain too passive, as they did in the 1930s when the Federal Reserve remained inactive despite the collapse of the monetary system and its own forecasts of widespread banking failures. Or, to choose a more recent example, foreign central banks' policies remained too passive in the 1960s when faced with inflation emanating from the United States. And the Federal Reserve did little to stop the inflation caused by its policy of interest rate control.

The papers at these sessions, and many of the discussions, show a rising interest within the academic profession in a policy rule. The type of policy rule that has attracted much interest does not require the central bank to close its doors. Rather, the central bank would adopt what Bennett McCallum has called an activist but non-discretionary policy rule. McCallum's paper in this volume proposes one type of rule. Robert Hall proposes another. Frederic Mishkin favors McCallum's (1984) rule. And I regard the McCallum rule as within the spirit of both our 1964 recommendation and the recent version I have offered elsewhere (Meltzer, [1983]).

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1. **Some prefer the term 'intermediate target" in place of "indicator." The two are not the same. An indicator in our terminology gives current information about future values of variables like GNP.**

There is, of course, no unanimity about rules either in the profession or in the papers. Ray Fair's paper favors, even urges, discretionary policies that seek to lower unemployment by increasing inflation or, in his model, by raising the price level. Benjamin Friedman's paper does not directly address the issue or comment on his preferred means of returning to price stability, but he appears to favor the use of an econometric model to forecast GNP growth, inflation, and other variables, and to use the model's forecasts to set targets for real income growth and inflation. He is critical of central banks' use of targets for growth of monetary aggregates and their occasional attempt to offset deviations from the announced targets, so he is unlikely to favor a monetary rule.

The Fair and Friedman papers are flawed, however. Fair concludes from estimation over a particular sample period that there is a potential tradeoff between real and nominal values. I thought the main issue between natural rate theorists and others was about whether there is an exploitable tradeoff—whether reductions in unemployment today are bought at the cost of higher unemployment tomorrow. Or, to put the same point in another way, I thought the issue was, and is, whether the average rate of unemployment can be lowered permanently and repeatedly by raising the rate of inflation. Natural-rate theorists do not have to deny that a tradeoff can be estimated for a particular sample period using a particular set of equations. The issue as I understand it is whether a model like Fair's preferred model can produce and reproduce the estimated tradeoff in repeated trials.[2] What an econometrician sees when he looks back after the event may be the result of statistical illusion, resulting from a large permanent change during a particular sample as in Brunner, Cukierman, and Meltzer (1981) or from non-linearities, misspecifications, etc. The fact that a tradeoff can be found in some sample period does not imply that policy can fool most of the people all of the time, or even most of the time, as Fair proposes to do.

A problem with Friedman's paper is that his model is misspecified. The real demand for goods and services depends on the nominal rate of interest and the nominal price of imports.[3] One of Friedman's claims is that he obtains his evidence from a structural model. This claim loses its force

2. Fair's use of levels of prices and output raises an issue about the stationarity of the estimates and the reliability of the findings. Meese and Singleton (1982) show the relevance of stationarity for tests of exchange rates. Also, his paper continues the indefensible tradition of computing tradeoffs between endogenous variables, one of which is assumed to be fixed.

3. Friedman refers to the import price as the terms of trade. This requires constant export prices. Other comments on the Fair and Friedman papers are in later sections.

when the model is seriously flawed. A principal result—the information he finds in long-term interest rates—reflects the improper specification. Further, Friedman's estimates suggest that a fall in nominal import prices raises real output, and a rise in nominal interest rates lowers real spending. These estimates imply that there is money illusion in the aggregate demand equation. This does not establish that his conclusions are wrong, but they are suspect and cannot be accepted as evidence for, or against, monetary targets or monetary rules.

Hall emphasizes that, relative to an 'activist" rule, discretionary policy has increased price variability and average inflation and unemployment in the past. Stanley Fischer's paper summarizes some of the costs of inflation and, as in Fischer (1981), he includes costs that could in principle be avoided by changing institutional arrangements, tax systems, depreciation rules, and the like.

Fischer's paper raises the type of question that must confront anyone who urges changes in policy arrangements. There is a long tradition in economics, going back at least to Adam Smith, of recommending policies or policy actions. Disregard of many of the recommendations has a tradition that is at least as old. A major problem for economists, and other social scientists, is to explain the persistence of the apparently large departures from optimality, noted by Fischer and emphasized by Hall. This is a major issue in political economy or public choice to which I return.

The rest of my discussion is divided into three parts. The following section discusses some differences in proposed activist, nondiscretionary policy rules. The next section proposes a specific rule and compares its properties to some rules proposed at this conference. The rules proposed at the conference, as well as the discretionary policies, neglect effects on the exchange rate and on the rest of the world. These are major omissions, as recent experience emphasizes. An older tradition treats the choice of policy rules as a choice between stability of internal and external prices or between domestic prices and exchange rates. I attempt to harmonize the two.

History does not suggest that any of the proposals are likely to be adopted. The final section considers some political economy aspects that are too often neglected in discussions of this kind.

Some differences in types of policy rules

A principal reason for adopting a policy rule is to provide information about the future and thus enable people and firms to plan more reliably. My major criticism of the current policy regime—discretionary policy with pre-announced monetary growth rates and fluctuating exchange

rates—is that this regime increases uncertainty about future inflation, effective tax rates, and other variables required for long-term planning. No one can have much confidence, as he looks ahead, about whether inflation in any country will be between zero and 25 percent, the approximate range of inflation rates observed in developed, democratic countries during the past decade.[4] This is costly and far from optimal, as several of the papers note.

My criticism is that discretionary policies fail to provide a predictable path for money and do not restrict governments to a path leading to (average) price stability. It is not intended as a criticism of fluctuating exchange rates. Nor is it a criticism of pre-announced monetary targets. These targets, and the relation of actual to announced money growth, provide useful information that helps people to improve their forecasts.

The contrary evidence in Friedman's paper in this volume does not strike me as compelling given the resources invested in central bank watching, the care with which money growth rates are studied by market participants, the considerable evidence on the relation between maintained average rates of inflation and maintained average rates of money growth, and the flaws in Friedman's paper emphasized in Goldfeld's comment, as well as those noted in the previous section. The evidence in the Fischer and Mishkin papers reminds us again that even if four quarters of money growth make no contribution to autoregressive forecasts of next quarter's GNP growth, sustained, high money growth produces inflation.[5]

Reduction in uncertainty will not be achieved by removing information from the public. What is required is not less information but more information and more reliable, more credible information about future monetary policy. A credible rule can contribute to the reduction in uncertainty about future nominal income, prices, and inflation, as McCallum's paper points out, but all rules are not the same.

A useful distinction is between rules that depend on prospective instead of retrospective information—between contingent rules that tie action to forecasts of future events and contingent rules that depend on past performance. Reliance on forecasts means that errors of forecast affect policy actions.

Hall's proposal is most explicit. He urges the Federal Reserve to adjust money growth each month based on quarterly forecasts of unemployment

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4. The chart in Mishkin's paper shows these data.

5. Many forecasters use three-year (or longer) moving averages of money growth to forecast inflation. Friedman's Table 5 suggests that some of the effects of money growth show up within one quarter.

and price changes for the next two years, and he recommends a particular social contract. Deviations of forecast unemployment from the natural rate have eight times the weight assigned to deviations of the predicted price level from the level consistent with price stability. Errors of forecast for the unemployment rate, therefore, have a magnified effect on policy: Overestimates of future unemployment require greater monetary expansion; underestimates of future unemployment result in slow monetary growth. Hall's simulations have errors of forecast implied by Taylor's (1980) model with a particular lag structure. In practice, his proposal is very unlikely to generate the relatively stable paths shown in his simulations. In fact, if the lags are variable, errors of forecast for unemployment may be relatively large. In this case, monthly adjustments of money growth can produce greater variability in prices and unemployment than present discretionary policies. I do not claim that Hall's procedure would, in fact, have this result. We simply do not know, and Hall's paper does not give any information on which to base an answer.

The broader issue is whether to rely on forecasts at all, and if so, whether to rely on near-term or longer-term forecasts. A related issue is how fast policy action adjusts to deviations of forecasts from desired levels or rates of change. Hall, Friedman, and Fair either explicitly or implicitly want policy actions to depend on forecasts, but they differ about how far policymakers should look ahead. Hall's rule, as already noted, requires policymakers to adjust money to monthly changes in forecasts. Fair and Friedman do not discuss this issue. McCallum's proposal, favored also by Mishkin, and mine (Meltzer [1983]), repeated below, require policymakers to ignore forecasts and respond only to observables.

Available data can be used to judge the issue. McNees (1981) gives several measures of errors of forecast by forecast horizon for 16 separate forecasters from 1976 to 1980. The average absolute error for 16 forecasts of the growth of real GNP made during the same quarter is 2.7 percent. Eight forecasts made after the middle of the quarter are only slightly more accurate. Their error is 2.4 percent. These errors of forecast help to explain why rapid response to short-term deviations in real variables can increase instability. For one-year forecasts the errors are smaller, but not small relative to the average growth rate. The mean error of forecast for real growth made four quarters ahead is 1 percent for the same five-year period. For inflation, the mean errors are about 1 percent, also, for the same period.

Webb (1983) reports similar findings. He computed median errors of forecast from a large sample of forecasts, for the year 1971 to 1982, made using different forecasting techniques. For both real growth and inflation

four quarters ahead, the averages of the median errors for the twelve years are the same, 1.7 percent. For the shorter period most closely corresponding to the McNees data, the average of the annual median errors is 0.8 percent for real growth and 1.3 percent for inflation. Errors in excess of 4 percent were made in some years. Friedman's Table 2 shows that errors in excess of ±2 percent in forecasts of nominal income growth one year ahead are likely to be common.

Either a rule or discretionary policy based on forecasts is capable of producing errors that are a large fraction of the annual change. A recent paper by Bomhoff (1982) shows that, for time series models, one source of this error is the change in the parameters of the models used to make forecasts. Bomhoff uses a multi-state Kalman filter to forecast levels of aggregate variables. Errors arise from three types of disturbances. There are permanent changes in rates of change, permanent changes in level, and transitory changes in level.

If all errors are of the third kind—transitory changes in level—errors of forecasts are independent of the length of the forecast period. On the other hand, if all errors are transitory and are not known until after data become available, the optimal policy is a do-nothing policy. The reason is clear. The expectation for every period is a constant level. If prices conformed to this model—which is to say that monetary and real changes never changed the expected price level—the price level would be stable, and the problem of achieving price stability would be simpler.

At the opposite extreme, all changes are permanent changes in rates of change. The proper response to a permanent change is to adjust as soon as the change is known reliably. An example, to which I return later, is a permanent change in the growth rate of productivity and real income. A policy of price stability requires a corresponding, permanent change in the growth rate of money. If there is uncertainty about the timing of changes in productivity growth, forecasts of both the future price level and the rate of price change are uncertain.

A rule (or discretionary policy) that relies on forecasts can mistake transitory errors for permanent changes. When this occurs, policy is excessively active not only because the mistaken response to transitory shocks introduces excess variability, but because changes in money are likely to induce some short-term changes in real variables. The opposite error is excessive passivity. Permanent shocks to productivity growth are treated as transitory changes in level. The policy rule restricts policy to a slow response, so prices vary more than the ideal that would be achieved if shocks could be properly identified as they occur.

The problem is no different in principle from the problem that arises when policymakers rely on a particular model or Phillips curve as proposed by Fair, Hall, and Friedman. The lag structure depends on the relative variance of the permanent and transitory components of shocks, as in Muth (1960). When these variances change, lags change, and forecasts go awry.

Forecasts using time series models face the same type of problem. The parameters of forecasts that rely on past values of aggregate data are subject to change. Bomhoff (1982) used a moving average process to study the distribution of shocks to money and other variables in six countries. He found that shifts in the distribution of shocks are relatively large at times. Meltzer (1984) compared the distribution of shocks and the variance of forecast errors under six different monetary regimes and found relatively large changes within a particular regime and across regimes.[6] These studies and the forecast errors reported by McNees (1981) and Webb (1983) give little reason to expect that a rule that responds to forecasts of future events is likely to produce the type of improvement that Hall expects.

Hall defines price stability as a constant realized value of the price level and proposes to reverse all changes that cause the price level to differ from its base period value. His aim is to maintain a constant, long-run expected value of the price level while permitting short-run price changes along a Phillips curve.

The proposed rule has two flaws. Hall makes no allowance for changes in the so-called natural rate of unemployment, and his choice of actual instead of expected price stability is inefficient and costly. The reason is that one-time permanent changes in the natural rate—following a productivity shock, a change in the terms of trade, or some other real shock to output—change the price level. Hall's rule requires a change in the stock of money to offset the effect on the price level. This action increases variability by changing aggregate demand, thereby changing output, prices, money wages, and other variables. There is no social benefit from the additional variability. Also, Hall does not explain why the adjustment of real wages to a one-time change in the natural rate must be made by changing money wages while keeping the price level unchanged.

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6. Here is an illustration using an autoregressive model. Let $x_t = \rho_t x_{t-1} + u_t$ where u_t is a transitory random error. Suppose ρ_t, the coefficient determining persistence, is not constant but is governed by $\rho_t = \rho_{t-1} + v_t$, where v_t is the random shift in p_t. When u_t is very large and the value of p_t is not very certain, activist policies based on forecasts are likely to introduce more noise than they remove.

The main issue here is whether price stability means that the long-run price level is constant or whether the expectation is constant. That the two do not lead to the same policies is shown by supposing that a supply shock increases output and reduces the natural rate. The decline in the price level, following the shock, raises real wages as part of the adjustment. The rationally expected price level is constant, after the shock, at a lower level.[7] I can see no reason why policy should reverse the fall in the price level and require an increase in money wages. Nor can I see why holders of money and other nominal assets should not share in the gain (or loss) from unanticipated changes in productivity through the real balance effect.

Some properties of proposed rules

McCallum and Mishkin favor a rule, proposed in McCallum (1984), under which the central bank adjusts the monetary base to offset deviations of the level of GNP from its target. The target path for nominal GNP is determined by the average long-term rate of growth of real GNP at stable prices. If nominal GNP falls below this path, the monetary base increases, and if nominal GNP rises above the path, the base is reduced.

McCallum's rule differs from Hall's in several ways. First, real shocks to the level of productivity result in one-time price level changes. Second, the two rules respond to changes in aggregate demand in qualitatively similar ways, but McCallum's rule does not rely on forecasts. Third, increases in the growth rate of real GNP produce a falling price level under McCallum's rule, and reductions in the real growth rate produce inflation. The reason is that the rule does not adjust the growth rate of the monetary base for changes in the growth rate of output. The quantitative importance of the omission depends on the size and frequency of changes in the growth rate of output. The effect on the rate of price change would have been larger for Japan or Germany than for the United States in the postwar years. Price stability in the U.S. would have increased, however, if money growth had adjusted to the decline in the growth rate of real output between the '60s and the '70s.

All of the rules and discretionary policies discussed so far ignore exchange-rate changes and the effect of such changes on domestic prices and output. This source of variability is much larger for some countries than for the United States, but the short-term effect of exchange-rate changes on the price level seems too large to ignore even in the United States.

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7. If productivity shocks are normally distributed with zero mean, there is no reason to expect drift in either direction.

My proposed rule for monetary growth, Meltzer (1983), adjusts for changes in velocity growth and real income growth. With some help from major foreign countries, the proposed rule smooths the effects of changes in exchange rates. The rule is expressed in rates of change, or growth rates, not in levels, but there is no problem of base drift or inflationary bias, and there is no provision for changes in the position from which growth rates are computed. The rule achieves price stability on average, but the price level changes when there are permanent changes in the level of real income.

The rule requires each of the central banks that issues a major international currency—the United States, Germany, Japan, and the United Kingdom—to set the growth rate of its monetary base equal to a three-year moving average of the rate of growth of the country's real output minus the three-year moving average of its monetary base velocity. The choice of three years is arbitrary. It provides a built-in stabilizer by keeping money growth above real income growth during cyclical recessions and below real income growth during periods of high expansion. Money growth adjusts gradually to maintained, permanent changes in the growth rate of output or velocity. No use is made of econometric or other forecasts, and there is no need to distinguish in advance whether observed changes are adjustments of levels or changes in rates of change.

On average the rate of price change is zero. Since all major countries follow the same rule for price stability, all have the same expected rate of price change, zero. The common expected rate of inflation contributes to exchange rate stability. Prices and exchange rates fluctuate, but one cause of fluctuations--differences in expected rates of inflation—is damped or eliminated.

The three-year period *can* be interpreted as twelve quarters, and the growth of the base *can* be adjusted quarterly. I believe that quarterly adjustment puts too much weight on transitory changes in velocity and real income. Semi-annual or annual adjustment or money growth reduce the influence of these self-reversing changes.

Countries that are not parties to the agreement *can* also benefit. They have the choice of adopting the rule, of pegging to one of the currencies, or of pegging to a basket currencies. Or they can choose an independent policy and float.

The proposed rule has five attractive features:

- The rule sets the growth rate of the monetary base, a variable that the public can observe and the central bank can control with minimal error.

- The rule is adaptive and modestly counter-cyclical, but there is no 'drift" in the level from which growth is measured.
- The rule does not adjust quickly to large transitory changes in level, but it adjusts fully to permanent changes in rates of growth by the third year.
- The rule does not depend on forecasts, so it is not sensitive to forecast errors.
- The rule provides for increased exchange rate stability if other major countries adopt compatible rules, but there is no need for international coordination of policies. Exchange rates fluctuate.

In The Tract on Monetary Reform, Keynes (1923) recognized the importance of achieving price stability by policies that maintained both internal and external stability. This emphasis has been missing in most recent discussions. Some argue for rules like the gold standard or a revised system of fixed exchange rates, fashioned along the lines of the Bretton Woods agreement. If followed, these rules maintain more exchange rate stability than in recent years, but neither an international gold standard nor a return to a Bretton Woods system assures that domestic prices remain stable. Others favor restrictions on domestic monetary policy to maintain domestic price stability but ignore s'hocks from abroad.

In open, interdependent economies, fluctuations in prices and output can be reduced if there is greater certainty about foreign and domestic disturbances. This can be achieved by an agreement on the principles, or rule, for the conduct of each country's monetary policy.

Perspectives from political economy

Stanley Fischer's paper points out that the principal costs of inflation arise from the absence of institutional change. Governments fail to index tax rates and depreciation schedules, or they are slow to make these changes. Governments do not offer indexed bonds to shield the public from the loss of wealth and the uncertainty about future values during periods of variable inflation. In our recent experience, the Federal Reserve and other agencies maintained ceilings on the interest rates paid to depositors until financial innovation eroded much of the base against which this part of the inflation tax was levied.

These costs of inflation could have been avoided or significantly reduced in scope. The fact that most developed countries have not made the institutional changes that reduce the major costs of inflation is inconsistent with the usual treatment of central banks and governments in economic models. Governments, in these models, are agents or intermediaries

that assist households to maximize the utility of consumption, and they improve people's welfare by providing public goods and removing public bads. The failure to adjust institutions to reduce the cost of inflation is puzzling in this perspective.

A basic difference in models of political economy, or public choice, is that governments can be analyzed as the representative, or agent, of voters who recognize that some people can increase their wealth in the polling place above their earnings in the marketplace. Since the distribution of income and consumption across households is more skewed than the distribution of votes, the representative or decisive voter typically has less than the mean income and consumption of the community or society in which he lives. He has an incentive to redistribute income.

In Meltzer and Richard (1981) a utility-maximizing, decisive voter chooses the amount of income redistributed. Money and inflation are not part of this model, but the same principles seem applicable. Indexation of tax rates, depreciation, interest payments, and the values of governments bonds would reduce or eliminate most of the tax revenue from a maintained inflation. The government, which is to say the voters, would either have to reduce spending or find an alternative source of revenue. Do other revenue sources offer as much opportunity for redistribution as the outstanding stocks of bonds, capital, and human wealth?

The chart in Mishkin's paper shows that there is not a single country with stable or falling prices, on average, in recent years. All countries depart from price stability in the same direction, and few countries have acted to eliminate the inflation tax on existing stocks of bonds and real capital or to index income tax brackets and consumption taxes. Many countries have indexed transfers, for example payments to the aged, welfare recipients, and other groups. This asymmetry is consistent with the political economy model and is difficult to reconcile with neoclassical models that ignore voting and income redistribution.

Helmut Schlesinger's paper brings out the importance of the political economy aspect. The first part of his paper discusses the evils of inflation. Dr. Schlesinger emphasizes that the proper policy goal is price stability—defined as zero inflation on average. The second part of his paper discusses the actual policy of the Bundesbank. It turns out that the actual policy is to accommodate the existing rate of inflation. He then discusses the process by which the Bundesbank chooses the rate of money growth—specifically the rate of the growth of the monetary base or, as the Germans prefer, central bank money. Here, we note that the policy is decided in consultation with the government, representatives of the trade unions, and

other groups. From the perspective of political economy, it is not an accident that this political process has produced a positive average rate of inflation in both the '60s and the '70s—under both fixed and floating rates. The German inflation rate has been lower on average than in many other countries, most likely for the reasons Dr. Schlesinger gives. But, despite many warnings about the costs of inflation and repeated commitments to price stability, a modest, positive rate of inflation is the experience of the Federal Republic and, he tells us, it is the policy of the Bundesbank to accommodate inflation.

Germany's choice to maintain inflation and to avoid full indexation of taxes and government debt is a decision to tax the public in a particular way. The magnitude of the tax and the share of total expenditure financed by inflation differs from one country to another, but the outcome of the political-economic process appears to be similar in all democratic countries. A systematic process is at work.

Policy decisions are political decisions. Although I, and many others, have proposed rules to restore and then maintain price stability, these proposals are typically innocent of any political mechanism. The Barro and Gordon (1983)paper, discussed by McCallum, is a formal demonstration of the importance of a monetary rule if we are to maintain price stability. A rule is the only way, in their model, to reduce inflation to zero and to maintain price stability. The absence of a rule imposes a social loss, but their model gives no reason why the political process, the decisive voter, or the policymaker should try to minimize this loss.

A related, but distinct, issue is to explain why, with discretionary policy, the government does not choose, and the public does not expect, price stability. Cukierman and Meltzer (1984)show that where the policymaker knows more than the public about his own objectives and persists in his policies for a time, discretionary policy (both with and without announcements of monetary growth) has an inflationary bias. The policymaker—taken as a representative of the political process—gains from positive monetary surprises. The reason is that he benefits from current reductions in unemployment produced by surprises and discounts the costs of inflation and the future unemployment required to reduce inflation. In this model, the policymaker's objectives shift, from time to time, with greater weight given at some times to unemployment and at other times to reducing inflation. A weakness of this analysis is that the policymaker's objective function does not reflect the decisions of a representative voter.

The lesson of this discussion is that sustained price stability is as likely as a political commitment to an enforceable monetary rule. Both seem

remote. To paraphrase Adam Smith, we get inflation and discretionary policy, not from the malevolence of policymakers but from their self interest. After years of effort, proponents of rules have not reached the point at which proponents of discretion, whether policymakers or academics, feel compelled to show that discretionary policies remove more instability to prices, output, or employment than they add, or to explain why we have been as far from both price stability and minimum unemployment as Hall's chart suggests.

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