Editors' Summary

THIS ISSUE of *Brookings Papers on Economic Activity* contains articles and reports presented at the thirty-third conference of the Brookings Panel on Economic Activity, which was held in Washington, D.C., on April 1 and 2, 1982. One article examines proposals for a gold standard. A second examines alternative models for projecting the effects of policy changes and assesses the forecasts of the Reagan administration. A third evaluates the Swedish attempt at a countercyclical investment policy rule. Three reports in this issue look at the significance of recent wage concessions, the costs of reducing inflation through restrictive monetary policy, and issues concerning how monetary policy should be conducted.

INTEREST in restoring a gold standard has reemerged in the past few years among some politicians and a few economists. In the first paper of this issue, Richard N. Cooper reviews historical experience with various forms of gold standard, states the theoretical grounds for advocating or opposing such a standard today, and examines some contemporary proposals. As Cooper observes, "These proposals are being treated with a seriousness that would have been astonishing twenty, ten, or even five years ago." An official examination was recently concluded by the Gold Commission that President Reagan appointed in response to a congressional mandate for a review of the role of gold in the domestic and international monetary systems. Although the Gold Commission recommended against restoration of any monetary role for gold, its report was marked by disagreement among its members, and advocates of a larger role for gold continue to press their case.

Gold-standard schemes are advocated on the grounds, either implicit or explicit, that they provide price stability and remove discretion from economic policymakers. The many proposals that are described as "gold

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standards'' are in fact very different in their implications for either of these objectives. Contemporary proposals range from some indirect form of gold backing for the currency—some link between money or bank reserves and official holdings of gold—to full-fledged restoration of gold as currency. Cooper treats this range of proposals as falling into two categories: those that require gold backing without any convertibility between gold and dollars, and those that promise convertibility, either for foreign official holdings or for any holder of gold or dollars.

Gold backing without convertibility is a mock gold standard at best and is not likely to achieve either objective of its advocates. If the value of gold reserves is more than adequate to meet the backing requirement, the reserves have no essential role; while if the requirement for gold backing does threaten to limit the money supply, the authorities can decide whether to buy more gold. The United States had gold backing for its currency from 1879 to 1968. Once the Federal Reserve System was established in 1914, the gold reserve requirements limited the money supply only on rare occasions, and monetary growth was generally much slower than the limit allowed by the gold reserve requirement. When that requirement threatened to limit the growth of money in 1945, it was lowered and, eventually, was removed completely. Thus the realistic possibility that the requirement would be changed as needed adds a further element of discretion to any system not requiring convertibility. The most artificial proposal for gold backing ties the money supply to the value of gold reserves calculated at some initial price and then specifies that the ''price,'' for this purpose, should rise by 3 percent a year. In this proposal gold plays no essential role; the proposal simply provides a rule for how fast the stock of money should grow. It removes all discretion from the monetary authorities, but has nothing to do with gold.

Cooper explains that gold convertibility would restrict or eliminate discretion, but would not be likely to stabilize either the quantity of money or prices. With full convertibility of Federal Reserve notes and 100 percent gold money—meaning paper money or bank deposits are backed 100 percent by gold reserves and, in addition, gold itself circulates as money—the Federal Reserve would effectively be out of business, banks could not make loans, and the quantity of money would be dependent on the production and distribution of gold. Most proposals fall short of a move to 100 percent gold money, continuing to allow, for

example, fractional reserve banking. In these regimes, conversion of dollars into gold directly affects the composition of the money supply and forces a contraction of bank reserves. If the Federal Reserve attempts to defend its gold reserves in these circumstances, as it probably would, it would have to tighten credit conditions or otherwise change the expectations that led to the desire to convert. Either way, the public's desire for more gold contracts the economy. Conversely, if gold holders want more dollars, it will lead to an expansion in money and credit, either automatically or through the attempts by the monetary authority to avoid gold inflows by lowering interest rates. Thus any shift in market sentiment about the relation between gold and the dollar could provoke prolonged monetary contraction or expansion and produce economic instability. Cooper notes that the response of gold and dollar holders to developments such as the OPEC price increases of the 1970s could have made those episodes far more destabilizing under a system of convertibility.

In addition to pointing out the destabilizing possibilities inherent in convertibility, Cooper observes a number of problems in implementing a system of convertibility in the first place. To be meaningful, convertibility must specify a price; but any price that could be specified or any attempt to find a workable price has severe drawbacks. In the neighborhood of the recent price of gold, full convertibility would not be credible because the volume of potential dollar assets to be converted is much larger than the available gold stock. By contrast, with a much higher price that assured the ability to accept any dollars offered, gold production would rise and sales from the large existing gold stocks and hoards would take place, flooding U.S. authorities with gold and the world with dollars. Even if convertibility were limited to foreign monetary authorities, the United States would either have to monetize the gold, which would be strongly expansionary, or would have to use discretionary policy to sterilize the gold inflows, defeating one major purpose of convertibility. Cooper points out that schemes to let the market find the right price at which to start convertibility would provide enormous incentives to South Africa and Russia, the major producers, and to countries that are major holders to manipulate the markets so as to make the price as high as possible. He shows that certain proposals such as the one outlined by Arthur Laffer and advocated by Senator Jesse Helms are deficient on this account and also provide strong incentives to

speculate for or against gold by creating a separate ceiling and floor for its price.

In general discussion, it is often stated or implied that stability of the general price level expressed in dollars would follow from maintaining stability of the dollar price of gold. Cooper observes that, even if a stable dollar price of gold could be maintained—which, as discussed above, he believes would be difficult, costly, and perhaps impossible in today's world—it would in no way promise stability of the general price level, which could move relative to the price of gold. Between 1816, when the United Kingdom went on a full legal gold standard, and 1913, just before the First World War, the price level (and hence the relative price of gold) underwent long swings: it fell 41 percent between 1816 and 1849, rose 51 percent between 1849 and 1873, fell 45 percent between 1873 and 1896, and rose 39 percent between 1896 and 1913. Similar swings in the relative price of gold and the general price level took place in the United States, which adopted a de facto gold standard in 1879. Thus even "price stability in the long run" can only be found in the historical record by a judicious choice of years for comparison. Cooper also shows that economic performance by a variety of measures was generally worse during the gold standard era than in the period after World War II. The variability of inflation, as measured by annual changes in wholesale prices, was a little worse under the gold standard in the United States and a little better in the United Kingdom. In both countries, the period following World War II witnessed faster and more stable growth in real income per capita and a lower average unemployment rate.

Cooper concludes that there is no case to be made for any return to a monetary role for gold. No version of a gold standard offers an answer to controlling inflation. And gold convertibility would risk great monetary instability. If convertibility were established at a high price, as might be necessary, it would enrich South Africa and the Soviet Union, along with present gold holders. Convertibility could also "place the monetary system of the United States hostage to political decisions in one or both of these countries." Cooper also considers briefly the possible usefulness of other commodity standards aimed at stabilizing the price level. He observes that most of these aim to limit greatly or eliminate entirely discretion in monetary management. Although he does not enter into the general debate about discretion versus rules in the conduct

of policy, he raises the question of "why one should think that experts are more clever at devising operational, nondiscretionary monetary regimes than they are at monetary management within a discretionary regime."

ATTEMPTS to stabilize the economy have been a feature of economic policymaking in industrial economies during most of the postwar period. In recent years some research has focused on the possibility that policy actions may be ineffective because the private sector anticipates them and offsets their intended effect. Because most policy actions have been discretionary rather than coming from a well understood rule, attempts to assess the effectiveness of policy, taking into account this possibility of anticipations by the private sector, are frustrated by the inability to know what the private sector anticipated about policy and when. In the second paper of this issue, John B. Taylor analyzes the effectiveness of the Swedish investment funds system which, he argues, avoids this difficulty because it can be regarded as a countercyclical policy rule that was known to the private sector. This system was designed to encourage investment during recessions by lowering the cost of capital and operated countercyclically for more than fifteen years, until it began to be used as a permanent investment stimulus in the early 1970s.

The investment funds system is formally described in terms of "allocation" and "release" of funds to and from an interest-free investment account at the Bank of Sweden. Firms allocated funds to the account as an alternative to paying a tax on profits, up to a limit of about one-fifth of their profits. These funds were then released to the firms to finance investment during recessions. Taylor translates this procedure into the system's effect on the cost of capital to firms. The allocation provision is equivalent to a permanent reduction in the corporate profits tax; the release of funds further reduces the cost of capital during recessions. The use of released funds for investment had one major tax cost: depreciation could not be claimed on any portion of a project financed with such funds, even though the funds could not be used to pay for more than a fraction of the project. This cost largely offset the benefit of the funds system for investment in equipment, so Taylor concentrates on investment in structures. He shows that a funds release has a substantial effect on the net price of a typical structures project

during recession. Assuming a 3 percent discount rate, it reduces that price by 16 percent, while with a 6 percent discount rate it reduces the net price by 30 percent.

To analyze the cyclical impact of the funds system on investment in structures, Taylor develops a theoretical model of the timing of investment. The model assumes that there are several types of capital with different, but fixed, "gestation lags" between the start of construction and its completion. Firms' desired capital-output ratio for each type of capital depends on the cost of capital, and hence on whether there is a funds release. Firms therefore need to forecast future aggregate demand in deciding how much capital they will begin to construct today for two reasons: future aggregate demand will directly affect the demand for their own output, and it will also affect their estimates of the cost of capital they will face when the investment actually takes place. Taylor assumes that reductions in current demand convey information about future demand and therefore discourage firms from starting capital construction. But with the funds system in effect, reductions in future demand may also lower the cost of capital, thereby offsetting the direct demand effect. Construction continues over several periods, so investment turns out to depend on changes in output over several past periods—a distributed lag accelerator equation.

The reduced form of this model, which is appropriate for statistical estimation, has coefficients that are a composite of the structural parameters of the model. One of these relates the cost of capital to the level of output in a stylized representation of the investment funds system in operation. Intuitively, one would expect the system to reduce the size of the accelerator coefficients for investment by offsetting the natural tendency for investment to move procyclically. The system might have a perverse effect, making investment more procyclical if firms, anticipating a recession, delayed their investment at the onset of recession; but Taylor argues that such destabilization is unlikely to outweigh the stabilizing effects of the system. He estimates the reduced-form accelerator equation for Sweden and indeed finds low and statistically insignificant coefficients, evidence that the funds system stabilized investment over the business cycle. For comparison, Taylor shows that investment in manufacturing structures in the United States was considerably more procyclical, and the same is true of Sweden in the late 1970s

after the investment funds system became a permanent investment stimulus and no longer operated as a countercyclical rule.

To estimate the quantitative effect of the investment funds system, the author uses independent estimates of the Swedish capital-output ratio, the discount factor, and other structural features of the model in order to calculate the role of the funds system from the reduced-form coefficients of the accelerator equation. He finds that Swedish investment would have been substantially more procyclical in the absence of the investment funds system, but that the sum of the accelerator coefficients would have been at most one-half of the estimated sum of the corresponding coefficients in the structures equation for the United States. Thus the investment funds system does seem to have stabilized Swedish investment in the 1950s and 1960s, but it is not solely responsible for its good performance during this period. Compared to the United States, Sweden had a more stable overall economy, making fluctuations in current output less informative about future output. Because firms had less reason to project continuing good or bad times from current experience, they would have less reason to alter investment plans in response to current conditions, and investment would thus have been less cyclical than it was in the United States even without the funds systems.

THE USE of conventional econometric models to guide policy has recently come under serious attack. Such models typically combine a large number of equations that attempt to capture the structural relations describing the response of the private sector to economic developments and to policy changes: the response of consumption to personal income and taxes, the response of business investment to interest rates, capacity utilization and other determinants, and so forth. Policy actions are modeled as exogenous changes in variables taken to be under the control of the policymaker, and their effects are evaluated by tracking their impact through the estimated model. The rational expectations critique of this procedure suggests that the effect of current policy actions on the economy depends importantly on private agents' expectations about future policy actions. Conventional models, which do not take this expectations mechanism into account, may produce erroneous estimates of the structural equations of the economy and misleading forecasts of

the effects of policy. As a corollary, some rational expectations theorists have argued that policies can only be evaluated, and hence their consequences predicted, when they take the form of fixed, publicly known rules; for only then is it possible to be confident about the public's expectations about policy and, therefore, their reactions to it.

In the third article of this issue, Christopher A. Sims argues that the importance of the rational expectations critique has been overstated and that the approach to policy analysis that rests on evaluating the consequences of fixed policy rules and changes in those rules has limited applicability. However, he also stresses that econometricians make unjustifiable claims for conventional models and proposes an alternative method of econometric policy analysis. He uses his method to analyze the Reagan administration's economic forecast, as presented in the 1982 *Economic Report of the President*.

Sims is critical of the specification and estimation of large-scale econometric models. He argues that the restrictions embodied in these models should not be taken as embodying structural knowledge and are of little use in the identification of structure. Nonetheless, he believes the models provide a valuable summary of historical experience as well as useful forecasts and that the restrictions themselves improve the forecasting performance of the models. Sims also regards existing models as deficient for failing to take into account the response of policy to economic developments. By treating policy variables as exogenous, when in fact policy responds to economic developments in a nonrandom way, the estimates of the effects of policy will be erroneous, confounding the response of economic variables to policy with the response of policy to those variables.

Sims accepts in principle the rational expectations critique that conventional models fail to take into account the effect of policy intervention on the structure of such models. But he argues that this failure is not as devastating as the rational expectations critics believe, and he is critical of the remedy they propose. They argue for estimating structural models that explicitly identify policy rules and for assuming that private agents understand and optimally respond to those rules and to changes in them. In their view, policy analysis is a choice among rules of behavior for the policy authority. While Sims grants that the rational expectations assumptions might be useful in analyzing the long-run effects of moving from one fixed rule to another, he believes such rule

changes are rare and not the most important type of policy choice. He goes on to argue that it is implausible to think that private agents will believe an announced change in rule and rapidly adjust their expectations and behavior to such announcements.

Sims offers reasons why historically announced changes in policy have turned out to be misleading and why announcements of permanent changes in rules are not credible. First, announced changes are not likely to be permanent because of the nature of the political process in a democracy. Policies are framed not by one individual, but by the imperfectly predictable political interaction of many different policymakers. Even if each policymaker steadfastly attempts to follow a particular rule, this does not mean that the particular rule will be adopted or adhered to. The public is aware of this, and treats even confident announcements of rule changes with skepticism. Second, announcement of a rule may create benefits for policymakers, while subsequent implementation of the rule imposes cost. In such cases, policymakers will have an incentive to abandon previous commitments. The public, anticipating this behavior, will distrust the announcement itself. Third, rational policymakers themselves are likely to change their actual behavior slowly as information about the consequences of their actions and changes in the economy's behavior gradually accumulate. Hence even if private agents had the computational capacity to absorb and quickly act on an announcement of new policy plans or rules, they should not be expected to do so. Sims concludes that the rational expectations critique of econometric policy analysis "is a cautionary footnote . . . rather than a deep objection to its foundations."

The observations that the rules governing policy are likely to change gradually and that the public's perception of those rules will change even more gradually lead Sims to the belief that the relations linking particular policy actions to economic performance will themselves change only gradually through time. There is no more reason to expect discontinuous jumps in the economy's response to policy actions because of expectational effects than there is to expect such jumps to follow from gradual changes in technology or institutions.

The complexity of the interaction among policy, expectations, and private behavior illuminated by rational expectations theory has made it apparent that the problem of specifying and identifying structural models is even more difficult than most economists had thought. Indeed,

Sims argues that it is impossible to tell from historical data which variables can be treated as exogenous policy variables. Economists have long been aware of the problem that variables such as the money supply or interest rates may be moving in response to contemporaneous economic conditions rather than vice versa; the newer rational expectations models provide examples of how they may respond to expectations of future events. Sims shows that purely statistical tests, such as those for Granger causality—temporal precedence among variables—cannot adequately deal with the problem. He provides a theoretical example in which the money supply responds passively to demand. Yet because the demand for money reflects optimizing behavior of households adjusting their holdings to changes in expected future income that are essentially random, money is its own best predictor and may even aid in the prediction of, and appear to cause, future GNP. Sims also reports empirical tests of the causality between money and GNP and shows the results are sensitive to which variables are included. In particular, the Granger causality of money vanishes once interest rates are included in the model as an additional variable.

Sims's general skepticism about structural restrictions, his belief that causal policy variables cannot be identified, and his belief that structure changes slowly all lead him to propose what he calls "reduced-form policy analysis." In this approach the model of the economy allows all variables to respond to their own past values or past values of other variables in the system. Variables such as the money supply, conventionally regarded as a policy variable, are not treated differently from other variables, and policy actions are regarded simply as disturbances to particular equations. This framework serves two main purposes: it can be used to make forecasts of economic developments, conditional on disturbances in any variable; and it can be used to assess policymakers' forecasts of future developments, conditional on their proposed policies. A forecast that turns out to imply many large deviations from the historical pattern can be regarded as implausible. Policy announcements that themselves appear far from historical experience are not likely to be carried out and have uncertain effects.

Sims implements his approach by estimating a six-variable quarterly vector autoregressive (VAR) model for the U.S. economy. Similar models have been estimated before by Sims and by Fischer (*BPEA, 2:1981*), but in this example Sims allows the model's parameters to drift

over time. A technique known as the Kalman filter is used to update the parameter estimates period by period throughout the sample.

For his VAR model, Sims uses variables that are central to the Reagan administration's economic forecast for 1982 through 1984, conditional on its policies: the money supply (as measured by M1), the GNP deflator, real GNP, and federal expenditures and revenues, all expressed as growth rates, and the three-month Treasury bill rate. He compares projections from his model with the administration's forecast and calculates that pattern of disturbances to his projections that would produce the forecast while minimizing the deviation of the projections from historical experience. In this way he finds the least unlikely combination of developments that could produce the administration's forecast.

The pattern of disturbances indicates a systematic optimistic bias in the administration's forecast. Real GNP, for example, is repeatedly higher than history would predict. Furthermore, disturbances do not conform to what would be expected by the rational expectations-monetarist view of the world. In that view, the expectation of credible monetarism would quickly extinguish inflation, bring down interest rates, and permit rapid real output growth without an exceptional increase in the velocity of money. In the projections, rapid increases in the velocity of money accompany slowly falling interest rates and only a sluggish decline in the inflation rate. Sims concludes that the administration's forecasts for real GNP and inflation, given the policy variable projections, offer convincing evidence that the proposed policy is not likely to have its intended effects and that the administration's projections of the policy variables are themselves implausible.

In the first report in this issue, Daniel J. B. Mitchell examines recent union contract concessions, some of which—particularly in the trucking and automobile industries—have gained wide publicity. Although the concessions have occurred in distressed industries in which workers were threatened by permanent loss of jobs, they have given rise to the hope that there may now be a sharp drop in general wage inflation. Mitchell reports forty-six such cases of concession in the period from 1979 through early 1982. Most of these resulted in lower wage increases or work-rule relaxations that slowed the increase in unit labor costs for the firms affected. Twenty-one actually involved wage cuts and thirteen, wage freezes. However Mitchell estimates that less than 2 million

workers out of a work force of approximately 100 million have been directly affected by concessions. Thus even with some spillover to the wages of nonunion workers in the same firms, the concessions would only reduce the economy-wide wage inflation in 1982 by a few tenths of a percentage point. Although Mitchell does not conduct a formal test, he does not believe the concessions have had a significant effect on wages elsewhere in the economy. Through the first quarter of 1982 first-year median union wage settlements throughout the economy showed little slowdown, with no slowdown at all compared to 1981 in manufacturing settlements. Wage increases for both unionized and nonunionized workers have moderated, but Mitchell judges the moderation is no greater than one would expect from the deep recession and the slowdown in the consumer price index that has occurred.

The concessions that have already been made would have greater significance if they permanently altered wage-setting practices so as to make wages more responsive to economic conditions. But Mitchell concludes, on the basis of historical experience, that the process of union wage determination is unlikely to be permanently changed by the current concessions. Perhaps surprisingly to the nonspecialist, he reports that "give backs" in wages and benefits are not unprecedented. Episodes of such concessions occurred immediately after the Korean War and in the late 1950s and early 1960s when, as now, bad times for some firms or industries threatened the jobs of senior workers. These earlier concessionary episodes were marked by the same unusual combination of management aggressiveness and experiments in labor-management cooperation that have characterized recent experience. But, Mitchell observes, when economic conditions improved, traditional bargaining behavior returned. He expects collective bargaining agreements will continue to be negotiated for two- or three-year terms, and unscheduled contract reopenings will remain rare except when there is threat of permanent plant shutdowns. Hence he does not expect more rapid response of wages and benefits to economic conditions in the future than in the past. Because he sees no permanent changes coming from recent concessions, Mitchell advocates government initiatives to promote a greater responsiveness.

MONETARY POLICY has embarked on a determined course of disinflation, and most observers regard the present deep recession as a consequence

of that policy. These developments highlight a central question in macroeconomics: how great a cost in terms of loss of output is required to stop inflation with such a policy? In the second report of this issue, Robert J. Gordon and Stephen R. King compare projections of this cost obtained from two alternative econometric strategies for estimating the response of inflation to changes in monetary policy: the traditional structural approach that is used in most econometric models and the technique known as vector autoregression (VAR). The VAR models are often advocated because they are less dependent on prior beliefs about how the economy works. The authors, proponents of the traditional structural approach, argue that prior knowledge is valuable. They point out that because the amount of data available for use in estimation is limited, both techniques have to impose restrictions on what enters the statistical estimation, but do so in very different ways. The structural approach relies on theory and general observation about how the economy operates to choose what variables are excluded from particular equations and to restrict the lag structure relating one variable to another. This approach also allows information about special developments, such as price controls, to enter the model explicitly. By contrast, the VAR technique explores the historical relations among economic variables without imposing restrictions based on prior knowledge or belief about how they are related: typically many lagged values of every variable are used to explain every other variable. This means VAR models can include only a very limited number of variables.

The authors summarize the costs of disinflation by calculating, for projections from each of the models they consider, a discounted sacrifice ratio—the ratio of the discounted future output loss to the long-run reduction in the inflation rate. Using one traditional model that they prefer, they estimate that a steady disinflationary monetary policy, similar to the stated policy of the administration and Federal Reserve, would achieve a long-run reduction of 5 percentage points in the inflation rate by sacrificing output with a present value of 29 percent of a year's GNP. Thus the sacrifice ratio estimated with this model is about 6. In other structural models they consider, the estimated sacrifice ratio for the same disinflation policy varies between about 4 and 10. Those models that assume that the exchange rate and energy prices are influenced by monetary policy result in the lowest sacrifice ratio.

Gordon and King find that estimates of the sacrifice ratios for VAR

models are extremely sensitive to which variables are included, ranging from practically zero to 34. In contrast to their traditional model, the VAR models allow money to explain inflation directly. However, money is generally unimportant in the VAR price equations and this treatment of money is not responsible for the low sacrifice ratios found in some of their projections. The authors regard these VAR models as unsatisfactory because, in their view, the model's projections are highly unrealistic. In the discussion at the meeting Christopher Sims argued that more sophisticated techniques than those used by the authors need to be employed in utilizing VAR models for forecasting.

In the third report of this issue, David E. Lindsey examines several recent developments in financial markets that have rekindled the debate over the appropriate conduct of monetary policy. In the first half of 1982, interest rates have stubbornly remained high despite slowing inflation and deepening recession. The growth of NOW accounts (negotiable orders of withdrawal), money market mutual funds, and sweep accounts have reopened the question of how to define money for the purpose of targeting Federal Reserve policy. And the great volatility of both interest rates and money growth in recent quarters has raised questions both about the usefulness of intermediate monetary targets for conducting policy and about how closely policymakers should control monetary aggregates if they are used as targets.

Lindsey points out that different, unusual factors have recently pushed the velocity of M1—the ratio of GNP to M1—in opposite directions. Money market mutual funds, which are not counted in M1, have grown dramatically. If even a small portion of these accounts substitutes for traditional transactions balances, M1 velocity will have increased noticeably on that account. On the other hand, velocity growth was presumably diminished by a shift into NOW accounts (and other new forms of checkable deposits) from non-M1 sources, a shift that added an estimated $12¼ billion to M1 during 1981, raising the M1 growth rate by about 2¾ percentage points that year. As a recent example of the erratic behavior of M1 and velocity, Lindsey shows that his preferred equation—one that predicted annual M1 growth well through 1981 using prices, interest rates, and output—predicted an annual rate of M1 growth of less than 1 percent in the first quarter of 1982 when its actual growth rate was over 10 percent. Implicitly, this equation predicted the average level of

interest rates well through 1981. But the combination of high interest rates and rapid money growth in early 1982 implies that demand for money has increased in a way not predicted by the equation and not allowed for in the Federal Reserve's target for money growth. With financial innovations continuing, Lindsey cautions against relying on the velocity of money to move predictably in the future.

Despite the difficulty in predicting velocity, or equivalently the relation between money growth and nominal GNP, Lindsey supports the general strategy of targeting growth of the monetary aggregates for the intermediate run. He believes this strategy is the best available protection against a monetary policy that might be inflationary in the long run or procyclical over the business cycle. But he considers and rejects the arguments of some economists for tighter control of money growth rates over periods as short as a week, a month, or even a quarter. He argues that such tight control would make interest rates more volatile. And he urges policymakers to use their judgment to adjust monetary targets in response to persuasive evidence that the money-demand function in the economy has shifted.