

## THE IMPORTANCE OF STABLE MONEY: THEORY AND EVIDENCE

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The importance of monetary stability derives from the significant independent influence of monetary change on the subsequent course of economic activity. If money did not matter at all or were of only secondary importance in affecting the flow of spending, income, and prices, monetary stability would be of little relevance.

Our views reflect theoretical models and the empirical evidence testing them that establish a close relation between economic stability and monetary stability, and between inflation and monetary growth in excess of the rate of real growth. Hence a stable monetary environment is crucial to achieve economic stability encompassing both stable prices and real growth immune to wide swings. The essential element required to generate a stable monetary environment is systematic policy, so as to minimize monetary shocks to the expectations of economic agents. Discretionary policy is unsystematic, hence fails this test. Increasing the variability of money growth in an attempt to fine-tune the economy will make the variance of real output greater than it would otherwise have been. An economy in which countercyclical policy is followed will end up with unstable money and unstable real output.

Postwar developments in monetary theory have shifted the issues that were the original centerpieces of analysis supporting the case for stable money. Correspondingly, the kinds of evidence suggested

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to test the analysis have changed to reflect the nature of the issues that are highlighted. We examine the developments in chronological order, beginning with Friedman's case for stable money, based on a theoretical argument against the pursuit of countercyclical stabilization policy (section I). Section II then examines the opposing theory of economic policy associated with Theil and Tinbergen and the Phillips curve analysis. That theory holds that countercyclical policy can be employed to stabilize the economy and that stable monetary policy is not decisive for that purpose. Successful countercyclical policy would achieve a standard deviation of money growth that would precisely offset the standard deviation of real economic growth that would otherwise occur, and thereby reduce the variance of real output below that of money. The section concludes with a discussion of the natural rate hypothesis that was the culmination of the Phillips curve analysis. The latest development we cover is the rational expectations hypothesis (section III). In each section we examine the implications of the theory for the stable money view and report the available evidence. In section IV we summarize the case for a legislated rule and present some new evidence for a monetary growth rule. Section V concludes with a brief discussion of the role that a constant monetary growth rule plays in the views of the schools of global monetarism, of Austrian economics, and of the new monetary economics.

## I. The Case Against Discretionary Monetary Policy

The general case against discretionary monetary policy formulated by Friedman (1953) is that, to function well, stabilization policy must offset random disturbances to economic activity; that is, it should remove the variation in income due to those disturbances. To achieve such a goal, two conditions must be satisfied: one involving timing, and the other involving the magnitude of the policy action. The timing of the policy action should conform to that of the disturbance, and the size of the policy action should be congruent with the size of the disturbance. If both conditions are not satisfied, the policy response will be insufficient and may even be destabilizing.

Friedman (1948, 1953) went on to argue that the lags in the effect of discretionary monetary policy are likely to be long and variable, reflecting both an "inside lag"—the time that elapsed before the monetary authority responded to the disturbance—and an "outside lag"—the time that elapsed before changes in monetary growth affect economic activity. As a result, discretionary policy actions might exacerbate rather than mitigate cyclical disturbances. In addition,

Friedman contended that there was no basis for believing that policymakers (and the economics profession) possess the detailed knowledge of the economy's complex interactions and of the lag structure requisite for the pursuit of successful countercyclical policies or for fine-tuning. Furthermore, in his view, even well-meaning monetary authorities were likely to respond to political influences. Politically advantageous, short-run actions by the authorities would ignore the long-run destabilizing consequences. The conclusion Friedman drew from this array of circumstances and from evidence to be considered in what follows was that monetary policy should be based on a legislated rule instructing the Federal Reserve to increase the quantity of money, or high-powered money, on a year-to-year basis at a steady known rate of growth.

Friedman did not allege that such a prescription would yield nirvana. He allowed for the possible accretion of knowledge of the operation of the economy once the rule was adopted that would permit improving it. Adoption of the rule would not eliminate cyclical change, but the rule would remove disturbances arising from erratic fluctuations in the supply of money. The effect would be to reduce the amplitude of the random shocks to real economic growth inherent in the operation of the economy.

Several types of evidence have been used to evaluate the case for a monetary rule, namely: the statistical record of changes in money growth rates and their relation to changes in economic activity; qualitative historical data; and simulations of the hypothetical path of economic activity under an assumed monetary growth rule, compared with the actual path. We first report the statistical and historical evidence.

One body of evidence, of which Clark Warburton was the author, predated Friedman's theoretical case against discretionary monetary policy. Warburton's writings from the early 1940s, when the Keynesian revolution was in full swing, until the end of his life in 1980, were in the quantity of money tradition and stressed the importance of monetary disequilibrium as the fundamental cause of business fluctuations. At the time that Warburton's views first appeared, attention to the role of money had all but vanished from professional work. His main evidence was based on deviations from trend of quarterly money data for the period 1918–65. He demonstrated that turning points in money preceded those in business, and concluded: "[A]n erratic money supply [was] the chief originating factor in business recessions and not merely an intensifying force in the case of severe depressions" (1966, Intro., p. 9). Warburton also cited, as prime examples of the harmful effects of discretionary policy, the mistakes of the

Federal Reserve System that produced the great contraction of 1929–33 and the contraction of 1937–38:

Since the time of the establishment of the Federal Reserve System, annual deviations in the quantity of money from a reasonable rate of growth have ranged from more than 30 percent excess to nearly 20 percent deficiency. There is no known need for annual variations in the quantity of money, from the estimated reasonable rate of growth, of more than 2 percent, and annual variations in the quantity of money outside this range have been invariably associated with business instability and with inflation or depression. The range of additional variation for seasonal purposes is probably not more than three percent. (1966, chap. 17 [1952], pp. 368–69)

The dismal record of the Federal Reserve led Warburton to strongly favor a legislated monetary rule that would limit the growth rate of money, for a given definition, to three percent per annum.<sup>1</sup>

The evidence provided by Friedman and his associates also utilized statistical and qualitative historical data. Unlike Warburton, who expressed the data as deviations from trend, Friedman and Schwartz used first differences of the logarithms of the money series. They then selected turning points in the series from 1867 to 1960, and compared the peaks and troughs in the percentage rate of change of the money stock with peaks and troughs in general business as dated by the National Bureau of Economic Research reference cycle chronology. On average, of the 18 nonwar cycles since 1870, peaks in the rate of change of the stock of money preceded reference peaks by 16 months, and troughs in the rate of change of the stock of money preceded reference troughs by 12 months. On this basis, they argued strongly that: “Appreciable changes in the rate of growth of the stock of money are a necessary and sufficient condition for appreciable changes in the rate of growth of money income”; and, “this is true both for long secular changes and also for changes roughly the length of business cycles” (1963a, p. 53). Using a different methodology over the same period, William Poole (1975) found that the evidence supported the Friedman and Schwartz conclusion.

To the question whether money changes conformed positively to the business cycle with a lead or inversely with a lag, the answer Friedman and Schwartz gave was that the dispersion (measured by the standard deviation) of the leads and lags, as computed under the

<sup>1</sup>Warburton (1964, p. 1328). In earlier studies, in the 1940s and 1950s, Warburton advocated a five percent annual growth rate in the money stock, inclusive of an adjustment for a projected steady secular decline in velocity of 1.5 percent per year. The shift to a lower proposed growth rate for money incorporated the assumption that the reversal in the trend of velocity in the 1950s—from negative to positive—would continue.

two interpretations, is uniformly lower when the money series is treated as conforming positively. Serial correlations, furthermore, of expansions with succeeding contractions and of contractions with succeeding expansions display the same patterns for the money change series and a proxy indicator of physical change in general business. Expansions in both series are not systematically correlated with the succeeding contractions, whereas contractions in both series are highly correlated with the succeeding expansions. This evidence supports the positive interpretation of the relation of money *changes* to the business cycle. Otherwise, if inverted conformity were the case and changes in business produced later changes in the opposite direction in money, then the correlations with the succeeding reference cycle phase for money and the physical change in general business measure should be opposite. But the pattern for business *does* reflect, with a lag, the pattern for money.

Statistical evidence provided by Friedman and Schwartz (1963b, p. 594) matched periods with a low standard deviation of year-to-year percentage changes in monetary growth with comparable periods in velocity, real income, and wholesale prices. They also matched periods with a high standard deviation of year-to-year changes in monetary growth with comparable periods in the other magnitudes. In the nine decades, 1869–1960, four periods of comparative stability in money growth were accompanied by relative stability of the rate of growth of output and the rate of change of prices: 1882–92; 1903–13; 1923–29; 1948–60. All other periods were characterized by unusually unstable money growth rates and unusually unstable rates of growth of output and rates of change of prices.

The qualitative historical evidence that Friedman and Schwartz examined also supported the conclusion that erratic money changes, as a result of discretionary actions by the authorities, were accompanied by economic changes in the same direction. Moreover, in a number of episodes when monetary changes had led changes in economic activity, the evidence that the monetary changes were independent of the changes in activity was irrefutable.

We now turn to the simulation studies that compare the hypothetical behavior of the U.S. economy under an assumed constant money growth rate rule with actual economic performance. The evidence is mixed. Friedman (1960) found that a rule would have outperformed discretionary policy in the interwar period, but that the case for the post-World War II period was less clear-cut. For the postwar period, at least until the mid-1960s, most studies (Bronfenbrenner 1961; Modigliani 1964; Argy 1971) concluded that discretionary policy outperformed a 3 or 4 percent monetary growth rule. One inference

might be that the Federal Reserve had learned from its "mistakes" in the interwar period. Recently, however, Argy (1979) found that for the period from the late 1960s to the late 1970s, a simulated monetary growth rule for a sample of nine industrial countries would have reduced the variance of real growth considerably below its actual variance.

Finally, Kochin (1980) found that over much of the postwar period U.S. monetary policy was destabilizing. His study, based on an interpretation of the results of several economic models, followed Friedman's (1953) procedure for evaluating stabilization policy.

## II. Keynesian Riposte and Return Sally

An analytical development that favored intervention along Keynesian lines was the Theil-Tinbergen theory of economic policy. That approach provided policymakers with an array of instruments—monetary, fiscal, incomes policies—to achieve multiple goals by matching instruments to goals following the principle of comparative advantage. This theory of economic policy combined with the use of optimal control procedures led to a strong case for fine-tuning. It was held that policymakers could devise feedback rules between real economic activity and monetary and fiscal policy that could be applied to offset disturbances to the private sector.

Another development that apparently advanced the case for countercyclical policy was the Phillips curve tradeoff. Phillips (1958), Samuelson and Solow (1960), and Lipsey (1960) reported evidence of a stable inverse relationship for the U.K., the U.S., and other countries between the rate of change of money wages (alternatively, the rate of change of the price level) and the level of unemployment. The findings led to the view that policymakers could choose, based on a social preference function, between high inflation and low employment, or low inflation and high unemployment, the desired choice to be achieved by discretionary monetary and fiscal policy.

The upshot of these developments was that many economists came to believe that the economy could be stabilized at any desired level of activity. Friedman's objections to fine-tuning seemed to have been circumvented.

Friedman's response came in his 1967 presidential address to the American Economic Association. He argued that the Phillips curve tradeoff was a statistical illusion arising from the failure to account for inflationary expectations. Monetary and fiscal policy could stabilize the economy at some arbitrary level of output or employment, but only temporarily and, even then, only at the expense of acceler-

ating inflation or deflation. Both Friedman (1968) and Phelps (1968) modified the Phillips curve approach by applying the concept of the natural rate employment—that rate consistent with the microeconomic decisions of firms and workers active in the labor force. The natural rate of employment reflects the *optimal choice* of workers between labor and leisure and the optimal mix of labor and other factors of production for firms in a dynamic economy. According to the “natural rate hypothesis,” the natural rate of employment is determined by the intersection of the demand and supply curves for labor, given demographic factors and labor market institutions. Hence *deviations of employment* from the natural rate are produced only by imperfect information and the costs of acquiring information that affect job search.

One explanation given for such imperfections in information was that employers and workers have different perceptions of changes in real wage rates. It was argued that firms always have *perfect information* on the prices of their output so that for them actual and expected real wages are always equal. In contrast, workers base their evaluations of prospective real wage rates on their *expectations* of what the rate of inflation will be over the duration of their contracts. For example, suppose inflation is rising and workers’ expectations do not fully reflect the *higher inflation rate*. Faced with lower real wage rates, firms will be willing to expand employment, which will put upward pressure on nominal wages. The result will appear as a *movement along the (short-run) Phillips curve*. However, once workers adjust their expectations to the higher inflation rate, they will demand higher money wages. The resultant rise in real wage rates will cause firms to reduce employment to its previous level. The economy will then return to the natural rate of unemployment consistent with labor market forces, *but at a higher rate of inflation*.

The *measured* unemployment rate is thus assumed to depend on the natural unemployment rate and the difference between the actual and expected inflation rates; that is, on the *inflation forecast error*, with some rate of adjustment of the unemployment rate to the forecast error. As long as the actual and expected inflation rates differ, measured unemployment *can differ from the natural rate*. However, in the *long run*, actual and expected inflation rates converge, and hence, measured unemployment reverts to the natural rate, though this *adjustment process* may be sluggish.

The theory of search is an alternative way of explaining unemployment. This theory posits that the natural rate of unemployment is determined by long-run demographic forces, but that deviations

from the natural rate are caused by short-run factors affecting the costs and duration of search.

The policy implication that emerged from the natural rate hypothesis was that stabilization policies aimed at reducing unemployment below the natural rate would have only temporary success. Any attempt to achieve permanent results would produce accelerating, and ultimately runaway, inflation. In addition, policies designed to peg the unemployment rate at the natural rate could lead easily to an explosive inflation or deflation if the forces determining the natural rate were to change. Such forces include changes in the labor force skill mix and demographic determinants of the labor force. Thus, the natural rate hypothesis strengthens the case for monetary stability, since monetary instability would produce deviations between the expected and actual inflation rates, causing fluctuations in unemployment and output.

### III. The Rational Expectations Hypothesis and the Case for Stable Money

Recent advances in the treatment of expectations supplement the case for monetary stability implied by the natural rate hypothesis. According to the rational expectations hypothesis, economic agents act rationally with respect to the gathering and processing of information, just as they do with respect to any other activity (Muth 1961). This proposition implies that agents will not make persistent forecast errors. If their forecasts turn out to be wrong, agents will learn the reason for their errors and revise their methods of forecasting accordingly. Such an approach seems more reasonable than alternative approaches commonly used to model expectations, such as static expectations that simply extrapolate existing conditions, or adaptive expectations that have the property of yielding continuous forecast errors. Additionally, in contrast to the adaptive expectations approach that uses only past values of the variable about which expectations are to be formed, the rational expectations hypothesis also uses other relevant information.

The rational expectations model assumes that private agents form expectations about the rate of inflation based on their understanding of the economic model that generates the inflation rate, as well as on the policy rule followed by the monetary authorities.

In a model based on rationally formed expectations, Sargent and Wallace (1975) demonstrated that systematic monetary policy would be completely ineffective in influencing real variables. They argued that if the monetary authorities devised a monetary feedback rule,



using optimal feedback techniques, according to which the authorities systematically altered the money supply to offset disturbances in real economic activity, then private decision-makers would learn the rule and incorporate it into their rational expectations. The thrust of this model—where deviations of output from its full employment (or natural) level can only be produced by an inflation forecast error—is that if expectations are formed rationally, the forecast error cannot be manipulated by systematic (and, therefore, anticipated) monetary or fiscal policy. Indeed, the *only* way output or unemployment can be altered from its natural rate is by an *unexpected* shock. However, *unexpected shocks*—monetary or other—have the negative attribute of increasing the level of uncertainty in the economy.

If a negatively sloping Phillips curve were observed, it might result from constant price expectations in a period with *ex post* fluctuations in actual inflation due to unanticipated random shocks that are negatively correlated with *ex post* fluctuations in measured unemployment (Begg 1982, p. 141). Lucas (1973) offered a variant explanation, in a world of rational expectations, for a negatively sloped short-run Phillips curve or, alternatively, a positively sloped short-run supply curve for output, which is determined by *lagged output* and the discrepancy between actual and expected inflation. Lucas assumed that the economy is characterized by *uncertainty*, and that competitive firms cannot readily discern whether a change in the price of their output reflects a change in the price level or a change in relative prices. He then demonstrated that other things equal, the greater the variance of the aggregate price level, owing to greater monetary variability, the more likely it is that firms will mistake a price level change for a change in relative prices. Expansion of output in response to an increase in the level of prices, holding relative prices constant, will ultimately lead to accumulation of inventory, cutbacks in output, layoffs, and more inelastic supply curves and also a more inelastic aggregate supply and Phillips curve. In addition, greater price level variability will be associated with greater resource misallocation because price level variability impairs the ability to perceive the information that prices convey in a market economy.

Brunner, Cukierman, and Meltzer (1980) perceive the problem of extracting the signal from prices somewhat differently from Lucas. For them, the distinction that needs to be made is not the sorting out of aggregate from relative price changes. It is rather the distinction between transitory and permanent price changes. Firms will wait to learn whether a change is permanent before reacting to it and, with great price variability, that process is made more difficult and prolonged than would otherwise be the case.

In any event, price variability reflecting discretionary money variability clearly has negative effects on the economy and reinforces the case for monetary stability. Moreover, the entire enterprise of selecting discretionary policies by simulation of econometric models has been challenged by Lucas (1976). His critique was based on the kinds of equations that are used in econometric models. These are reduced forms of effects on the economy of existing policy arrangements that incorporate the private sector's expectations of policy effects on economic variables. Were the authorities to change the policy rule, the public would adjust its expectations accordingly. Consequently, attempts to forecast the effects of alternative policies without accounting for changes in private agents' expectations are bound to lead to inappropriate policies.

Discretionary policy (defined as policy reacting to the current situation) based on optimal control techniques has been shown by Kydland and Prescott (1979) to be suboptimal and possibly destabilizing in a world of rational expectations. The policy chosen at each point in time may be the best, given the current situation. In the authors' terminology, the policy may be consistent, but it will be suboptimal because the policymaker has failed to take into account the optimizing rules of economic agents. The decisions of agents will change as they come to recognize the change in policy. The example Kydland and Prescott cite is that agents may expect tax rates to be lowered in recessions and increased in booms and make decisions in light of those expectations. Over successive periods, it is not optimal to continue with the initial policy because control theory is not the appropriate tool for dynamic economic planning. Current decisions of economic agents are affected by what they expect future policy to be. A government that attempted to reduce unemployment by increasing the money supply without attention to the rational inflation expectations of private agents would end up with a suboptimal mix of the natural rate of unemployment and positive inflation, despite the fact that it sought to maximize its "social welfare function" by combining the desirability of full employment and zero inflation. The authors conclude (1977, p. 487):

The implication of this analysis is that, until we have . . . [a tested theory of economic fluctuations], active stabilization may very well be dangerous and it is best that it not be attempted. Reliance on policies such as a constant growth in the money supply and constant tax rates constitute a safe course of action. When we do have the prerequisite understanding of the business cycle, the implication of our analysis is that policymakers should follow rules rather than have discretion. The reason that they should not have discretion is

not that they are stupid or evil but, rather, that discretion implies selecting the decision which is best, given the current situation. *Such behavior either results in consistent but suboptimal planning or in economic instability.*

Oversimplification by certain proponents of the rational expectations hypothesis should be noted. A number of factors could lead to nonneutral effects of anticipated monetary growth even in the presence of rational expectations. First, anticipated monetary growth can have effects on the natural rate of unemployment (output) through a real balance effect on the aggregate expenditure function, or by changing the steady state capital-labor ratio and thus affecting the real rate of interest (Buiter 1980).

Second, if the assumption that both government and the private sector have equal access to information is violated when there is a rule for systematic monetary policy, then it is possible for the government to change its policy after the private sector has formed its expectations and thereby affect the inflation forecast error. As a result, output and unemployment can deviate from the natural rate. Such an outcome is also possible in cases where wages are determined by multi-period overlapping contracts (Fischer 1977). In that situation, even if private agents form their expectations rationally, the government can systematically affect output and employment between contract negotiating dates. Third, if the assumption of market clearing is abandoned, yet the assumption of rational expectations is maintained, then it is possible for output to be affected by stabilization policy. Explanations for price stickiness range from the Keynesian disequilibrium approach (Buiter 1980) to price setting behavior in a world of high coordination costs (Cagan 1980).

Fourth, evidence of persistence—that unemployment does not rapidly disappear and bring the economy to full employment—or alternatively, the existence of serial correlation of output and employment over the business cycle, has been advanced as contradicting the rational expectations approach. On the other hand, McCallum (1980) explains persistence within the rational expectations context as reflecting real costs of adjusting the fixed capital stock and other factors of production. For Lucas (1975), persistence occurs because of information lags that prevent “even relevant past variables from becoming perfectly known” (p. 1114), and an accelerationist effect of physical capital. Finally, the rational expectations approach fails to explain how private agents learn from their forecast errors in forming rational expectations (De Canio 1979).

We now turn to the evidence for the rational expectations hypo-

thesis. The evidence most generally cited is that by Barro (1977a, 1977b, 1981) and Barro and Rush (1980). Barro and Rush regressed the unemployment rate over the 1949–77 period on lagged values of a measure of unexpected monetary growth and of expected monetary growth. Expected monetary growth was estimated from a regression of current monetary growth on past monetary growth, the deviation of government spending from its trend, and past unemployment. Such a regression was designed to capture the monetary rule that economic agents perceived. The predicted values of the regression were employed to represent expected monetary growth, and the residuals, to represent unexpected monetary growth.

Barro and Rush found most of the variation in unemployment was explained by unexpected monetary growth, and that expected monetary growth was not statistically significant. They concluded that expected monetary growth is neutral and that only unsystematic elements of monetary policy affect the unemployment rate—a finding that is supportive of the rational expectations hypothesis.

The evidence that Barro has presented—that only unexpected monetary growth explains variations in unemployment—has been challenged. Cagan (1980) argued, following a more traditional approach, that most variations in output and employment can be explained by deviations in money growth from a long-run trend, without invoking rational expectations. Sargent (1976) demonstrated that it is difficult to distinguish Barro's results from those produced by a more traditional approach because of the observational equivalence of natural and unnatural rate theories. For Sargent, the only way to test a refutable hypothesis is to be able to isolate periods involving a change in clear-cut policy rules. Gordon (1976a, 1976b, 1979) argues that, unless it can be shown that the full effect of a change in nominal income is absorbed by price change, the case for the neutrality hypothesis is not confirmed. In his view, to the extent that some of the effect of expected monetary growth is absorbed by output change, scope remains for stabilization policy. Mishkin (1982) also finds that anticipated movements in monetary growth have effects on output and unemployment that are larger than those of unanticipated movements, but his evidence confirms that expectations are rational.

The rational expectations approach appears to be firmly established, despite unresolved questions including those mentioned above. A clear implication of the literature is that active monetary intervention is likely to lead to large price level changes with little favorable effect on output or employment. Unpredictable policies are likely to

increase the degree of uncertainty in the economy and enlarge the fluctuations around the natural rate. The aim of policy should therefore be to establish predictable monetary rules, preferably rules that are easily understood, with full consideration of all the relevant costs and benefits.

#### IV. The Case for a Legislated Rule

Modigliani's presidential address to the American Economic Association (1977) disputed monetarist views that (a) the economy is sufficiently shockproof that stabilization policies are not needed; (b) postwar fluctuations resulted from unstable monetary growth; (c) stabilization policies decreased rather than increased stability. He finds that "Up to 1974, these [stabilization] policies have helped to keep the economy reasonably stable by historical standards, even though one can certainly point to some occasional failures" (1977, p. 17). He attributes the serious deterioration in economic stability since 1973 to "the novel nature of the shocks that hit us, namely, supply shocks. Even the best possible aggregate demand management cannot offset such shocks without a lot of unemployment together with a lot of inflation. But, in addition, demand management was far from the best." The failure, he contends, was the result of ineffective use of stabilization policy "including too slavish adherence to the monetarists' constant money growth prescription."

Modigliani's defense of stabilization policies amounts to acknowledging specific failures while asserting overall success, except when exogenous supply shocks occur which "we had little experience or even an adequate conceptual framework to deal with" (1977, p. 17).

Table 1 shows the standard deviations of quarter-to-quarter deviations of a two-quarter moving average from a 20-quarter growth rate of M1. The standard deviations are a proxy for unexpected monetary change (shocks) that, according to both older and newer approaches, should be associated with consequent effects on real output and, once fully anticipated, on prices. The table, therefore, also shows the standard deviations of quarter-to-quarter annualized real output growth rates for three postwar subperiods: 1952I to 1960IV; 1961I to 1971III (alternatively, 1973III); 1971III (1973IV) to 1982III.

The variability of the (unexpected) money series declined moderately during the 1960s and until the quarter preceding the Nixon price controls or, alternatively, the quarter preceding the 1973 oil price shock. Over the same subperiods, real output variability also declined, but substantially more than the decline in money variability. In the final subperiods, both money variability and real output

variability rose to levels exceeding the ones prevailing in the initial subperiod.<sup>2</sup>

Modigliani's attribution of the serious deterioration of economic stability since 1973 to "too slavish adherence to the monetarists' constant money growth rule" is not apparent in Table 1. The inability of stabilization policy to cope with unexpected developments *supports* monetarist views. If policymakers are thought to have an informational advantage over private agents and so able to reduce fluctuations of output around its natural rate, they must be able to make correct inferences about the precise character of current shocks. That does not seem to be the case.

Theory and evidence strongly suggest that a systematic monetary rule is superior to discretion. A fixed rule with no feedback from the current situation to policy instruments, a rule that is simple and preannounced, is the most favorable condition for stabilizing the

TABLE 1  
Comparative Variability of Monetary Growth and Rates of  
Change of Real GNP, Postwar Subperiods Quarterly,  
1952I—1982III

Period	Standard Deviation of Quarter-to-Quarter Percentage Changes in:	
	Deviations from a 20-Quarter Moving Average of M1 of a 2-Quarter Moving Average (1)	Annualized Real Output Growth (2)
1952I—1960IV	1.93	4.76
1961I—1971III	1.80	3.17
1961I—1973III	1.75	3.20
1971III—1982III	2.11	4.79
1973IV—1982III	2.18	4.89

Note: We are indebted to the division of research of the St. Louis Federal Reserve Bank for the data underlying col. 1.

<sup>2</sup>Milton Friedman has called our attention to the similarity between the results of a table he constructed for the period 1962I through 1982IV (divided at 1971I and 1973III), and of our table. He calculated a geometric mean of 12-term moving standard deviation of growth rates of M1, M2, and real output. The increase in the variability of M1 from 1962-71 to 1971-82 of 0.26 in his table matches our finding of an increase of 0.31; the increase in the variability of real output he found of 1.61 is almost identical with the increase of 1.62 in our table. For M2, in his table, the increase in variability from the first to the second period is much sharper than for M1—1.21 compared to 0.26.

*economy.* Any feedback rules that involve government manipulation of the private sector's forecast errors is doomed to failure. There is no information available to authorities that is not also available to the private sector.

A fixed, simple, preannounced rule can take a number of forms. For some who are opposed to discretionary policy, the preferred systematic rule is the gold standard rule, for others, an interest rate or price rule. We do not examine the reason such rules have won support from their adherents. The rule we favor is a constant monetary growth rule. It satisfies the requirement for a systematic preannounced policy or regime that economic agents can incorporate in their expectations. It is a rule which can easily be implemented. The case for it, as stated initially by Friedman, is that economists lack adequate knowledge to conduct discretionary policy successfully. A monetary growth rule would obviate monetary policy mistakes. When physicians take the Hippocratic oath, they pledge not to do harm to their patients. Economists should take a similar oath with respect to the instruments that they may be in a position to administer.

The development of the rational expectations approach suggests that public response to stable monetary growth would contribute to the stabilization of the economy. Constant monetary growth will not make the business cycle obsolete. But avoidance of the mistakes of discretionary monetary policy will reduce the amplitude of fluctuations inevitable in a dynamic economy.

## V. Divergent Views on a Constant Monetary Growth Rule

*Economists who accept the primacy of monetary change in producing changes in economic activity do not all agree that the policy solution is to adopt a rule for constant monetary growth. We may distinguish the views of adherents of global monetarism, Austrian economics, and the new monetary economics.*

Global monetarism emphasizes that the world economy is highly integrated with respect both to commodity and capital markets, international price and interest rate arbitrage serving to coordinate national economies. The appropriate unit of analysis, therefore, is not the individual national economy but rather the world. The elements of the doctrine were constructed for a world of fixed exchange rates where the domestic rate of inflation is determined exogenously by the world rate of inflation, and the domestic money stock is determined by the rate of growth of domestic nominal income, set by the world inflation rate. For such an approach, prescribing a rule for

domestic monetary growth is pointless. Under a flexible rate regime, however, domestic monetary authorities can control their money supplies *if they choose*. Regardless of the exchange rate regime, global monetarism has not supported a monetary rule for a single nation.

Austrian economics acknowledges the role of monetary policy in producing inflation, and shares the monetarist view that the result of monetary attempts to reduce unemployment below its natural level is accelerating inflation. The chief emphasis, however, is less on these propositions than on the distortions in the production process resulting from monetary expansion. Moreover, in Austrian economics, flexible exchange rates are not the path to domestic monetary control. Hayek, for example, favors fixed exchange rates as a constraint on the government's overexpansion of the domestic money supply. The preferred solution, however, is the abolition of central banks, and the establishment of a commodity money. Hayek recently has advocated the denationalization of money and giving private producers freedom to offer alternative kinds of money. The market would then choose the money that would prove to be stable. Hence no legislated rule would be required.

The new monetary economics enters under the free-market banner. In the system that we are familiar with, money is the product of pervasive government regulation. Had free-market policies prevailed for transactions services, economists of that persuasion argue, a more efficient banking system would have been created, and velocity would have been much different. The new monetary economics therefore opposes a constant monetary growth rule on the ground that macroeconomic performance, under free-market provision of money, could be much better than a rule would have produced. Different schemes have been elaborated by members of this school to replace an inefficiently regulated money stock, but as Hall (1982, p. 1555) writes: "None of them would rely on the concept of a money stock or its stability relative to total income. Whether their macroeconomic performance would equal that of a simple money growth rule is still a matter of controversy."

Proposals to change utterly root and branch the existing monetary system strike us as ignoring the enormous attachment of the private sector to arrangements that have become customary. Imposing a system that appeals to visionaries as far more satisfactory than the one markets have adjusted to, given the existing network of regulations, is not the historical way in which alterations in the monetary system have occurred. A complete breakdown in existing arrangements as a result, say, of the catastrophe of hyperinflation would be



a prerequisite to adoption *de novo* of one of the schemes the new monetary economics espouses.

The new monetary economics, by proclaiming that results superior to those of a monetary growth rule are within reach, shares some of the confidence of interventionists. Advocates of a monetary growth rule are skeptical not only about demand management or fine-tuning by interventionists, but also about the prospects that new schemes for settling transactions can be as easily implemented as they can be devised.

Some observers predict that the deregulation process now under way will obscure the quality of moneyness of assets and hence render control by the central bank problematical. We regard this apocalyptic view as unduly alarmist. Not so long ago, it was commonly argued that payment of interest on demand deposits would mean the end of their use as transactions balances. That has not happened and we do not foresee radical changes on the horizon in the operation of the payments system. The alternatives are not the creation *de novo* of a set of monetary arrangements or the preservation unchanged of the existing set.

For all the talk of the adoption of monetarism by central banks, their performance gives little indication that they in fact have been influenced by the central message of the doctrine—monetary instability is a potent source of unstable economic performance. Note, for example, the wide swings that have been observed even in a smoothed two-quarter moving average of the U.S. money growth rate since 1980—1.9 percent in the second quarter, 5.8 percent in the third quarter, followed by 13.2 percent in the fourth quarter; in the four quarters of 1981, 8.1, 7.1, 4.9, 3.0 percent; and in the first three quarters of 1982, 8.3, 7.1, and 3.4 percent, with the fourth quarter figure a likely high multiple of the third quarter figure. Is this monetarism?

A legislated rule has *never* been tried. It is a modest step towards restraining monetary authorities, but both theory and evidence suggest that it could be a giant step toward achieving economic stability.

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## A COMMENT ON “THE IMPORTANCE OF STABLE MONEY”

*Gottfried Haberler*

**I**t is a great pleasure to comment on the excellent paper by Michael Bordo and Anna Schwartz. It covers a large area and a vast literature. I can touch only on some highlights.

Let me first mention areas of agreement. Surely money, monetary policy, and monetary mismanagement account for a very large part of economic instability. There has never been a significant inflation, prices rising say four or five percent for two or more years without a significant increase in the quantity of money, and practically all serious cyclical depressions have been due largely to monetary mismanagement.

The situation is a little different for mild recessions. The authors admit that when they say that the business cycle would be still there, even if the steady monetary growth rule which they recommend were strictly followed, it would presumably be a milder cycle.

The authors argue convincingly that fine-tuning will not work and they reject discretionary monetary and fiscal policies. I fully agree if by fine tuning we mean an attempt to iron out by discretionary financial measures all ripples or minor recessions in economic activity. But I submit that discretionary measures cannot be ruled out altogether.

Let me illustrate what I have in mind by the Great Depression of the 1930s.

In their “truly great book” *The Monetary History of the United States* (if I may use the words of the eminent Keynesian, Sir Roy Harrod<sup>1</sup>), Milton Friedman and Anna Schwartz have demonstrated

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<sup>1</sup>See his review of *A Monetary History of the United States*, *The University of Chicago Law Review* 32 (Autumn 1964): 186–196.

convincingly that the exceptional severity and length of the depression were due to errors of commission and omission of monetary policy—commission, deflationary measures taken during the depression (e.g., in 1931 after Britain dropped the gold standard which put pressure on the dollar)—and errors of omission, failure to stop the deflationary spiral by sufficiently large open market operations. Let me also mention that Sir Roy Harrod defended the Friedman-Schwartz view against the criticism of other Keynesians who argued that the disaster could not have been avoided by monetary measures. Sir Roy said, “Monetary policy has not been tried.”

For example, Nicholas Kaldor argued that monetary expansion would merely have led to a decline in the velocity of circulation of money and not to an increase in the flow of spending.<sup>2</sup> He tried to support his theory by pointing to Canada where there was no collapse of the banking system, but a sharper decline in velocity of money than in the United States. He did *not* point out that Canada was forced to depress its economy to about the same level as in the United States, whatever its monetary policy, because the Canadian dollar was rigidly linked to the U.S. dollar by the gold standard.

The point I want to make is that if an economy has sunk into a severe recession or depression, either as a consequence of monetary mismanagement as in the 1930s, or possibly for some other reason, there can be made a strong case for discretionary expansionary action. As far as the 1930s are concerned, I would go one step further. I think Keynes was right at that time to ask for deficit spending. Monetary expansion alone surely would have eventually led to an increase in the flow of spending, but it would have taken a long time and in the process a large amount of liquidity would have been created which later would have caused inflationary troubles. Therefore, a strong case can be made for injecting money directly into the income stream by government deficit spending. I once heard Milton Friedman say on television that in circumstances such as in the 1930s government deficit spending can be recommended.

Let me mention that these monetary causes of the Great Depression have been widely stressed in the literature. For example, Joseph A. Schumpeter, whose theory of the business cycle was not a mone-

<sup>2</sup>Nicholas (Lord) Kaldor, “The New Monetarism” *Lloyds Bank Review* 97 (July 1970). He argued that, since in July 1932 the monetary base (what Friedman calls high-powered money) was 10 percent higher than it was in July 1929, the monetary explanation of the extraordinary severity of the depression is invalid. He evidently thinks that the monetary authorities had done their duty by keeping the monetary base barely stable in the face of a massive decline of the money supply as a consequence of the stock exchange crash and several waves of bank failures.

tary one, said that the collapse of the U.S. banking system and the deflation involved "turned retreat into a rout"; what otherwise would have been a recession became a catastrophic depression.<sup>3</sup>

The second section of the Bordo-Schwartz paper deals with what the authors call the "Keynesian Riposte and Return Sally." The Keynesian riposte was the Tinbergen-Theil theory of economic policy and the Phillips curve. I shall not say much about these two theories. It would mean flogging a dead horse.

Let me make only two brief remarks. First, that there is no stable tradeoff between inflation and unemployment has been known for a long time. Long before Phillips, the Harvard economist Sumner Slichter argued that the Federal Reserve should not try to stabilize the price level but should stabilize the inflation rate at, say, 5 percent. The answer was that you cannot stabilize a significant rate of inflation or, more precisely, that a stable rate of inflation would soon lose its power to contain unemployment. Only an unanticipated *increase* of inflation could temporarily reduce unemployment.

The second remark is that one should not call the Phillips theory the Keynesian reply to Friedman's criticism of fine tuning. True, some Keynesians embraced it, notably Samuelson and Solow. But Keynes himself would surely have rejected it if he had been alive. He always was concerned with inflation both before and after the *General Theory*. For example, in 1937, one year after the publication of the *General Theory*, he wrote in three famous articles in *The Times* that inflation had again become a serious problem and that policy should be changed accordingly.<sup>4</sup> He did so despite the fact that the rate of inflation at that time was not particularly high by post-World War II standards and unemployment was still about 10 percent. The fact is that he changed his position so quickly that many of his followers could not keep pace with him.

<sup>3</sup>See *Essays of J.A. Schumpeter*, Richard Y. Clemence, ed. (Cambridge, Mass.: Addison-Wesley Press, 1951), p. 214.

<sup>4</sup>It can be argued that Keynes consistently was concerned about inflation, although in the 1930s, when he wrote *The General Theory*, he rightly concentrated his fire on deflation. Keynes's three articles in *The Times* are reprinted in T.W. Hutchison, *Keynes Versus the Keynesians? An Essay on the Thinking of J.M. Keynes and the Accuracy of Its Interpretation by His Followers* (London: Institute of Economic Affairs, 1977). For details see also T.W. Hutchison, *On Revolution and Progress in Economic Knowledge* (Cambridge: Cambridge University Press, 1978), especially chap. 6, "Demythologizing the Keynesian Revolution," pp. 175-199. In a letter to *The Times* (July 28, 1933) Keynes went out of his way to emphasize his agreement with Pigou on policy. (See T.W. Hutchison, "The Keynesian Revolution," p. 187). For Keynes's view on inflation see Thomas M. Humphrey, "Keynes on Inflation," *Federal Reserve Bank of Richmond Economic Review* 67 (January/February 1981): 3-13.

The key concept in Friedman's reply to the Keynesian criticism is "the natural rate of unemployment," which in the authors' words is "determined by the intersection of the demand and supply curves of labor." The natural rate of unemployment is a rather elusive concept. Nobody really knows for sure what the natural rate of unemployment is at any one time. This plays into the hands of the monetarist policy prescription. Since we do not know at any time what the natural rate of unemployment is, it makes no sense to say that the actual rate of unemployment should be pushed to the natural rate by discretionary monetary or fiscal measures.

There is no time to go into details.<sup>5</sup> All I can do is to indicate very briefly the reasons for my doubts about the usefulness of the concept.

To say that the wage is determined by demand and supply of labor implies that there is perfect competition in the labor market. The theory that unemployment is compatible with perfect competition has been brought out very clearly in an important paper by Karl Brunner, Alex Cukierman, and Allan Meltzer<sup>6</sup> which has attracted much attention. The authors insist that *persistent* unemployment can and does occur in economies where "all markets instantaneously clear," that is to say, under perfect competition. That seems to contradict one of the most basic principles of economics. If supply of labor exceeds demand, the price, the wage, should fall and equilibrium with full employment be restored.

The solution of the puzzle is that the authors have a rather odd definition of unemployment. They distinguish between positive and negative unemployment. Negative unemployment is what usually is called over-full employment; for example, people working longer hours than they really want. This is described as the "substitution of future leisure for present leisure." By the same logic positive unemployment has to be defined as the "substitution of present leisure for future leisure."

To say that the unemployed choose leisure makes it quite clear

<sup>5</sup>I discussed these problems in greater detail in my book *Economic Growth and Stability: An Analysis of Economic Change and Policies* (Los Angeles: Nash Publishing, 1974). See also my paper "The Economic Malaise of the 1980's: A Positive Program for a Benevolent and Enlightened Dictator," *Contemporary Economic Problems 1981-1982* (Washington, D.C.: American Enterprise Institute), and my paper prepared for the Accademia Nazionale dei Lincei, Rome, on the occasion of the Antonio Feltrinelli Prize, 1980, "The World Economy, Macroeconomic Theory and Policy—Sixty Years of Profound Change," to be published by the American Enterprise Institute, Washington, D.C., 1983.

<sup>6</sup>Karl Brunner, Alex Cukierman, and Allan Meltzer, "Stagflation, Persistent Unemployment and the Permanence of Economic Shocks," *Journal of Monetary Economics* 6 (October 1980): 467-492.



that the authors really speak of spurious, voluntary unemployment. But the indispensable distinction between voluntary and involuntary unemployment is alien to this school of thought. That the words come from Keynes's *General Theory* has probably made the distinction suspect.

Actually, the distinction has often been made, though using other words, in serious discussions of the unemployment problem, both theoretical and empirical. An excellent discussion can be found, for example, in Pigou's *Theory of Unemployment* (London, 1930) and compilers of unemployment figures have to struggle to prevent voluntary unemployment from contaminating their statistics, no doubt without complete success. The published unemployment figures surely contain significant numbers of spurious, voluntary employment; high unemployment benefits and welfare payments partly account for that.

Let me elaborate a little, for the wage problem is of paramount importance. Consider the case of the U.S. steel or auto industry. Wages in those industries are more than 50 percent higher than the U.S. average, and unemployment is very high. Does it make sense to say that the unemployed steel or auto worker, because of "a forecasting error," chooses leisure? One could perhaps say that from the standpoint of the *union* the unemployment is voluntary, that the union bosses are willing to pay the price of high unemployment to keep the wage level high. But for the unemployed steel and auto workers the situation is quite different. They surely would be happy to work at the ruling wage or even a slightly lower one, if jobs were available.

To my mind the plain fact is that wages have become very rigid even in non-union industries. I am fond of quoting Frank H. Knight, who like Henry Simons, Jacob Viner, and other members of the older generation of the Chicago school, did not ignore or minimize the great importance of the growing rigidity of wages and prices for the smooth working of the monetary system. Knight wrote, "In a free market these differential changes [between prices of 'consumption goods' and 'capital goods' on the one hand, and the prices of 'productive services, especially wages,' on the other hand] would be temporary, but even then might be serious, and with important markets [especially the labor market] as unfree as they actually are, the results take on the proportion of a disaster."<sup>7</sup> Knight wrote with the

<sup>7</sup>Frank H. Knight, "The Business Cycle, Interest and Money," *Review of Economic Statistics* 23 (May 1941). Reprinted in Frank H. Knight's *On the History and Methods of Economics* (Chicago: University of Chicago Press, 1956), p. 224. See also p. 211: "Wages are notoriously sticky, especially with respect to any downward change of the hourly wage rates."

deflation of the 1930s in mind, but what he says about wage and price rigidity applies equally to the case of disinflation and recession.

Since then the situation has become much worse. Government policies, generous unemployment benefits, welfare payments, minimum wages, etc., have, of course, greatly contributed to the rigidity of wages. Needless to add that the labor unions are not the only culprits. Other pressure groups, such as organized agriculture, with the help of governments, keep prices of their products high and rigid, causing a drop in output and employment, or alternatively causing labor to be used to produce huge unsalable surplus stocks, held by the government at the taxpayer's expense.

In recent years more and more economists have come to the conclusion that a decisive recovery from the worldwide recession requires a reduction of the level of real wages. Two years ago a large group of prominent German economists, several of them of monetarist persuasion, urged a wage freeze to let inflation bring down the real wage. Recently *The Economist*, a stronghold of Keynesianism, in two articles recommended a wage reduction—a recommendation which shocked many of its readers.<sup>8</sup> It might be mentioned that this recommendation is entirely in line with what Keynes taught. In *The General Theory* Keynes accepted the classical proposition that marginal productivity of labor declines when more labor is employed and that therefore the real wage has to decline. The trouble is that after many years of inflation the Keynesian policy to bring the real wage down by inflation does not work so well any more. Widespread indexation of wages and other incomes and inflationary expectations have made the Keynesian method to reduce real wages increasingly ineffective.

Herbert Giersch, a good monetarist, has argued in several important papers that all industrial countries suffer from excessively high real wages and too low profits. He thinks it will take several years to bring about the necessary adjustment in the income distribution.<sup>9</sup>

I find myself in substantial agreement with what the authors say about the rational expectations theory. The basic idea that people on the whole do not simply extrapolate the current situation or the recent trend but try to form a rational judgment of how the situation is likely to develop, including the actions of the policymakers, is undoubtedly correct. This fits in very well with the monetarist prescription of steady monetary growth.

<sup>8</sup>"Work on a Pay Cut," *The Economist*, London, November 27, 1982, pp. 11–12, and "Wage Cuts," *The Economist*, London, December 18, 1982, pp. 14–15.

<sup>9</sup>See Herbert Giersch, "Prospects for the World Economy," *Skandinaviska Enskilda Banken Quarterly Review*, Stockholm 1982, pp. 104–110.

But the rational expectations theorists often spoil their case by overstatements, by assuming that people on the whole reach the same, correct conclusion about future developments, including the future course of monetary and fiscal policy.

In the final section of their paper the authors critically examine the views of three groups of economists who accept the proposition that "monetary change" is of primary importance for changes in economic activity, but reject the rule of steady monetary growth.

The first group is the "global monetarists," the second the proponents of "Austrian economics," and the third the "new monetarists."

The authors do not say who the global monetarists are, but they seem to have advocates of the gold standard in mind. Under the gold standard there is indeed no room for the rule of steady monetary growth. The exchange rates are fixed and monetary growth is exogenously determined. The authors are right that all this is different under floating. If there was time I would say something on the recent criticism of floating.

Let me mention, however, that I can think of only one well-known economist who is not an advocate of gold who can be called a global monetarist. Ronald McKinnon argued recently that for the United States the flow of spending, etc., is better explained by changes in the world money supply than by the changes in the U.S. money supply. But his statistics have been convincingly challenged by Henry Goldstein.<sup>10</sup>

One more remark: It should not be forgotten that for the numerous countries that peg their currency to the dollar, the deutsche mark or some other currency or basket of currencies, the rule of steady monetary growth obviously is not applicable. An example: The Austrian schilling is pegged to the deutsche mark, and the Austrian National Bank, unlike the German or Swiss banks, does not set monetary growth targets. This has been interpreted to mean that Austria follows a Keynesian rather than a monetarist policy. As I have explained elsewhere, the correct interpretation is that the German Bundesbank provides the monetarist basis for Austria.<sup>11</sup>

The term "Austrian economics" is not easy to define. Professor Machlup has tried to clarify the problem in an excellent article in the *New Encyclopedia of the Social Sciences*. As far as money is con-

<sup>10</sup>Henry Goldstein, "A Critical Appraisal of McKinnon's World Money Supply Hypothesis," Federal Reserve Bank of Chicago, November 1982. (Mimeographed)

<sup>11</sup>See my paper on "Austria's Economic Development After the Two World Wars: A Mirror Picture of the World Economy," in *The Political Economy of Austria* (Washington, D.C.: American Enterprise Institute, 1981), and "Austro-Keynesianism or Austro-Monetarism," pp. 67-69.

cerned, the older members of the Austrian school, including Ludwig von Mises, were staunch supporters of the gold standard. This is also true of the so-called Neo-Austrian school that flourishes at New York University. The members of this school are largely disciples and followers of Mises. (I might mention that the only real Austrian at New York University, Fritz Machlup, is not a member of the Neo-Austrian school.)

In his earlier writings Hayek supported the gold standard and argued that from the theoretical standpoint the ideal rule would be to keep the quantity of money constant.

I don't know whether he still believes that. But his latest radical proposal to get the government entirely out of the business of money creation, to eliminate central banks, to "denationalize" money, should be regarded as a counsel of despair, that the modern democratic government cannot be entrusted with such a complicated task as that of regulating the money supply in an acceptable way. He therefore wants to turn over the creation of money to the forces of free markets. Banks should be free to create money, and the forces of free competition will lead to the survival of the fittest; that is to say, those banks that issue the stablest money will survive.

This sounds rather strange to put it mildly. I still think that the Bagehot was right when he said that "money cannot manage itself."

I agree with the authors that Hayek's proposal, as well as similar proposals of the new monetarists, to "change utterly root and branch the existing monetary system ignores the enormous attachment of the private sector to arrangements that have become customary." I would add that these proposals also ignore the enormous importance of having a common unit of account and medium of exchange. If the moneys issued by different banks competed freely in the market, the result would be either the emergence of a private monopoly or oligopoly of money creation, or the circulation, side by side, of several kinds of money with fluctuating exchange rates between them. Either one of these two outcomes would be intolerable. The immediate result would be to bring the government back into the business of money creation.

I therefore agree with the authors that a steady monetary growth rule is the best policy, but I hope they will agree that discretionary measures cannot be ruled out altogether. In addition to the situations mentioned earlier, there is the fact that the steady rate of growth of the money supply will have to be changed from time to time. The rate of monetary growth cannot remain unchanged for long periods irrespective of institutional changes in the monetary area and sur-

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rounding conditions. A change of the growth rate is an act of discretion. I conclude that the present practice of many central banks to have narrow target ranges rather than a single monetary growth target is a reasonable compromise.