The Monetarist Controversy or, Should We Forsake Stabilization Policies?

By FRANCO MODIGLIANI*

In recent years and especially since the onset of the current depression, the economics profession and the lay public have heard a great deal about the sharp conflict between "monetarists and Keynesians" or between "monetarists and fiscalists." The difference between the two "schools" is generally held to center on whether the money supply or fiscal variables are the major determinants of aggregate economic activity, and hence the most appropriate tool of stabilization policies.

My central theme is that this view is quite far from the truth, and that the issues involved are of far greater practical import. There are in reality no serious analytical disagreements between leading monetarists and leading nonmonetarists. Milton Friedman was once quoted as saying, "We are all Keynesians, now," and I am quite prepared to reciprocate that "we are all monetarists"—if by monetarism is meant assigning to the stock of money a major role in determining output and prices. Indeed, the list of those who have long been monetarists in this sense is quite extensive, including among other John Maynard Keynes as well as myself, as is attested by my 1944 and 1963 articles.

In reality the distinguishing feature of the monetarist school and the real issues of disagreement with nonmonetarists is not monetarism, but rather the role that should probably be assigned to stabilization policies. Nonmonetarists accept what I regard to be the fundamental practical message of *The General Theory*: that a private enterprise economy using an intangible money *needs* to be stabilized, *can* be stabilized, and therefore *should* be stabilized by appropriate monetary and fiscal policies. Monetarists by contrast take the view that there is no serious need to stabilize the economy; that even if there were a need, it could not be done, for stabilization policies would be more likely to increase than to decrease instability; and, at least some monetarists would, I believe, go so far as to hold that, even in the unlikely event that stabilization policies could on balance prove beneficial, the government should not be trusted with the necessary power.

What has led me to address this controversy is the recent spread of monetarism, both in a simplistic, superficial form and in the form of growing influence on the practical conduct of economic policy, which influence, I shall argue presently, has played at least some role in the economic upheavals of the last three years.

In what follows then, I propose first to review the main arguments bearing on the *need* for stabilization policies, that is, on the likely extent of instability in the absence of such policies, and then to examine the issue of the supposed destabilizing effect of pursuing stabilization policies. My main concern will be with instability generated by the traditional type of disturbances—demand shocks. But before I am through, I will give some consideration to the difficult problems raised by the newer type of disturbance—supply shocks.

I. The Keynesian Case for Stabilization Policies

A. *The General Theory*

Keynes' novel conclusion about the need for

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stabilization policies, as was brought out by the early interpreters of *The General Theory* (for example, John Hicks, the author, 1944), resulted from the interaction of a basic contribution to traditional monetary theory—liquidity preference—and an unorthodox hypothesis about the working of the labor market—complete downward rigidity of wages.

Because of liquidity preference, a change in aggregate demand, which may be broadly defined as any event that results in a change in the market clearing or equilibrium rate of interest, will produce a corresponding change in the real demand for money or velocity of circulation, and hence in the real stock of money needed at full employment. As long as wages are perfectly flexible, even with a constant nominal supply, full employment could and would be maintained by a change of wages and prices as needed to produce the required change in the real money supply—though even in this case, stability of the price level would require a countercyclical monetary policy. But, under the Keynesian wage assumption the classical adjustment through prices can occur only in the case of an increased demand. In the case of a decline, instead, wage rigidity prevents the necessary increase in the real money supply and the concomitant required fall in interest rates. Hence, if the nominal money supply is constant, the initial equilibrium must give way to a new stable one, characterized by lower output and by an involuntary reduction in employment, so labeled because it does not result from a shift in notional demand and supply schedules in terms of real wages, but only from an insufficient real money supply. The nature of this equilibrium is elegantly captured by the Hicksian *IS-LM* paradigm, which to our generation of economists has become almost as familiar as the demand-supply paradigm was to earlier ones.

This analysis implied that a fixed money supply far from insuring approximate stability of prices and output, as held by the traditional view, would result in a rather unstable economy, alternating between periods of protracted unemployment and stagnation, and bursts of inflation. The extent of downward instability would depend in part on the size of the exogenous shocks to demand and in part on the strength of what may be called the Hicksian mechanism. By this I mean the extent to which a shift in *IS*, through its interaction with *LM*, results in some decline in interest rates and thus in a change in income which is smaller than the original shift. The stabilizing power of this mechanism is controlled by various parameters of the system. In particular, the economy will be more unstable the greater the interest elasticity of demand for money, and the smaller the interest responsiveness of aggregate demand. Finally, a large multiplier is also destabilizing in that it implies a larger shift in *IS* for a given shock.

However, the instability could be readily counteracted by appropriate stabilization policies. Monetary policy could change the nominal supply of money so as to *accommodate* the change in real demand resulting from shocks in aggregate demand. Fiscal policy, through expenditure and taxes, could *offset* these shocks, making full employment consistent with the initial nominal money stock. In general, both monetary and fiscal policies could be used in combination. But because of a perceived uncertainty in the response of demand to changes in interest rates, and because changes in interest rates through monetary policy could meet difficulties and substantial delays related to expectations (so-called liquidity traps), fiscal policy was regarded as having some advantages.

B. *The Early Keynesians*

The early disciples of the new Keynesian gospel, still haunted by memories of the Great Depression, frequently tended to outdo Keynes' pessimism about potential instability. Concern with liquidity traps fostered the view that the demand for money was highly interest elastic; failure to distinguish between the short- and long-run marginal propensity to save led to overestimating the long-run saving rate, thereby fostering concern with stagnation, and to underestimating the short-run propensity, thereby exaggerating the short-run multiplier. Interest rates were supposed to affect, at best, the demand for long-lived fixed investments, and the interest elasticity was deemed to be low. Thus, shocks were believed to produce a large response. Finally, investment demand was seen as capriciously controlled by "animal spirits," thus providing an important source of shocks. All this justified calling for very active stabilization policies. Furthermore, since the very circumstances which produce a large response to demand shocks also produce a large response to *fiscal* and a small response to *monetary* actions, there was a tendency to focus on fiscal policy as the main tool to keep the economy at near full employment.

C. *The Phillips Curve*

In the two decades following *The General Theory*, there were a number of developments of the Keynesian system including dynamization of the model, the stress on taxes versus expenditures and the balanced budget multiplier, and the first attempts at estimating the critical parameters through econometric techniques and models. But for present purposes, the most important one was the uncovering of a "stable" statistical relation between the rate of change of wages and the rate of unemployment, which has since come to be known as the Phillips curve. This relation, and its generalization by Richard Lipsey to allow for the effect of recent inflation, won wide acceptance even before an analytical underpinning could be provided for it, in part because it could account for the "puzzling" experience of 1954 and 1958, when wages kept rising despite the substantial rise in unemployment. It also served to dispose of the rather sterile "cost push"–"demand pull" controversy.

In the following years, a good deal of attention went into developing theoretical foundations for the Phillips curve, in particular along the lines of search models (for example, Edmund Phelps et al.). This approach served to shed a new light on the nature of unemployment by tracing it in the first place to labor turnover and search time rather than to lack of jobs as such: in a sense unemployment is all frictional—at least in developed countries. At the same time it clarified how the availability of more jobs tends to reduce unemployment by increasing vacancies and thus reducing search time.

Acceptance of the Phillips curve relation implied some significant changes in the Keynesian framework which partly escaped notice until the subsequent monetarists' attacks. Since the rate of change of wages decreased smoothly with the rate of unemployment, there was no longer a unique Full Employment but rather a whole family of possible equilibrium rates, each associated with a different rate of inflation (and requiring, presumably, a different long-run growth of money). It also impaired the notion of a stable underemployment equilibrium. A fall in demand could still cause an initial rise in unemployment but this rise, by reducing the growth of wages, would eventually raise the real money supply, tending to return unemployment to the equilibrium rate consistent with the given long-run growth of money.

But at the practical level it did not lessen the case for counteracting lasting demand disturbances through stabilization policies rather than by relying on the slow process of wage adjustment to do the job, at the cost of protracted unemployment and instability of prices. Indeed, the realm of stabilization policies appeared to expand in the sense that the stabilization authority had the power of choosing the unemployment rate around which employment was to be stabilized, though it then had to accept the associated inflation. Finally, the dependence of wage changes also on past inflation forced recognition of a distinction between the short- and the long-run Phillips curve, the latter exhibiting the long-run equilibrium rate of inflation implied by a *maintained* unemployment rate. The fact that the long-run tradeoff between unemployment and inflation was necessarily less favorable than the short-run one, opened up new vistas of "enjoy-it-now, pay-later" policies, and even resulted in an entertaining literature on the political business cycle and how to stay in the saddle by riding the Phillips curve (see for example, Ray Fair, William Nordhaus).

II. The Monetarists' Attack

A. *The Stabilizing Power of the Hicksian Mechanism*

The monetarists' attack on Keynesianism was directed from the very beginning not at the Keynesian framework as such, but at whether it really implied a need for stabilization. It rested on a radically different empirical assessment of the value of the parameters controlling the stabilizing power of the Hicksian mechanism and of the magnitude and duration of response to shocks, given a stable money supply. And this different assessment in turn was felt to justify a radical downgrading of the *practical relevance* of the Keynesian framework as distinguished from its *analytical validity*.

Liquidity preference was a fine contribution to monetary theory but in practice the responsiveness of the demand for money, and hence of velocity, to interest rates, far from being unmanageably large, was so small that according to a well-known paper by Milton Friedman (1969), it could not even be detected empirically. On the other hand, the effect of interest rates on aggregate demand was large and by no means limited to the traditional fixed investments but quite pervasive. The difficulty of detecting it empirically resulted from focusing on a narrow range of measured market rates and from the fact that while the aggregate could be counted on to respond, the response of individual components might not be stable. Finally, Friedman's celebrated contribution to the theory of the consumption function (1957) (and my own work on the life cycle hypothesis with Richard Brumberg and others, reviewed by the author, 1975) implied a very high short-run marginal propensity to save in response to transient disturbances to income and hence a small short-run multiplier.

All this justified the conclusion that (i) though demand shocks might qualitatively work along the lines described by Keynes, quantitatively the Hicks mechanism is so strong that their impact would be *small* and *transient*, provided the stock of money was kept on a steady growth path; (ii) fiscal policy actions, like other demand shocks, would have *minor* and *transitory* effects on demand, while changes in money would produce *large* and *permanent* effects on money income; and, therefore, (iii) the observed instability of the economy, which was anyway proving moderate as the postwar period unfolded, was most likely the result of the unstable growth of money, be it due to misguided endeavors to stabilize income or to the pursuit of other targets, which were either irrelevant or, in the case of balance of payments goals, should have been made irrelevant by abandoning fixed exchanges.

B. *The Demise of Wage Rigidity and the Vertical Phillips Curve*

But the most serious challenge came in Friedman's 1968 Presidential Address, building on ideas independently put forth also by Phelps (1968). Its basic message was that, despite appearances, wages were in reality perfectly flexible and there was accordingly *no* involuntary unemployment. The evidence to the contrary, including the Phillips curve, was but a statistical illusion resulting from failure to differentiate between price changes and *unexpected* price changes.

Friedman starts out by reviving the Keynesian notion that, at any point of time, there exists a unique full-employment rate which he labels the "natural rate." An unanticipated fall in demand in Friedman's competitive world leads firms to reduce prices and also output and employment along the short-run marginal cost curve—unless the nominal wage declines together with prices. But workers, failing to judge correctly the current and prospective fall in prices, misinterpret the reduction of nominal wages as a cut in *real* wages. Hence, assuming a positively sloped supply function, they reduce the supply of labor. As a result, the effective real wage rises to the point where the resulting decline in the demand for labor matches the reduced supply. Thus, output falls not because of the decline in demand, but because of the entirely voluntary reduction in the supply of labor, in response to erroneous perceptions. Furthermore, the fall in employment can only be temporary, as expectations must soon catch up with the facts, at least in the absence of new shocks. The very same mechanism works in the case of an increase in demand, so that the responsiveness of wages and prices is the same on either side of the natural rate.

The upshot is that Friedman's model also implies a Phillips-type relation between inflation, employment or unemployment, and past inflation,—provided the latter variable is interpreted as a reasonable proxy for expected inflation. But it turns the standard explanation on its head: instead of (excess) employment causing inflation, it is (the unexpected component of) the rate of inflation that causes excess employment.

One very basic implication of Friedman's model is that the coefficient of price expectations should be precisely unity. This specification implies that whatever the shape of the short-run Phillips curve—a shape determined by the relation between expected and actual price changes, and by the elasticity of labor supply with respect to the perceived real wage—the long-run curve *must be vertical*.

Friedman's novel twist provided a fresh prop for the claim that stabilization policies are not really needed, for, with wages flexible, except possibly for transient distortions, the Hicksian mechanism receives powerful reinforcement from changes in the real money supply. Similarly, the fact that full employment was a razor edge provided new support for the claim that stabilization policies were bound to prove destabilizing.

C. *The Macro Rational Expectations Revolution*

But the death blow to the already badly battered Keynesian position was to come only shortly thereafter by incorporating into Friedman's model the so-called rational expectation hypothesis, or *REH*. Put very roughly, this hypothesis, originally due to John Muth, states that rational economic agents will endeavor to form expectations of relevant future variables by making the most efficient use of all information provided by past history. It is a fundamental and fruitful contribution that has already found many important applications, for example, in connection with speculative markets, and as a basis for some thoughtful criticism by Robert Lucas (1976) of certain features of econometric models. What I am concerned with here is only its application to macro-economics, or *MREH*, associated with such authors as Lucas (1972), Thomas Sargent (1976), and Sargent and Neil Wallace (1976).

The basic ingredient of *MREH* is the postulate that the workers of Friedman's model hold rational expectations, which turns out to have a number of remarkable implications: (i) errors of price expectations, which are the only source of departure from the natural state, cannot be avoided but they can only be short-lived and random. In particular, there cannot be persistent unemployment above the natural rate for this would imply high serial correlation between the successive errors of expectation, which is inconsistent with rational expectations; (ii) any attempts to stabilize the economy by means of stated monetary or fiscal rules are bound to be totally ineffective because their effect will be fully discounted in rational expectations; (iii) nor can the government successfully pursue *ad hoc* measures to offset shocks. The private sector is already taking care of any anticipated shock; therefore government policy could conceivably help only if the government information was better than that of the public, which is impossible, by the very definition of rational expectations. Under these conditions, *ad hoc* stabilization policies are most likely to produce instead further destabilizing shocks.

These are clearly remarkable conclusions, and a major *re*discovery—for it had all been said 40 years ago by Keynes in a well-known passage of *The General Theory*:

> If, indeed, labour were always in a position to take action (and were to do so), whenever there was less than full employment, to reduce its money demands by concerted action to whatever point was required to make money so abundant rela-

> tively to the wage-unit that the rate of interest would fall to a level compatible with full employment, we should, in effect, have monetary management by the Trade Unions, aimed at full employment, instead of by the banking systems. [p. 267]

The only novelty is that *MREH* replaces Keynes' opening "if" with a "since."

If one accepts this little amendment, the case against stabilization policies is complete. The economy is inherently pretty stable—except possibly for the effect of government messing around. And to the extent that there is a small residual instability, it is beyond the power of human beings, let alone the government, to alleviate it.

III. How Valid Is the Monetarist Case?

A. *The Monetarist Model of Wage Price Behavior*

In setting out the counterattack it is convenient to start with the monetarists' model of price and wage behavior. Here one must distinguish between the model as such and a specific implication of that model, namely that the long-run Phillips curve is vertical, or, in substance, that, in the long run, money is neutral. That conclusion, by now, does not meet serious objection from nonmonetarists, at least as a first approximation.

But the proposition that other things equal, and given time enough, the economy will eventually adjust to any indefinitely maintained stock of money, or *n*th derivative thereof, can be derived from a variety of models and, in any event, is of very little practical relevance, as I will argue below. What is unacceptable, because inconsistent with both micro and macro evidence, is the specific monetarist model set out above and its implication that all unemployment is a voluntary, fleeting response to transitory misperceptions.

One may usefully begin with a criticism of the Macro Rational Expectations model and why Keynes' "if" should not be replaced by "since." At the logical level, Benjamin Friedman has called attention to the omission from *MREH* of an explicit learning model, and has suggested that, as a result, it can only be interpreted as a description not of short-run but of long-run equilibrium in which no agent would wish to recontract. But then the implications of *MREH* are clearly far from startling, and their policy relevance is almost nil. At the institutional level, Stanley Fischer has shown that the mere recognition of long-term contracts is sufficient to generate wage rigidity and a substantial scope for stabilization policies. But the most glaring flaw of *MREH* is its inconsistency with the evidence: if it were valid, deviations of unemployment from the natural rate would be small and transitory—in which case *The General Theory* would not have been written and neither would this paper. Sargent (1976) has attempted to remedy this fatal flaw by hypothesizing that the persistent and large fluctuations in unemployment reflect merely corresponding swings in the natural rate itself. In other words, what happened to the United States in the 1930's was a severe attack of contagious laziness! I can only say that, despite Sargent's ingenuity, neither I nor, I expect, most others at least of the nonmonetarists' persuasion are quite ready yet to turn over the field of economic fluctuations to the social psychologist!

Equally serious objections apply to Friedman's modeling of the commodity market as a perfectly competitive one—so that the real wage rate is continuously equated to the *short-run* marginal product of labor—and to his treatment of labor as a homogenous commodity traded in an auction market, so that, at the going wage, there never is any excess demand by firms or excess supply by workers. The inadequacies of this model as a useful formalization of present day Western economies are so numerous that only a few of the major ones can be mentioned here.

Friedman's view of unemployment as a voluntary reduction in labor supply could at best provide an explanation of variations in labor force—and then only under the questionable assumption that the supply function has a significantly positive slope—but cannot readily account for changes in unemployment. Furthermore, it cannot be reconciled with the well-known fact that *rising* unemployment is accompanied by a fall, not by a *rise* in quits, nor with the role played by temporary layoffs to which Martin Feldstein has recently called attention. Again, his competitive model of the commodity market, accepted also in *The General Theory*, implies that changes in real wages, adjusted for long-run productivity trend, should be significantly negatively correlated with cyclical changes in employment and output and with changes in money wages. But as early as 1938, John Dunlop showed that this conclusion was rejected by some eighty years of British experience and his results have received some support in more recent tests of Ronald Bodkin for the United States and Canada. Similar tests of my own, using quarterly data, provide striking confirmation that for the last two decades from the end of the Korean War until 1973, the association of trend adjusted real compensations of the private nonfarm sector with either employment or the change in nominal compensation is prevailingly positive and very significantly so.[1]

This evidence can, instead, be accounted for by the oligopolistic pricing model—according to which price is determined by *long-run* minimum average cost up to a mark-up reflecting entry-preventing considerations (see the author, 1958)—coupled with some lags in the adjustment of prices to costs. This model implies that firms respond to a change in demand by endeavoring to adjust output and employment, without significant changes in prices relative to wages; and the resulting changes in available jobs have their initial impact not on wages but rather on unemployment by way of layoffs and recalls and through changes in the level of vacancies, and hence on the length of average search time.

If, in the process, vacancies rise above a critical level, or "natural rate," firms will endeavor to reduce them by outbidding each other, thereby raising the rate of change of wages. Thus, as long as jobs and vacancies remain above, and unemployment remains below, some critical level which might be labeled the "noninflationary rate" (see the author and Lucas Papademos, 1975), wages and prices will tend to accelerate. If, on the other hand, jobs fall below, and unemployment rises above, the noninflationary rate, firms finding that vacancies are less than optimal —in the limit the unemployed queuing outside the gate will fill them instantly—will have an incentive to reduce their relative wage offer. But in this case, in which too much labor is looking for too few jobs, the trend toward a sustained decline in the rate of growth of wages is likely to be even weaker than the corresponding acceleration when too many jobs are bidding for too few people. The main reason is the nonhomogeneity of labor. By far the largest and more valuable source of labor supply to a firm consists of those already employed who are not readily interchangeable with the unemployed and, in contrast with them, are concerned with protecting their earnings and not with reestablishing full employment. For these reasons, and because the first to quit are likely to be the best workers, a reduction of the labor force can, within limits, be accomplished more economically, not by reducing wages to generate enough quits, but by firing or, when possible, by layoffs which insure access to a trained labor force when demand recovers. More generally, the inducement to

[1]Thus, in a logarithmic regression of private nonfarm hourly compensation deflated by the private nonfarm deflator on output per man-hour, time, and private nonfarm employment, after correcting for first-order serial correlation, the latter variable has a coefficient of .17 and a t-ratio of 5. Similar though less significant results were found for manufacturing. If employment is replaced by the change in nominal compensation, its coefficient is .40 with a t-ratio of 6.5. Finally, if the change in compensation is replaced by the change in price, despite the negative bias from error of measurement of price, the coefficient of this variable is only $-.09$ with an entirely insignificant t-ratio of .7. The period after 1973 has been omitted from the tests as irrelevant for our purposes, since the inflation was driven primarily by an exogenous price shock rather than by excess demand. As a result of the shock, prices, and to some extent wages, rose rapidly while employment and real wages fell. Thus, the addition of the last two years tends to increase spuriously the positive association between real wages and employment, and to decrease that between real wages and the change in nominal wages or prices.

reduce relative wages to eliminate the excess supply is moderated by the effect that such a reduction would have on quits and costly turnover, even when the resulting vacancies can be readily filled from the ranks of the unemployed. Equally relevant are the consequences in terms of loss of morale and good will, in part for reasons which have been elaborated by the literature on implicit contracts (see Robert Gordon). Thus, while there will be some tendency for the rate of change of wages to fall, the more so the larger the unemployment—at least in an economy like the United States where there are no overpowering centralized unions—that tendency is severely damped.

And whether, given an unemployment rate significantly and persistently above the noninflationary level, the rate of change of wages would, eventually, tend to turn negative and decline without bound or whether it would tend to an asymptote is a question that I doubt the empirical evidence will ever answer. The one experiment we have had—the Great Depression—suggests the answer is negative, and while I admit that, for a variety of reasons, that evidence is muddied, I hope that we will never have the opportunity for a second, clean experiment.

In any event, what is really important for practical purposes is not the long-run equilibrium relation as such, but the speed with which it is approached. Both the model sketched out and the empirical evidence suggest that the process of acceleration or deceleration of wages when unemployment differs from the noninflationary rate will have more nearly the character of a crawl than of a gallop. It will suffice to recall in this connection that there was excess demand pressure in the United States at least from 1965 to mid-1970, and during that period the growth of inflation was from some 1.5 to only about 5.5 percent per year. And the response to the excess supply pressure from mid-1970 to early 1973, and from late 1974 to date was equally sluggish.

B. *The Power of Self-Stabilizing Mechanisms: The Evidence from Econometric Models*

There remains to consider the monetarists' initial criticism of Keynesianism, to wit, that even without high wage flexibility, the system's response to demand shocks is small and shortlived, thanks to the power of the Hicksian mechanism. Here it must be acknowledged that every one of the monetarists' criticisms of early, simpleminded Keynesianism has proved in considerable measure correct.

With regard to the interest elasticity of demand for money, post-Keynesian developments in the theory of money, and in particular, the theoretical contributions of William Baumol, James Tobin, Merton Miller, and Daniel Orr, point to a modest value of around one-half to one-third, and empirical studies (see for example, Stephen Goldfeld) are largely consistent with this prediction (at least until 1975!). Similarly, the dependence of consumption on long-run, or life cycle, income and on wealth, together with the high marginal tax rates of the postwar period, especially the corporate tax, and leakages through imports, lead to a rather low estimate of the multiplier.

Last but not least, both theoretical and empirical work, reflected in part in econometric models, have largely vindicated the monetarist contention that interest effects on demand are pervasive and substantial. Thus, in the construction and estimation of the MIT-Penn-Social Science Research Council (*MPS*) econometric model of the United States, we found evidence of effects, at least modest, on nearly every component of aggregate demand. One response to money supply changes that is especially important in the *MPS*, if somewhat controversial, is via interest rates on the market value of all assets and thus on consumption.

There is, therefore, substantial agreement that in the United States the Hicksian mechanism is fairly effective in limiting the effect of shocks, and that the response of wages and prices to excess demand or supply will also work *gradually* toward eliminating largely, if not totally, any effect on employment. But in the view of nonmonetarists, the evidence overwhelmingly supports the conclusion that the *interim* response is still of significant magnitude and of considerable duration, basically because the wheels of the offsetting mechanism grind slowly. To be sure, the first link of the mechanism, the rise in short-term rates, gets promptly into play and heftily, given the low money demand elasticity; but most expenditures depend on long-term rates, which generally respond but gradually, and the demand response is generally also gradual. Furthermore, while this response is building up, multiplier and accelerator mechanisms work toward amplifying the shock. Finally, the classical mechanism—the change in real money supply through prices—has an even longer lag because of the sluggish response of wages to excess demand.

These interferences are supported by simulations with econometric models like the *MPS*. Isolating, first, the working of the Hicksian mechanism by holding prices constant, we find that a 1 percent demand shock, say a rise in real exports, produces an impact effect on aggregate output which is barely more than 1 percent, rises to a peak of only about 2 percent a year later, and then declines slowly toward a level somewhat over 1.5 percent.

Taking into account the wage price mechanism hardly changes the picture for the first year because of its inertia. Thereafter, however, it becomes increasingly effective so that a year later the real response is back at the impact level, and by the end of the third year the shock has been fully offset (thereafter output oscillates around zero in a damped fashion). Money income, on the other hand, reaches a peak of over 2.5, and then only by the middle of the second year. It declines thereafter, and tends eventually to oscillate around a *positive* value because normally, a demand shock requires eventually a change in interest rates and hence in velocity and money income.

These results, which are broadly confirmed by other econometric models, certainly do not support the view of a highly unstable economy in which fiscal policy has powerful and everlasting effects. But neither do they support the monetarist view of a highly stable economy in which shocks hardly make a ripple and the effects of fiscal policy are puny and fast vanishing.

C. *The Monetarist Evidence and the St. Louis Quandary*

Monetarists, however, have generally been inclined to question this evidence. They countered at first with tests bearing on the stability of velocity and the insignificance of the multiplier, which, however, as indicated in my criticism with Albert Ando (1965), must be regarded as close to worthless. More recently, several authors at the Federal Reserve Bank of St. Louis (Leonall Andersen, Keith Carlson, Jerry Lee Jordan) have suggested that instead of deriving multipliers from the analytical or numerical solution of an econometric model involving a large number of equations, any one of which may be questioned, they should be estimated directly through "reduced form" equations by relating the change in income to current and lagged changes in some appropriate measure of the money supply and of fiscal impulses.

The results of the original test, using the current and but four lagged values of M^1 and of high Employment Federal Expenditure as measures of monetary and fiscal impulses, turned out to be such as to fill a monetarist's heart with joy. The contribution of money, not only current but also lagged, was large and the coefficients implied a not unreasonable effect of the order of magnitude of the velocity of circulation, though somewhat higher. On the other hand, the estimated coefficients of the fiscal variables seemed to support fully the monetarists' claim that their impact was both small and fleeting: the effect peaked in but two quarters and was only around one, and disappeared totally by the fourth quarter following the change.

These results were immediately attacked on the ground that the authors had used the wrong measure of monetary and fiscal actions, and it was shown that the outcome was somewhat sensitive to alternative measures; however, the basic nature of the results did not change, at least qualitatively. In particular, the outcome does not differ materially, at least for the original period up to 1969, if one replaces high employment outlays with a variable that might be deemed more suitable, like government expenditure on goods and services, plus exports.

These results must be acknowledged as disturbing for nonmonetarists, for there is little question that movements in government purchases and exports are a major source of demand disturbances; if econometric model estimates of the response to demand disturbances are roughly valid, how can they be so grossly inconsistent with the reduced form estimates?

Attempts at reconciling the two have taken several directions, which are reviewed in an article coauthored with Ando (1976). Our main conclusion, based on simulation techniques, is that when income is subject to substantial shocks from many sources other than monetary and fiscal, so that these variables account for only a moderate portion of the variations in income (in the United States, it has been of the order of one-half to two-thirds), then the St. Louis reduced form method yields highly unstable and unreliable estimates of the true structure of the system generating the data.

The crucial role of unreliability and instability has since been confirmed in more recent work of Daniel O'Neill in his forthcoming thesis. He shows in the first place that different methods of estimation yield widely different estimates, including many which clearly overstate the expenditure and understate the money multipliers. He further points out that, given the unreliability of the estimates resulting from multicollinearity and large residual variance, the relevant question to ask is not whether these estimates differ from those obtained by structural estimation, but whether the *difference is statistically significant*; that is, larger than could be reasonably accounted for by sampling fluctuations.

I have carried out this standard statistical test using as true response coefficients those generated by the *MPS* model quoted earlier.[2] I find that, at least when the test is based on the largest possible sample—the entire post-Korean period up to the last two very disturbed years—the difference is totally insignificant when estimation is in level form (F is less than one) and is still not significant at the 5 percent level, when in first differences.

This test resolves the puzzle by showing that there really is no puzzle: the two alternative estimates of the expenditure multipliers are not inconsistent, given the margin of error of the estimates. It implies that one should accept whichever of the two estimates is produced by a more reliable and stable method, and is generally more sensible. To me, those criteria call, without question, for adopting the econometric model estimates. But should there be still some lingering doubt about this choice, I am happy to be able to report the results of one final test which I believe should dispose of the reduced form estimates—at least for a while. Suppose the St. Louis estimates of the expenditure multiplier are closer to God's truth than the estimates derived through econometric models. Then it should be the case that if one uses their coefficients to forecast income beyond the period of fit, these forecasts should be appreciably better than those obtained from a forecasting equation in which the coefficients of the expenditure variable are set equal to those obtained from econometric models.

I have carried out this test, comparing a reduced form equation fitted to the period originally used at St. Louis, terminating in 1969 (but reestimated with the lastest revised data) with an equation in which the coefficients of government expenditure plus exports were constrained to be those estimated from the *MPS*, used in the above F-test. The results are clear cut: the errors using the reduced form coefficient are not smaller but on the average substantially *larger* than those using *MPS* multipliers. For the first four years, terminating at the end of 1973, the St. Louis equation produces errors which are distinctly larger in eight quarters, and smaller in but three, and its squared error is one-third larger. For the last two years of turmoil, both equations perform miserably, though even here the *MPS* coefficients perform just a bit better. I have repeated this test with equations estimated through the first half of the postwar period, and the results are, if anything, even more one-sided.

The moral of the story is pretty clear. First,

[2]For the purpose of the test, coefficients were scaled down by one-third to allow for certain major biases in measured government expenditure for present purposes (mainly the treatment of military procurement on a delivery rather than work progress basis, and the inclusion of direct military expenditure abroad).

reduced form equations relying on just two exogenous variables are very unreliable for the purpose of estimating structure, nor are they particularly accurate for forecasting, though per dollar of research expenditure they are surprisingly good. Second, if the St. Louis people want to go on using this method and wish to secure the best possible forecast, then they should ask the *MPS* or any other large econometric model what coefficients they should use for government expenditure, rather than trying to estimate them by their unreliable method.

From the theory and evidence reviewed, we must then conclude that opting for a constant rate of growth of the nominal money supply can result in a stable economy only in the absence of significant exogenous shocks. But obviously the economy has been and will continue to be exposed to many significant shocks, coming from such things as war and peace, and other large changes in government expenditure, foreign trade, agriculture, technological progress, population shifts, and what not. The clearest evidence on the importance of such shocks is provided by our postwar record with its six recessions.

IV. The Record of Stabilization Policies: Stabilizing or Destabilizing

A. *Was Postwar Instability Due to Unstable Money Growth?*

At this point, of course, monetarists will object that, over the postwar period, we have *not* had a constant money growth policy and will hint that the observed instability can largely be traced to the instability of money. The only way of meeting this objection squarely would be, of course, to rerun history with a good computer capable of calculating 3 percent at the helm of the Fed.

A more feasible, if less conclusive approach might be to look for some extended periods in which the money supply grew fairly smoothly and see how the economy fared. Combing through our post-Korean War history, I have been able to find just two stretches of several years in which the growth of the money stock was relatively stable, whether one chooses to measure stability in terms of percentage deviations from a constant growth or of dispersion of four-quarter changes. It may surprise some that one such stretch occurred quite recently and consists of the period of nearly four years beginning in the first quarter of 1971 (see the author and Papademos, 1976). During this period, the average growth was quite large, some 7 percent, but it was relatively smooth, generally well within the 6 to 8 percent band. The average deviation from the mean is about .75 percent. The other such period lasted from the beginning of 1953 to the first half of 1957, again a stretch of roughly four years. In sharp contrast to the most recent period, the average growth here is quite modest, only about 2 percent; but again, most four-quarter changes fell well within a band of two percentage points, and the average deviation is again .7. By contrast, during the remaining 13-year stretch from mid-1957 to the end of 1970, the variability of money growth was roughly twice as large if measured by the average deviation of four quarter changes, and some five times larger if measured by the percentage deviation of the money stock from a constant growth trend.

How did the economy fare in the two periods of relatively stable money growth? It is common knowledge that the period from 1971 to 1974, or from 1972 to 1975 if we want to allow a one-year lag for money to do its trick, was distinctly the most unstable in our recent history, marked by sharp fluctuations in output and wild gyrations of the rate of change of prices. As a result, the average deviation of the four-quarter changes in output was 3.3 percent, more than twice as large as in the period of less stable money growth. But the first stretch was also marked by well above average instability, with the contraction of 1954, the sharp recovery of 1955, and the new contraction in 1958, the sharpest in postwar history except for the present one. The variability of output is again 50 percent larger than in the middle period.

To be sure, in the recent episode serious exogenous shocks played a major role in the development of prices and possibly output, although the same is not so readily apparent for the period 1953 to 1958. But, in any event, such extenuating circumstances are quite irrelevant to my point; for I am not suggesting that the stability of money was the major cause of economic instability—or at any rate, not yet! All I am arguing is that (i) there is no basis for the monetarists' suggestion that our postwar instability can be traced to monetary instability—our most unstable periods have coincided with periods of relative monetary stability; and (ii) stability of the money supply is not enough to give us a stable economy, precisely because there are exogenous disturbances.

Finally, let me mention that I have actually made an attempt at rerunning history to see whether a stable money supply would stabilize the economy, though in a way that I readily acknowledge is much inferior to the real thing, namely through a simulation with the *MPS*. The experiment, carried out in cooperation with Papademos, covered the relatively quiet period from the beginning of 1959 to the introduction of price-wage controls in the middle of 1971. If one eliminates all major sources of shocks, for example, by smoothing federal government expenditures, we found, as did Otto Eckstein in an earlier experiment, that a stable money growth of 3 percent per year does stabilize the economy, as expected. But when we allowed for all the historical shocks, the result was that with a constant money growth the economy was far from stable—in fact, it was distinctly less stable than actual experience, by a factor of 50 percent.

B. *The Overall Effectiveness of Postwar Stabilization Policies*

But even granted that a smooth money supply will not produce a very stable world and that there is therefore room for stabilization policies, monetarists will still argue that we should nonetheless eschew such policies. They claim, first, that allowing for unpredictably variable lags and unforseeable future shocks, we do not know enough to successfully design stabilization policies, and second, that the government would surely be incapable of choosing the appropriate policies or be politically willing to provide timely enforcement. Thus, in practice, stabilization policies will result in destabilizing the economy much of the time.

This view is supported by two arguments, one logical and one empirical. The logical argument is the one developed in Friedman's Presidential Address (1968). An attempt at stabilizing the economy at full employment is bound to be destabilizing because the full employment or natural rate is not known with certainty and is subject to shifts in time; and if we aim for the incorrect rate, the result must perforce be explosive inflation or deflation. By contrast, with a constant money supply policy, the economy will automatically hunt for, and eventually discover, that shifty natural rate, wherever it may be hiding.

This argument, I submit, is nothing but a debating ploy. It rests on the preposterous assumption that the only alternative to a constant money growth is the pursuit of a very precise unemployment target which will be adhered to indefinitely no matter what, and that if the target is off in the second decimal place, galloping inflation is around the corner. In reality, all that is necessary to pursue stabilization policies is a rough target range that includes the warranted rate, itself a range and not a razor edge; and, of course, responsible supporters of stabilization policies have long been aware of the fact that the target range needs to be adjusted in time on the basis of forseeable shifts in the warranted range, as well as in the light of emerging evidence that the current target is not consistent with price stability. It is precisely for this reason that I, as well as many other nonmonetarists, would side with monetarists in strenuous opposition to recent proposals for a target unemployment rate rigidly fixed by statute (although there is nothing wrong with Congress committing itself and the country to work toward the eventual achievement of some target unemployment rate through *structural* changes rather than aggregate demand policies).

Clearly, even the continuous updating of targets cannot guarantee that errors can be avoided altogether or even that they will be promptly recognized; and while errors persist, they will result in some inflationary (or deflationary) pressures. But the growing inflation to which Friedman refers is, to repeat, a crawl not a gallop. One may usefully recall in this connection the experience of 1965–70 referred to earlier, with the further remark that the existence of excess employment was quite generally recognized at the time, and failure to eliminate it resulted overwhelmingly from political considerations and not from a wrong diagnosis.[3]

There remains then only the empirical issue: have stabilization policies worked in the past and will they work in the future? Monetarists think the answer is negative and suggest, as we have seen, that misguided attempts at stabilization, especially through monetary policies, are responsible for much of the observed instability. The main piece of evidence in support of this contention is the Great Depression, an episode well documented through the painstaking work of Friedman and Anna Schwartz, although still the object of dispute (see, for example, Peter Temin). But in any event, that episode while it may attest to the power of money, is irrelevant for present purposes since the contraction of the money supply was certainly not part of a comprehensive stabilization program in the post-Keynesian sense.

When we come to the relevant postwar period, the problem of establishing the success or failure of stabilization policies is an extremely taxing one. Many attempts have been made at developing precise objective tests, but in my view, none of these is of much value, even though I am guilty of having contributed to them in one of my worst papers (1964). Even the most ingenious test, that suggested by Victor Argy, and relying on a comparison of the variability of income with that of the velocity of circulation, turns out to be valid only under highly unrealistic restrictive assumptions.

Dennis Starleaf and Richard Floyd have proposed testing the effectiveness of stabilization by comparing the stability of money growth with that of income growth, much as I have done above for the United States, except that they apply their test to a cross section of industrialized countries. They found that for a sample of 13 countries, the association was distinctly positive. But this test is again of little value. For while a negative association for a given country, such as suggested by my *U.S.* test, does provide some weak indication that monetary activism helped rather than hindered, the finding of a positive association across countries proves absolutely nothing. It can be readily shown, in fact, that, to the extent that differential variability of income reflects differences in the character of the shocks—a most likely circumstance for their sample—successful stabilization also implies a positive correlation between the variability of income and that of money.

But though the search for unambiguous quantitative tests has so far yielded a meager crop, there exists a different kind of evidence in favor of Keynesian stabilization policies which is impressive, even if hard to quantify. To quote one of the founding fathers of business cycle analysis, Arthur Burns, writing in 1959, "Since 1937 we have had five recessions, the longest of which lasted only 13 months. There is no parallel for such a sequence of mild—or such a sequence of brief—contractions, at least during the past hundred years in our country" (p. 2). By now we can add to that list the recessions of 1961 and 1970.

There is, furthermore, evidence that very similar conclusions hold for other industrialized countries which have made use of stabilization policies; at any rate that was the prevailing view among participants to an international conference held in 1967 on the subject, "Is the busi-

[3]Friedman's logical argument against stabilization policies and in favor of a constant money growth rule is, I submit, much like arguing to a man from St. Paul wishing to go to New Orleans on important business that he would be a fool to drive and should instead get himself a tub and drift down the Mississippi: that way he can be pretty sure that the current will eventually get him to his destination; whereas, if he drives, he might make a wrong turn and, before he notices he will be going further and further away from his destination and pretty soon he may end up in Alaska, where he will surely catch pneumonia and he may never get to New Orleans!

ness cycle obsolete?" (see Martin Bronfenbrenner, editor). No one seemed to question the greater postwar stability of all Western economies—nor is this surprising when one recalls that around that time business cycle specialists felt so threatened by the new-found stability that they were arguing for redefining business cycles as fluctuations in the *rate of growth* rather than in the *level* of output.

It was recognized that the reduced severity of fluctuations might in part reflect structural changes in the economy and the effect of stronger built-in stabilizers, inspired, of course, by the Keynesian analysis. Furthermore, the greater stability in the United States, and in other industrialized countries, are obviously not independent events. Still, at least as of the time of that conference, there seemed to be little question and some evidence that part of the credit for the greater stability should go to the conscious and on balance, successful endeavor at stabilizing the economy.

V. The Case of Supply Shocks and the 1974–76 Episode

A. *Was the 1974 Depression Due to Errors of Commission or Omission?*

In pointing out our relative postwar stability and the qualified success of stabilization policies, I have carefully defined the postwar period as ending somewhere in 1973. What has happened since that has so tarnished the reputation of economists? In facing this problem, the first question that needs to be raised is whether the recent combination of unprecedented rates of inflation as well as unemployment must be traced to crimes of commission or omission. Did our monetary and fiscal stabilization policies misfire, or did we instead fail to use them?

We may begin by establishing one point that has been blurred by monetarists' blanket indictments of recent monetary policy: the virulent explosion that raised the four-quarter rate of inflation from about 4 percent in 1972 to 6.5 percent by the third quarter of 1973, to 11.5 percent in 1974 with a peak quarterly rate of 13.5, can in no way be traced to an excessive, or to a disorderly, growth of the money supply. As already mentioned, the average rate of money growth from the beginning of 1970 to the second half of 1974 was close to 7 percent. To be sure, this was a high rate and could be expected sooner or later to generate an undesirably high inflation —but how high? Under any reasonable assumption one cannot arrive at a figure much above 6 percent. This might explain what happened up to the fall of 1973, but not from the third quarter of 1973 to the end of 1974, which is the really troublesome period. Similarly, as was indicated above, the growth of money was reasonably smooth over this period, smoother than at any other time in the postwar period, staying within a 2 percent band. Hence, the debacle of 1974 can just not be traced to an erratic behavior of money resulting from a misguided attempt at stabilization.

Should one then conclude that the catastrophe resulted from too slavish an adherence to a stable growth rate, forsaking the opportunity to use monetary policy to stabilize the economy? In one sense, the answer to this question must in my view be in the affirmative. There is ample ground for holding that the rapid contraction that set in toward the end of 1974, on the heels of a slow decline in the previous three quarters, and which drove unemployment to its 9 percent peak, was largely the result of the astronomic rise in interest rates around the middle of the year. That rise in turn was the unavoidable result of the Fed's stubborn refusal to accommodate, to an adequate extent, the exogenous inflationary shock due to oil, by letting the money supply growth exceed the 6 percent rate announced at the beginning of the year. And this despite repeated warnings about that unavoidable result (see, for example, the author 1974).

Monetarists have suggested that the sharp recession was not the result of too slow a monetary growth throughout the year, but instead of the deceleration that took place in the last half of 1974, and early 1975. But this explanation just does not stand up to the facts. The fall in the quarterly growth of money in the third and fourth quarters was puny, especially on the basis of revised figures now available: from 5.7 percent in the second to 4.3 and 4.1—hardly much larger than the error of estimate for quarterly rates! To be sure, in the first quarter of 1975 the growth fell to .6 percent. But, by then, the violent contraction was well on its way—between September 1974 and February 1975, industrial production fell at an annual rate of 25 percent. Furthermore, by the next quarter, monetary growth had resumed heftily. There is thus no way the monetarist proposition can square with these facts unless their long and variable lags are so variable that they sometimes turn into substantial leads. But even then, by anybody's model, a one-quarter dip in the growth of money could not have had a perceptible effect.

B. *What Macro Stabilization Policies Can Accomplish, and How*

But recognizing that the adherence to a stable money growth path through much of 1974 bears a major responsibility for the sharp contraction does not per se establish that the policy was mistaken. The reason is that the shock that hit the system in 1973–74 was not the usual type of demand shock which we have gradually learned to cope with, more or less adequately. It was, instead, a supply or price shock, coming from a cumulation of causes, largely external. This poses an altogether different stabilization problem. In particular, in the case of demand shocks, there exists in principle an ideal policy which avoids all social costs, namely to offset completely the shock thus, at the same time, stabilizing employment and the price level. There may be disagreement as to whether this target can be achieved and how, but not about the target itself.

But in the case of supply shocks, there is no miracle cure—there is no macro policy which can both maintain a stable price level and keep employment at its natural rate. To maintain stable prices in the face of the exogenous price shock, say a rise in import prices, would require a fall in all domestic output prices; but we know of no macro policy by which domestic prices can be made to fall except by creating enough slack, thus putting downward pressure on wages. And the amount of slack would have to be substantial in view of the sluggishness of wages in the face of unemployment. If we do not offset the exogenous shock completely, then the initial burst, even if activated by an entirely transient rise in some prices, such as a once and for all deterioration in the terms of trade, will give rise to further increases, as nominal wages rise in a vain attempt at preserving real wages; this secondary reaction too can only be cut short by creating slack. In short, once a price shock hits, there is no way of returning to the initial equilibrium except after a painful period of both above equilibrium unemployment and inflation.

There are, of course, in principle, policies other than aggregate demand management to which we might turn, and which are enticing in view of the unpleasant alternatives offered by demand management. But so far such policies, at least those of the wage-price control variety, have proved disappointing. The design of better alternatives is probably the greatest challenge presently confronting those interested in stabilization. However, these policies fall outside my present concern. Within the realm of aggregate demand management, the only choice open to society is the cruel one between alternative feasible paths of inflation and associated paths of unemployment, and the best the macroeconomist can offer is policies designed to approximate the chosen path.

In light of the above, we may ask: is it conceivable that a constant rate of growth of the money supply will provide a satisfactory response to price shocks in the sense of giving rise to an unemployment-inflation path to which the country would object least?

C. *The Monetarist Prescription: Or, Constant Money Growth Once More*

The monetarists are inclined to answer this question affirmatively, if not in terms of the country's preferences, at least in terms of the preferences they think it should have. This is evidenced by their staunch support of a continuation of the 6 percent or so rate of growth through

1974, 1975, and 1976.

Their reasoning seems to go along the following lines. The natural rate hypothesis implies that the rate of inflation can change only when employment deviates from the natural rate. Now suppose we start from the natural rate and some corresponding steady rate of inflation, which without loss of generality can be assumed as zero. Let there be an exogenous shock which initially lifts the rate of inflation, say, to 10 percent. If the Central Bank, by accommodating this price rise, keeps employment at the natural rate, the new rate of 10 percent will also be maintained and will in fact continue forever, as long as the money supply accommodates it. The only way to eliminate inflation is to increase unemployment enough, above the natural rate and for a long enough time, so that the cumulated reduction of inflation takes us back to zero. There will of course be many possible unemployment paths that will accomplish this. So the next question is: Which is the least undesirable?

The monetarist answer seems to be—and here I confess that attribution becomes difficult —that it does not make much difference because, to a first approximation, the cumulated amount of unemployment needed to unwind inflation is independent of the path. If we take more unemployment early, we need to take less later, and conversely. But then it follows immediately that the specific path of unemployment that would be generated by a constant money growth is, if not better, at least as good as any other. Corollary: a constant growth of money is a satisfactory answer to supply shocks just as it is to demand shocks—as well as, one may suspect, to any other conceivable illness, indisposition, or disorder.

D. *Why Constant Money Growth Cannot Be the Answer*

This reasoning is admirably simple and elegant, but it suffers from several flaws. The first one is a confusion between the price level and its rate of change. With an unchanged constant growth of the nominal money stock, the system will settle back into equilibrium not when the rate of inflation is back to zero but only when, in addition, the price level itself is back to its initial level. This means that when inflation has finally returned back to the desired original rate, unemployment cannot also be back to the original level but will instead remain above it as long as is necessary to generate enough deflation to offset the earlier cumulated inflation. I doubt that this solution would find many supporters and for a good reason: it amounts to requiring that none of the burden of the price shock should fall on the holder of long-term money fixed contracts—such as debts—and that all other sectors of society should shoulder entirely whatever cost is necessary to insure this result. But if, as seems to be fairly universally agreed, the social target is instead to return the system to the original rate of inflation—zero in our example—then the growth of the money supply cannot be kept constant. Between the time the shock hits and the time inflation has returned to the long-run level, there must be an additional increase in money supply by as much as the price level or by the cumulant of inflation over the path.

A second problem with the monetarists' argument is that it implies a rather special preference function that depends only on cumulated unemployment. And, last but not least, it requires the heroic assumption that the Phillips curve be not only vertical in the long run but also linear in the short run, an assumption that does not seem consistent with empirically estimated curves. Dropping this last assumption has the effect that, for any given social preference, there will be in general a unique optimal path. Clearly, for this path to be precisely that generated by a constant money growth, would require a miracle—or some sleight of the invisible hand!

Actually, there are grounds for holding that the unemployment path generated by a constant money growth, even if temporarily raised to take care of the first flaw, could not possibly be close to an optimal. This conclusion is based on an analysis of optimal paths, relying on the type of linear welfare function that appears to underlie the monetarists' argument, and which is also a straightforward generalization of Okun's famous "economic discomfort index." That index (which according to Michael Lovell appears to have some empirical support) is the sum of unemployment and inflation. The index used in my analysis is a weighted average of the cumulated unemployment and cumulated inflation over the path. The weights express the relative social concern for inflation versus unemployment.

Using this index, it has been shown in a forthcoming thesis of Papademos that, in general, the optimum policy calls for raising unemployment at once to a certain critical level and keeping it there until inflation has substantially abated. The critical level depends on the nature of the Phillips curve and the relative weights, but does not depend significantly on the initial shock—as long as it is appreciable. To provide an idea of the order of magnitudes involved, if one relies on the estimate of the Phillips curve reported in my joint paper with Papademos (1975), which is fairly close to vertical and uses Okun's weights, one finds that (i) at the present time, the noninflationary rate of unemployment corresponding to a 2 percent rate of inflation can be estimated at 5.6 percent, and (ii) the optimal response to a large exogenous price shock consists in increasing unemployment from 5.6 to only about 7 percent. That level is to be maintained until inflation falls somewhat below 4 percent; it should then be reduced slowly until inflation gets to 2.5 (which is estimated to take a couple of years), and rapidly thereafter. If, on the other hand, society were to rate inflation twice as costly as unemployment, the initial unemployment rate becomes just over 8 percent, though the path to final equilibrium is then shorter. These results seem intuitively sensible and quantitatively reasonable, providing further justification for the assumed welfare function, with its appealing property of summarizing preferences into a single readily understandable number.

One important implication of the nature of the optimum path described above is that a constant money growth could not possibly be optimal while inflation is being squeezed out of the system, regardless of the relative weights attached to unemployment and inflation. It would tend to be prevailingly too small for some initial period and too large thereafter.

One must thus conclude that the case for a constant money growth is no more tenable in the case of supply shocks than it is in the case of demand shocks.

VI. Conclusion

To summarize, the monetarists have made a valid and most valuable contribution in establishing that our economy is far less unstable than the early Keynesians pictured it and in rehabilitating the role of money as a determinant of aggregate demand. They are wrong, however, in going as far as asserting that the economy is sufficiently shockproof that stabilization policies are not needed. They have also made an important contribution in pointing out that such policies might in fact prove destabilizing. This criticism has had a salutary effect on reassessing what stabilization policies can and should do, and on trimming down fine-tuning ambitions. But their contention that postwar fluctuations resulted from an unstable money growth or that stabilization policies decreased rather than increased stability just does not stand up to an impartial examination of the postwar record of the United States and other industrialized countries. Up to 1974, these policies have helped to keep the economy reasonable stable by historical standards, even though one can certainly point to some occasional failures.

The serious deterioration in economic stability since 1973 must be attributed in the first place 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. This failure must be attributed in good measure to the fact that we had little experience or even an adequate conceptual framework to deal with such shocks; but at least from my reading of the record, it was also the result of failure to use stabilization policies, including too slavish adherence to the monetarists' constant money growth presciption.

We must, therefore, categorically reject the monetarist appeal to turn back the clock forty years by discarding the basic message of *The General Theory*. We should instead concentrate our efforts in an endeavor to make stabilization policies even more effective in the future than they have been in the past.

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