

**Section I: Stabilization Policies:  
Theoretical and  
Empirical Issues**

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## RECENT DEVELOPMENTS IN THE THEORY OF STABILIZATION POLICY

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During the past decade the theoretical framework underlying macroeconomic stabilization analysis has undergone a number of significant developments. Theories designed to explain the crucial linkage between aggregate demand policy and real economic variables have been revised following the research on the "new microfoundations" of employment and inflation. Critical expectations effects of stabilization policy have been incorporated into the theoretical framework through the use of rational expectations. Optimal control techniques have become sophisticated enough to be used on large nonlinear econometric models, and more recently have been adapted for use in models with endogenous expectations. Supply considerations have been recognized as having important policy implications and, when necessary, have been incorporated into policy analyses. Theories underlying the choice between rules and discretionary policy have been altered and refined. These developments are likely to play an important role in the practical evaluation of economic policy in the years ahead.

This paper reviews these developments in the theory of stabilization policy and outlines some of their implications for macroeconomic policy evaluation. The first section reviews the theories which have

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John B. Taylor is Professor of Economics, Columbia University. The author wishes to thank Robert Barro, Jerry Green, Dale Henderson, and Laurence Meyer for helpful comments on an earlier draft, and the National Science Foundation for financial support.

been developed to explain the effect of policy variables on the real economy. As there is still little consensus here, a number of alternative representative models are presented and compared. The second section examines the implications of these different theories for the problem of reducing the rate of inflation, which is likely to be one of the more important policy issues in the years ahead. The third section discusses a number of issues which have arisen in recent policy analyses and which are closely related to the changes in the theoretical framework: The Lucas critique of traditional policy evaluation procedures, the applicability of optimal control, the choice of rules versus discretion, and the applicability of the new equilibrium approach to stabilization policy.

With few exceptions this review focuses on theoretical research on domestic stabilization policies. International considerations and empirical results are reviewed in other papers prepared for this conference. Some of the topics reviewed here have recently been the subject of a large number of survey and expositional works. The variety of survey papers by Barro (1979), Buiter (1979), Fischer (1979), McCallum (1979), Phelps (1979), Prescott (1977), Santomero and Seater (1978), and Shiller (1978) and the books by the Ball committee (1978), and Sargent (1979) provide further detail and alternative perspectives on the topics reviewed here.

Expectations play a predominant role in any discussion of stabilization analysis. For the discussion that follows, the benchmark assumption will be that expectations are formed rationally. Variations from this benchmark -- due perhaps to the necessity of people gradually learning about whether the economy has undergone a structural change -- are

considered in the course of the discussion along with variations in the model underlying the policy analysis.

#### THEORIES OF AGGREGATE DEMAND EFFECTS ON REAL OUTPUT AND EMPLOYMENT

In the idealized world of complete markets with perfect information about opportunities in all markets, changes in the money supply -- or more generally, changes in aggregate demand -- do not affect real economic variables such as real GNP and employment. Apart from distribution effects, aggregate demand fluctuations are translated point-for-point into price fluctuations. Money is neutral. Many of the theoretical developments in macroeconomics in the 1970s have been concerned with explaining, in more detail and with more rigor than earlier theories, why this neutrality is not observed in the real world. A reasonably firm understanding of the mechanism generating this non-neutrality is certainly necessary for evaluating stabilization policy because aggregate demand management tools, such as money growth and government expenditure plans, are the primary instruments of stabilization policy.<sup>1</sup>

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<sup>1</sup>The effects of government policies which impact directly on relative prices can be evaluated in principle using the standard allocative theories of microeconomics. Some examples: a relative lowering of tax rates on capital would be expected to stimulate investment by raising the desired capital-labor ratio; a higher steady rate of inflation has allocation effects by acting as a tax on real money balances; and unemployment insurance can raise the equilibrium unemployment rate by driving a wedge into the work-leisure tradeoff. Apart from disagreement over the magnitude of the relevant elasticities for measuring these effects, there has been a general consensus among economists that such policies have real effects. However, because these policies are used for allocative or distributional purposes, they are not generally flexible enough to be considered seriously in stabilization analysis. Nevertheless, their importance cannot be overlooked in analyzing macroeconomic trends. See Feldstein (1978) for a summary of such effects on unemployment.

Recent theories of the observed link between aggregate demand and real variables can be grouped into two types -- information-based theories in which the uncertainties about economy-wide disturbances are emphasized, and contract-based theories in which temporary rigidities in prices and wages are emphasized. At the risk of becoming too taxonomic, it will be useful to further classify each of these theories. Among the information-based theories it is important to distinguish between those in which the uncertainty is whether an observed economic change is local or economy-wide, and those in which the uncertainty is whether an economic change is temporary or permanent. Similarly, among the contract-based theories it is important to distinguish between those that emphasize relative price shifts due to asymmetrical rigidities (for example, wages are rigid while prices are flexible), and those that emphasize the general persistence of all prices due to non-synchronous price (or wage) setting relative to a prevailing trend in prices (or wages).

#### Uncertainty about Local Versus Aggregate Economic Conditions

Perhaps the most significant finding of the research<sup>2</sup> on the "new microeconomics" is that imperfect information about economic conditions outside an individual's own market or industry can have profound implications for the behavior of inflation and employment. Suppose aggregate demand increases because of a higher rate of money growth. Then individual firms will find an increased demand for their products, and will respond by increasing their production (and perhaps running down

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<sup>2</sup>See Phelps et al (1970).

their inventories of finished goods). But much of this higher real production may be due to the misperception on the part of each firm that the increased demand is a relative shift toward the product it sells. Because there is always imperfect information about whether an increase in sales is a local phenomenon, this misperception and the consequent real output response is unavoidable. If, on the contrary, each firm knew that the increase in demand was common to all firms in the economy, and was due to the purely nominal increase in the money supply, then its production response would be much smaller. If prices and wages were generally flexible, then firms would know that prices and wages should quickly rise to offset the increase in the money supply, and therefore that an increase in output would not be warranted. In the limiting case of perfectly flexible prices, good information about what is going on elsewhere in the economy enables firms to respond just as they would be predicted to do in the money-neutral world of general equilibrium theory. But even with perfectly flexible prices, imperfect information creates a non-neutrality in which firms respond to aggregate demand stimulus by increasing real output. The link between aggregate demand and real variables, according to this theory, depends in no essential way on price or wage rigidities. As long as there is imperfect information about the source of aggregate demand shifts, the correlation between aggregate demand and real output will exist. Of course, the possibility of a coincidence of perfectly flexible prices and wages with these well-known empirical correlations means that policy implications will be much different.

Simple descriptions of this theory are found in Phelps et al. (1970) and Lucas (1973). The algebra of the Lucas presentation is

convenient for our purposes and can be represented in terms of a simple quantity theory of aggregate demand.

$$(1) \quad y + p = m + v$$

combined with an "aggregate supply" equation

$$(2) \quad y = \alpha(p - \hat{p}).$$

All variables are measured in logarithms and should be thought of as deviations from secular trends:  $y$  is real GNP,  $p$  is the aggregate price index,  $m$  is the money supply, and  $v$  is velocity. The  $\hat{p}$  term represents a forecast of the price level before the information about  $m$  and  $v$  becomes available. The difference between  $p$  and  $\hat{p}$  represents the average difference between each firm's observation of demand conditions during the period and its guess about economy-wide demand conditions. This difference represents the misperception or mistake discussed above which causes firms to increase their production. The sum of all firms' production responses is  $y$ . (It turns out that it is convenient algebraically to use prices to index demand conditions.)

Substituting from (1) into (2) and noting that from (2) that  $\hat{y}=0$ , we find<sup>3</sup>

$$(3) \quad y = \alpha(m - \hat{m} + v - \hat{v}).$$

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<sup>3</sup>We take  $\hat{p}$  to be a rational (unbiased) forecast of  $p$ ; hence  $E(p-\hat{p}) = 0$ . "Biased" forecasts are treated in Section 1.2 below and arise because of information confusion about what is the actual model underlying policy or the structure of the economy. These "biased" forecasts have forms which resemble adaptive expectations, but unlike adaptive expectations are closely related to the structure of the model.

Hence real output responds positively to unanticipated money  $m-\hat{m}$  and unanticipated velocity  $v-\hat{v}$ . This is the critical link between real variables and aggregate demand which the theory explains.

However, because only unanticipated changes in aggregate demand affect real output, the policy implications of this linkage theory are striking: if the monetary authorities change their policy instrument  $m$  in a way which can be predicted by individuals in the economy, then in our notation  $m=\hat{m}$  and the change in  $m$  does not affect real output at all. And from equation (1) the change in  $m$  is translated entirely into a point-for-point change in  $p$ , apart from any unanticipated shifts in velocity. This famous "policy-ineffectiveness" result, emphasized by Lucas (1973), Sargent and Wallace (1975) and Barro (1976), has understandably stimulated a large volume of research.

The significance of this theory for practical stabilization analysis is not simply the neutrality result -- the idealized general equilibrium model has long been known to yield neutrality as discussed above. Rather the significance is due to the appearance of neutrality in a model which explains the empirically observed correlation between aggregate demand policy and real output. The theory would be of little practical importance if it did not generate this important empirical result. The econometric work of Sargent (1976) and Barro (1977, 1978), has been aimed at making this empirical connection more formal and rigorous.

I think it is fair to say that this empirical work has demonstrated that the theory is consistent with these correlations. Other facts have been more difficult to reconcile with the theory. The persistence of unemployment is one regularity which does not emerge from

the simple theory, and was used as a critique of the theory by Hall (1975) and Modigliani (1977). A number of modifications of the theory to account for this persistence have been suggested. Lucas (1975) emphasized that unanticipated shocks could cause firms' capital stock to get out of line, and this would have repercussions on production in later periods as the capital stock is adjusted. Sargent (1979) emphasized adjustment costs in changing employment. Blinder and Fischer (1978) have placed more emphasis on finished-goods inventory being drawn down or accumulated. Optimal inventory adjustments in later periods will then require production changes and thereby cause a correlation between output changes at different dates. All these theoretical modifications of the basic information-based model with perfectly flexible prices can in principle explain persistence, but it has yet to be demonstrated whether actual inventory behavior or costs of employment adjustment are sufficient to explain the persistence.

There is, of course, much other evidence which the theory can be tested against. Two pieces of evidence which seemingly run counter to the theory are procyclical productivity changes, and a slight tendency for real wages to vary procyclically, though the latter is much less pronounced. Sargent (1979), extending the work of Lucas (1970), has shown, however, that these observations are consistent with the limited-information flexible price models. His proof involves disaggregating employment into straight-time and over-time, and assuming that straight-time employment is more costly to adjust, but that over-time workers must be paid more on average. Under these conditions firms will find it optimal to employ more straight-time workers than over-time workers on average, but to make larger changes in employment among over-time

workers than straight-time workers, when demand conditions change across the business cycle. This behavior implies that real average hourly earning will tend to increase during booms, because of the shift of the mix of workers toward higher paid overtime employment, even though real wages may fall for both groups of workers. Moreover, since fewer over-time workers are employed on average than straight-time workers, their marginal productivity is higher. Hence, the shift toward more over-time employment causes average productivity in the economy to increase. Sargent (1978) has attempted to see if this intricate theory is sufficient to explain the phenomena quantitatively, and finds that, although there are some discrepancies, the theory generally conforms to the facts. Another explanation for the procyclical behavior of real wages is given in Phelps (1969) using a model of inventory behavior. New data now becoming available on real inventories may permit a check of this explanation.

From the point of view of stabilization theory a number of extensions of the basic information-based model represented in equation (2) should be mentioned. Cukierman (1979) has shown that the limited-information assumptions can be generalized to permit firms to change their expenditures in order to better determine the source of economy-wide events. This makes the information structure endogenous to the rest of the economy, including policy, and thereby removes the criticism that the theory unrealistically places an exogenous information structure on economic agents. He finds that the general results of the theory are robust with respect to this modification.

McCallum and Whitaker (1979) have shown that the policy neutrality result does not apply to such aggregate demand tools as automatic

stabilizers because these react simultaneously to changes in economic conditions, rather than with a lag as in the feedback monetary policy discussed above. For example, with progressive taxes, after-tax income immediately changes as a fraction of total income when nominal income fluctuates. This can have direct real stabilizing effects. It should be emphasized, however, that in principle monetary policy could be made to operate just as simultaneously as the automatic stabilizers. This has not been the case in practice, however, except for extreme interest rate pegging where the central banks' supply of reserves responds instantaneously to changes in demand.

#### Uncertainty about Temporary Versus Permanent Changes in Economic Conditions

The theory discussed above emphasizes lack of information about whether demand changes are local or economy-wide. From the viewpoint of stabilization policy, an equally important type of uncertainty is the lack of information about whether an observed economic change is temporary or permanent. Theories which emphasize temporary versus permanent effects are, of course, not new to macroeconomics, as exemplified by Friedman's (1956) original permanent income theory of consumption. Muth (1960, 1961) also emphasized the distinction in his original work on rational expectations. Here we are concerned with the importance of this uncertainty for the link between aggregate demand and real output. The general point is that a shift in nominal aggregate demand, which is expected to be permanent will have a much smaller effect on real output and a correspondingly larger effect on prices, than a shift which is expected to be temporary.

Suppose, for example, that in an attempt to reduce the rate of inflation the central bank reduces the growth rate of the money supply. The information problem which economic agents face is whether this change is a permanent one, or whether the central bank will soon give up on its resolve to lower the growth rate of the money supply. In reality, this information problem is not trivial, and cannot be eliminated simply by announcing that today's start at monetary restraint is the beginning of a permanent shift in policy. Lack of credibility about whether the shift is indeed permanent may be cured only by the public observing the results of the new policy.

During the transition period when people learn whether the shift is temporary or permanent, the policy of restraint can have real output effects, even if prices are perfectly flexible. This can be illustrated using the algebra introduced above.<sup>4</sup> Equation (2) can be written in terms of inflation rates rather than price levels by subtracting the lagged price from  $p$  and  $\hat{p}$ . This gives

$$(4) \quad y_t = \alpha(\pi_t - \hat{\pi}_t)$$

when  $\hat{\pi}_t$  is the expected rate of inflation. Suppose that  $\pi_t = \hat{\pi}_t$  so that there is initially no uncertainty, but that starting in period  $t+1$  the central bank reduces the rate of growth of the money supply to a level that will generate an inflation rate of  $\pi_s < \hat{\pi}_t$  for  $s > t$ . If the new policy is not fully credible, then people will not immediately adjust their expectations to  $\pi_s$ . A reasonable assumption would be that they expect a level of inflation which incorporates the new information

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<sup>4</sup>The following discussion is based on Taylor (1975).

about  $\pi$  as well as the previously expected rate of inflation. In simple terms:

$$(5) \quad \hat{\pi}_s = \lambda \pi_s + (1-\lambda)\hat{\pi}_{s-1} \quad s = t+1, t+2, \dots$$

Formula (5) can be derived more formally using Bayesian techniques which incorporate the uncertainty about whether the new inflation rate is permanent or whether the observed change is a temporary occurrence. The parameter  $\lambda$  will be time dependent in general, however, and this should be taken into account if one is interested in quantitative policy evaluation.

To see the effects of the new monetary policy on real output assume for simplicity that  $\pi_s$  is equal to a constant  $\pi^*$  for  $s > t + 1$ . Then from (5) we have

$$(6) \quad \hat{\pi}_s = \lambda \sum_{i=0}^{s-1} (1-\lambda)^i \pi^* + (1-\lambda)^s \hat{\pi}_t$$

for  $s \geq t + 1$ . Hence,  $\hat{\pi}_s$  converges to  $\pi^*$ , but will be greater than  $\pi^*$ , if  $\pi^*$  is less than  $\hat{\pi}_t$  (if the new monetary policy is to aim for a lower rate of inflation). The gap between the expected rate of inflation  $\hat{\pi}_s$  and the actual rate of inflation  $\pi^*$  will be larger, the smaller is  $\lambda$ . Hence, the less credibility there is about the new policy, the larger the inflation gap and the larger the reduction in real output. There will be no reduction in real output if  $\lambda=1$ . In this way the uncertainty about permanent versus temporary effects has an important influence on the way policy is linked to real economic variables.

The type of model represented here in very simple terms has been emphasized in stabilization policy analyses by Fellner (1976),

B. Friedman (1979), and Taylor (1975). A full macroeconomic model developed by Brunner, Cukierman, and Meltzer (1979) uses the distinction between permanent and temporary effects to examine the influence of supply shocks as well as demand shocks on production. Flood and Garber (1979) have provided estimates of similar credibility parameters in the case of monetary reform in the German hyper-inflation.

These types of models have been criticized, especially when used for policy analyses of the type discussed here, because they appear to depend on policy deception (see Barro (1978)). While the potential for deception is clearly present in these models they are equally applicable to situations where all parties disclose their intentions. Unfortunately, disclosure does not generate immediate credibility. It is the problem associated with this lack of credibility which these models emphasize.

#### Contracts and Relative Price Effects

Imperfect information is not the only reason that aggregate demand would be expected to influence real output. Temporary rigidities in prices or wages might force some of the change in nominal demand into changes in real production. Since casual observation suggests that such rigidities are pervasive either in the form of explicit contracts or less formal implicit contracts, economists have been willing to take these rigidities as given. The main theoretical development in this area during the past several years has been to recognize that the form which these rigidities takes is important for stabilization analysis. Attempts have been made to model these rigidities with more detail than was previously available, and to trace out

the implications for policy. Two different forms of this type of analysis can be usefully distinguished.

The most common form of this type of model assumes that wages are at least temporarily rigid, but that prices are perfectly flexible in the sense that firms cannot directly influence profit margins by marking up their prices relative to wage costs. Firms simply adjust their demand for labor when the real wage shifts against them. Recent examples of this type of model are found in Fischer (1977), Phelps (1978), and Calvo (1980). Letting  $w_t$  represent the nominal wage and keeping the notation introduced earlier, the most rudimentary form of this model is

$$(7) \quad y_t = \alpha(p_t - w_t).$$

When the real wage rises firms reduce output and employment, until the marginal productivity of labor is increased. If  $w_t$  is partially pre-determined, perhaps because of multiperiod contracts which were set in previous periods, then the link between aggregate demand and real output follows directly. If aggregate demand is determined according to equation (1) then

$$(8) \quad y_t = \frac{\alpha}{1+\alpha} m_t + \frac{\alpha(v_t - w_t)}{1+\alpha}$$

and clearly changes in nominal  $m_t$  get translated into real output. The mechanism is simply that a higher money supply raises prices which lowers the real wage and stimulates employment and production.

The major advance in using this type of model has been to develop the mechanism determining the nominal wage. Fischer assumes, for

example, that there are overlapping contracts with a fraction of the contracts set in each period so as to keep the expected real wage constant. A consequence of this assumption is that aggregate demand effects do not persist for longer than the length of the longest contract. Another consequence is that wage or price trends have no tendency to persist. In these two respects this type of model has many features which are similar to the results of the information-based models. This has led Gramlich (1979), for example, to conclude that wage-rigidities do not add much in the way of policy implications to a rational expectations models. In principle, of course, announced monetary policy affects real variables in such models, even with rational expectations. This has been emphasized by Fischer (1977). The question is whether they describe the wage and price dynamics in an empirically accurate way that is relevant for policy analysis.

The main feature of these models is their dependence on real wage changes for all employment effects. As discussed above, it has been difficult to find much variation in the real wage over the business cycle. Empirical checks of this model along the lines of Sargent (1978) using the distinction between straight-time and over-time workers would therefore be very useful.

On the other hand, there are important policy problems where changes in real wages are the central issue. For example, a supply shock could shift the marginal productivity downward requiring a reduction in the real wage. With sticky wages, this reduction might be difficult without monetary intervention. In effect the monetary authorities can use monetary policy to shift the price level to a position such that the real wage is equal to the level which workers

would have aimed for, if they had known about the shock when they signed the contract. This is the conclusion of Phelps (1978) who bases his analysis on such a model. Gordon's (1975) analysis of agricultural supply shocks reaches a similar conclusion if farm prices shift up while industrial prices are assumed to be relatively rigid. Blinder (1979) also emphasizes these relative price rigidities in examining the appropriate response of policy to an oil price shock. One difficulty with all these analyses is the possibility that the assumed rigid wage (or price) eventually adjusts to offset the policy-induced shift in relative prices. In the Phelps analysis, this is not much of a difficulty in principle because the real wage is pushed toward what workers and firms would have negotiated otherwise. Another difficulty, already alluded to, is that the models do not capture much of the persistence effects of inflation and unemployment which now seem to present important policy problems. In this respect they are similar to the information-based models reviewed above.

#### Staggered Contracts and Inflation Persistence

By most measures the variability of the general price level in recent years has been larger than the variability of all but a small number of relative prices. For example, the real wage has been relatively stable compared with the sharp rise in nominal wages and prices. Moreover, changes in both nominal wages and prices are more highly correlated with business cycle fluctuation than changes in the relative wage. For these reasons, one might suspect that analyses which focus on real wage changes as the sole cause of employment shifts might be omitting other factors.

Another class of models which are based on rigidities in wages and prices deemphasize the aggregate effects of relative price shifts and focus on the problems of general price movements. These models emphasize the fact that all prices and wages are not set in unison across the economy but are generally staggered, and that a primary determinant of the price decision is the prevailing price outstanding in the market. Hall (1979) has recently developed a microeconomic model which gives an explanation for the importance of setting prices relative to the prevailing price.

An example of this type of model is given in Taylor (1979). Firms and workers decide on a wage  $x_t$  in period  $t$  which is to last for two periods. The contract wage  $x_t$  is set according to the expected prevailing wage during the contract period with suitable adjustments to reflect demand conditions. Hence

$$9) \quad x_t = \frac{\hat{w}_t + \hat{w}_{t+1}}{2} + \frac{\alpha}{2}(\hat{y}_t + \hat{y}_{t+1})$$

here  $w_t = 1/2(x_t + x_{t-1})$  is the average wage at time  $t$ . The expectations of  $y_t$  represent demand pressure on wage decisions. If we make the additional assumption that profit margins are relatively stable then  $p_t = w_t + \gamma$  where  $\gamma$  is a constant parameter which we can set to zero without loss of generality. By holding the relative wage constant, the model purposely abstracts from relative price changes and focuses on general price movements.

In this model, as with the previous model based on price rigidities, aggregate demand policy has a direct effect on real output. If equation (1) is the aggregate-demand relationship, then the mechanism

works as follows: the price level is predetermined since the wage is predetermined and profit margins do not adjust. Hence, an increase in the money supply increases real balances, which tends to increase the real demand for goods. This results in an increase in production and hence an increase in employment. Eventually wages and prices will adjust because the favorable demand conditions will give firms the incentive to pay increased wage demands. This in turn tends to raise prices and reduce real money balances. Eventually a new equilibrium is reached at a higher price level but with the same level of production. Money is neutral in the long run.

What is different about this model compared with those discussed in the previous section is that convergence to the new equilibrium takes time, and there is never any important shift in relative wages (there is a period during which the workers who had settled their contracts when the money supply was changed tend to fall behind other workers but this is not necessarily integral to the workings of the model). The inertia in wage movements following the shift in money supply can be demonstrated by solving the model to obtain<sup>5</sup>

$$(10) \quad x_t = \beta x_{t-1} + \delta m_t$$

where  $\beta$  and  $\delta$  depend on the parameter  $\alpha$ . Hence, a change in the money supply sets off a series of changes in the contract wage  $x_t$  and hence in the average wage  $w_t$ . This series of changes in  $w_t$  is matched by the price level  $p_t$  and, if the money supply is held fixed at the new level,

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<sup>5</sup>The derivation requires the use of rational expectations to solve out for the expectation variables.

is reflected in a similar pattern of changes in real output. Because of these persistence effects this type of model would seem to be more useful for examining stabilization problems associated with reducing inflation, or more generally achieving price stability, than the models discussed in the previous section. If changes in real wages are also thought to be important, then they can easily be incorporated into the analysis. Theoretical frameworks of this kind have been used for policy analysis by Phelps (1978a), Gertler (1977), Modigliani and Papademos (1978), Papademos (1979), and Taylor (1980).

These models have some similarities to the "disequilibrium" models developed by Clower (1965) and Barro and Grossman (1976). Important differences not generally found in "disequilibrium" models are the use of rational expectations, a reasonably explicit description of the contract mechanism, and a reliance on the more traditional aggregate demand framework without the development of market spillover effects or of binding supply constraints. These differences largely reflect empirical considerations or modelling strategies. It is not yet clear what is to be gained empirically or theoretically from incorporating disequilibrium spillover effects. A recent paper by Green and Honkapohja (1979) has attempted to bring rational expectations into a framework which corresponds more closely with the disequilibrium models. However, their approach is designed to avoid explicit treatment of the nonlinearities caused by setting market transactions equal to the minimum of supply and demand. Rational expectations are much easier to deal with in linear models, and this is one reason the "demand is determining" assumption is used. Another reason is that the assumption seems to be empirically realistic in many situations.

### Comparison of the Alternative Theories

What sets the contracting models off from the information-based models is of course the use of "sticky" prices, and the corresponding disuse of the market-clearing assumptions. In the contract models, markets "clear" in the short run in the sense that supply adjusts to meet the demand; in the long run, prices eventually adjust to clear markets. In the information models, on the other hand, prices instantaneously adjust to clear markets in the short run. Which approach is better? I have used the contracting approach because it corresponds more closely with my interpretation of the market mechanisms in the real world. It is not just the widely discussed long-term labor contracts which suggest this interpretation, but also the much more common (at least in the U.S.) implicit contracts, which are much shorter and are usually not called contracts. In fact, long-term labor contracts have so many indexing provisions that they probably correspond more closely with shorter contracts. Research in this area has shown that "contracts" do not have to be very long to generate a very lengthy persistence of wage and price inflation. (See Taylor (1980), for example.) But in using these contracting models, one has to be aware that without an explicit utility maximization framework, there is a possibility that the models are not robust to changes in policy. Again my preference has been to make the most of these models in situations where the contracting mechanisms are relatively robust.

At the same time, it is difficult not to appreciate the theoretical elegance of the information models, and the potential to use the traditional tools of microeconomics to conduct policy analysis with these models. But even the information-based models have some ad hoc

assumptions, especially when they need to be modified for empirical work. One of the major recent developments in the literature on market-clearing rational expectations has been to pursue a more theoretically rigorous approach with the aim of omitting the remaining ad hoc features, in particular the money demand equation or quantity theory equations (such as equation (1) in this paper). See Wallace (1977) and Cass and Shell (1979).<sup>6</sup>

The work by Azariadis (1975), Baily (1974), and D. F. Gordon (1976) does not provide as much of a foundation for contract models as one might have originally thought. These theories do not suggest why contracts are set in nominal terms without contingencies. In fact, Barro (1979) has suggested that these microeconomic theories are more useful in showing that the market-clearing models are useful "as if" devices. Calvo and Phelps (1978) and Hall and Lilien (1979) have provided alternative theories of contracts which emphasize the practical and theoretical difficulties of making contracts contingent on everything.

Most of the policy discussions associated with the theories reviewed above have been about the effectiveness of policy or whether policy activism is useful or not. In the market-clearing setting, only

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<sup>6</sup>A useful appraisal of the overlapping generations model approach advocated by Wallace is contained in Cass and Shell (1979). The major appeal of this approach is the enormous theoretical mileage one gets from the disaggregation of generations. At an abstract level this disaggregation is very similar to the disaggregation of contracts according to when they are negotiated -- a feature of the contracting models discussed in Section 1.4. More generally one suspects that different types of disaggregation are likely to yield additional theoretical insights. Another example is the two-sector model explored by Sargent and Wallace (1971), Henderson and Sargent (1973), and Foley and Sidrauski (1970).

unanticipated changes in aggregate-demand policy matter, so announced policies do affect output. In contracting models aggregate-demand policy has effect whether it is anticipated or not. Hence, in these models, policy is effective and, in certain cases, policy activism is desirable. Some examples of the optimal reaction to supply shocks were discussed above.

McCallum (1977) has argued that price rigidities are not really the source of the policy effectiveness in the contracting models. In criticizing the contract model used by Phelps and Taylor (1977) he shows that monetary policy is ineffective if one removes inventory effects on production, but uses the supply equation in the form of equation (2). However, inventory effects on production are an important part of models where prices do not adjust to clear markets. Firms will want to increase production, for example, if inventories are drawn down below optimal levels because price adjustments are not quick enough. This is the rationale behind the inventory effects on production in the Phelps-Taylor model. Omitting the term attributes suboptimal inventory management to rational firms. This point has been demonstrated by Frydman (1979) in a critique of McCallum's results.

The main outcome of the policy-effectiveness debate is a general consensus that rational expectations per se does not rule out effective aggregate-demand management. It is the flexible-price market-clearing assumption that makes policy ineffective for short run stabilization policy.<sup>7</sup>

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<sup>7</sup>Fischer (1978) and Lucas (1975) mention the nonneutrality that comes even in market-clearing models from the substitution out of money into real capital when the expected rate of inflation rises. However, this mechanism is not seriously considered as a tool of aggregate demand-management. Moreover it is likely to be offset by tax effects. A useful discussion of the relationship between rational expectations and policy effectiveness is found in Lucas (1980).

## POLICIES TO STABILIZE PRICES

The practical policy implications of these models can be alternatively stated from the viewpoint of price stabilization rather than from the viewpoint of policy intervention to affect output. Suppose, for example, that the rate of inflation is generally agreed to have become too high, either because of past policy mistakes or unavoidable velocity shifts, and that the monetary authorities want to reduce the rate of inflation. The important question is whether the monetary restraint necessary to achieve this goal of price stabilization will cause a recession and how large that recession will be. The answer to that question will obviously influence the policymakers' choice of how much restraint to apply.

If we take literally the information-based models, which emphasize the uncertainty between aggregate and local shocks, then if this policy of restraint is announced it will not have any effect on real output. There will be no recession since inflation will match the reduction in monetary growth point for point. This striking conclusion is, of course, contrary to the views of many economists and policymakers, and I think for this reason the model is still rejected by many economists as a practical guide to policy.

On the other hand, if there is uncertainty about whether the changes in policy are permanent or temporary (as discussed above), then the real effects of policy will exist, and a recession would be expected to occur. The size and duration of the recession would depend on the speed with which people begin to believe that the central bank is firm in its resolve to restrain money growth. If the credibility is high or increases quickly, then the recession could be very mild.

Fellner (1979) indicates why he thinks that credibility is likely to increase quickly, if a clear announced policy of restraint is undertaken, and that people's expectations of inflation would be swiftly revised downwards.

The contract-based models yield different conclusions. The models which emphasize real wage shifts because of asymmetric rigidities do not suggest any reason for a recession to last longer than the length of the average contract. The inflation rate could be put on its new target path in the first period; in the second period wages would adjust. In fact, if the restraining policy was announced and believed one period (year?) in advance, there would be no decline in output. In this case, this type of contract model does not give results that are much different from the market clearing models.

The general staggered contract models suggest, on the other hand, that the recession would be somewhat longer because the adjustment process is passed on gradually from one contract to the next. However, because there are some forward-looking features to these models (see equation (9)), the recession would not be expected to be as severe as would be implied by the simple reduced forms (see equation (10)). The policy of restraint (if it is believed) would change the parameters of (10), so as to reduce the size of the recession. Accurate quantitative estimates of how much the parameters would be expected to change have yet to be obtained, though simulation results in Taylor (1980) suggest that it is likely to be significant.

In sum, each of the models reviewed here has implications about the real effects of a policy of price stabilization. (These models ignore, of course, any direct positive real effects that a more certain

price level might bring; see Fischer and Modigliani (1979) for a discussion of these direct effects.) In the cases where the real effect is likely to be significant, it would be interesting and useful to compare empirically its magnitude with the estimates provided by conventional econometric techniques as summarized by Okun (1978). This is a feasible and well-defined estimation problem as the discussion above makes clear.

#### ALTERNATIVE TECHNIQUES FOR THE ANALYSIS OF STABILIZATION POLICY

This section gives an overview of several recent developments concerning the choice of alternative techniques to analyze stabilization policy. Some of these issues are intimately connected with the theoretical developments summarized in the first section.

#### The Lucas Critique of Econometric Policymaking

Econometric models have played a large role in policy formulation in recent years. It is rare that the staff members of policymaking agencies do not run alternative policies through the major large scale econometric models before meeting with their "principals," even if they do not have formal models of their own. Whether this heavy use of econometric models actually influences the decisions of policymakers is another question. Political or other noneconomic considerations are frequently a factor. But when "pure" economic advice is sought, the results of the econometric models are certainly taken into account. For example, the property of almost all econometric models that nonaccommodative monetary policy has small effects on prices and large effects on output, undoubtedly influences policymakers to choose more accommodative policies than they otherwise would.

Lucas (1976) has criticized this type of econometric policymaking. He argues convincingly that the parameters of these models are not invariant to changes in policy, so that the policy experiments performed on these models (which treat the parameters as fixed) give misleading results. R. J. Gordon (1976) suggests that suitable modifications of econometric policy evaluation procedures could deal with the Lucas criticism. The parameters could, in principle, be made endogenous.

The parameters of econometric models can shift for many reasons, but the one Lucas emphasized was that rational economic agents would forecast the future effects of policy, and accordingly, modify their behavior in a way not described in the econometric models. To deal with this problem it is necessary at least to reestimate the econometric models taking these expectation effects into account. The most practical way to do this with existing econometric techniques is to use the rational expectations assumption. Having specified and estimated an econometric model with rational expectations it is then possible to perform a policy analysis to take account of the expectations effects. This is the approach taken by Taylor (1979a). A simple quarterly econometric model of the U. S. economy was estimated during the 1954-1976 period, imposing rational expectations on economic agents. Using the estimated parameters of this model, alternative policies were compared, and for a given set of policy preferences, optimal policies were calculated. Because the model incorporated contracts of the kind discussed above, a policy tradeoff between inflation and unemployment was implied by the model and this was calculated using the estimated parameters. The tradeoff was characterized by a "best" relationship

between output stability and price stability.<sup>8</sup> This optimal relationship apparently dominated actual policy during the period as well as the policy of a constant growth rate for the money supply. Constant money growth would have given better results than actual policy, however, according to these estimates.

Anderson (1979) and Fair (1979) have tried to estimate the quantitative significance of the Lucas critique by simulating conventionally estimated econometric models, with rational expectations inserted. They both find the effects to be quantitatively significant, but their results are difficult to interpret because the conventional models were not formulated as rational expectations models. For example, Anderson (1979) finds that the Phillips curve is much steeper when he imposes rational expectations on the model. But clearly the specifiers of his model would have altered its specifications if they knew rational expectations would be imposed. It is likely that the adaptive expectations distributed lags used in such models are designed to capture other dynamic properties than pure extrapolative forecasting.

Quantitative work of this kind with rational expectations is only just beginning. More experience with these techniques will be necessary before they can be accurately appraised as significant improvements over conventional econometric policy evaluation procedures. The results available thus far are promising, are already giving rough

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<sup>8</sup>Flemming (1976) p. 73 suggests that a tradeoff between output stability and price stability might be a good way to characterize the policy problem. Phelps and Taylor (1977), Taylor (1980), and Green and Honkapohja (1979) have calculated theoretical tradeoffs of this kind. An international comparison of such tradeoffs is given in Taylor (1980a).

empirical estimates of the effect of policy, and indicate that further research is fruitful.

Two objections can be raised against these attempts to account for the Lucas critique. One is that the rational expectations assumption is not accurate because it does not incorporate learning on the part of individuals about the economy. If this learning problem is significant, then these techniques will have to be modified. Learning effects are likely to be a serious empirical problem immediately following a major economic reform. This was illustrated above for the case where the monetary authorities change their policy and people do not know whether it is a permanent or temporary change. However, even if learning problems are significant, these techniques will be useful for evaluating alternative policy procedures over a long period of time. For example, it is useful to know if a less accommodative monetary policy during the 1960s and 1970s would have increased the amplitude of business cycle fluctuations as much as conventional econometric models would imply. If the use of rational expectations gave results much different from other models over long enough periods for the rational expectations assumption to be realistic, then the results would be taken into consideration in recommending how accommodative policy should be in the 1980s.

Another objection to the quantitative use of rational expectations as described here is that there are other reasons that parameters of a model could change. For example, even if rational expectations were used, behavioral relations for contract-wage determination might shift with policy as workers and firms change contract lengths. While expectations are probably a significant source of parameter drift, this

does not mean that models can ignore other behavioral shifts. Successful policy evaluation requires careful modelling of all behavioral relations.

#### The New Equilibrium Approach to Policy Evaluation

Lucas and Sargent (1978) have suggested that the pervasiveness of these other sources of parameter shifts means that minor modifications of econometric models are not sufficient. They recommend a "new equilibrium" approach to modelling in which all economic relations are based on explicit utility maximization analysis. If tastes and technology remain relatively constant -- or can be modelled as exogenous factors -- then this approach, in principle, will avoid these other types of parameter shifts. The approach is attractive because once one has developed a model based on sound utility maximization principles, macroeconomic policy analysis is conducted like any other welfare analysis in microeconomics. Explicit externalities can be located and offset by optimal policies, and no approximate aggregate welfare criteria such as output and price stability are necessary. One would design policy to maximize the welfare of the representative individual. Attempts to design business cycle or econometric models along these lines include the work by Barro (1976), Lucas (1975), Hansen and Sargent (1980) and Kydland and Prescott (1980).

This approach represents a fundamental change in macroeconomic policy evaluation and its full practical implementation will take a long time as emphasized by Lucas and Sargent (1978). As an alternative to the approach outlined in the previous section, several reservations about this new equilibrium approach might be mentioned. Does utility

maximization provide any additional constraints on an economic model which do not already come from a set of explicit decision rules and rational expectations? If it does not, then the gains from beginning each analysis with explicit utility maximization are not clear. For example, one of the major ad hoc features of decision rules designed for empirical work is that they include lags to capture the gradual adjustment of firms to new economic conditions. With utility maximization, these lags are "explained" by adjustment costs which tend to make it optimal for firms to adjust slowly. But one has almost as much freedom to choose adjustment costs in a utility framework as one does to choose lag length when writing down decision rules. Unless good micro-economic or technological information is available to measure these adjustment costs, the utility maximization approach does not seem to provide additional information in this case.

Another reservation concerns the practical use of the welfare of the representative individual as the criterion for stabilization policy. In principle this approach is better than the alternative approach of postulating an aggregate measure of welfare, which might include measures of inflation or aggregate employment stability. But the aggregate welfare approach has advantages in practice. It is very difficult to incorporate some of the welfare gains of price stability into individual utility functions. The gains from a relatively stable aggregate price level involve such considerations as providing a more certain framework for private decision making. Until one finds a way to incorporate these complex effects into individual utility functions, the use

of aggregate criteria may serve as satisfactory and workable alternatives.

#### Rules Versus Discretion

The debate between those favoring rules versus discretion has not diminished in recent years but the arguments have been modified. A definitional change is that rules are now rarely taken to mean holding policy instruments constant. Feedback rules, in which the money supply responds in a systematic way to economic developments, are rules as much as constant money growth.

Kydland and Prescott (1977) have suggested that the problem of time inconsistency (see also Calvo (1979)), implies that rules should be used rather than discretion. Time inconsistency can arise because of taste change or because people forecast future behavior of policymakers. In both cases policymakers may be tempted to change plans after they have announced the optimal path. Time inconsistency does not imply that optimization techniques cannot be used (see Fischer (1980) for a discussion of this issue), but it does raise questions of how policy should be implemented. Kydland and Prescott (1977) argued that rules would be a way to reduce the incentive for policymakers to change plans. Rules do not generally exploit the initial conditions of a maximization problem as much as fully optimal policies. If policymakers do not exploit initial conditions today, then people might expect that they will not exploit initial conditions in the future. But of course there is no logical guarantee. This

preference for rules over discretion is a practical, rather than a logical, implication of time inconsistency problems.<sup>9</sup>

Another practical reason to prefer rules over discretion is that, especially with rational expectations, it is difficult to estimate the impact of alternative discretionary paths with great accuracy. The rational-expectations assumption is not accurate unless one can assume people are familiar with how policy works; this might require that they have experience with one type of rule for a long period of time.<sup>10</sup>

Fischer (1979) has suggested a compromise resolution to the rules versus discretion debate: rules should be used in normal times, but in the case of an unanticipated disaster (such as a financial panic) discretion should come into play. It is difficult to disagree with this eclectic solution to the problem, but practical implementation might prove difficult. Objective measures of what is normal and what is abnormal are difficult to obtain in economics.

A less constructive, but perhaps more realistic resolution to the rules versus discretion debate comes from deemphasizing the distinction between the two. If policymakers make the same policy decision whenever their staffs' econometric forecasts are the same, then in effect

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<sup>9</sup>Monetarists who advocate the use of a fixed money growth rule, suggest that, because of initial conditions (a high inflation rate inherited from the past), the growth rate be diminished to the target path slowly when starting out on such a plan. There is a time inconsistency argument here. If higher rates of money growth are advocated because of initial condition, then what is to keep people from expecting a return to high money growth when similar conditions arise again in the future?

<sup>10</sup>Another practical reason is that statistical estimates of policy effects are considerably less complex if one can focus on rules.

they are using rules. The rules might be difficult to describe and even more difficult to estimate, but they are rules nonetheless. If this is a good description of the way policy works, then research which focuses on alternative rules rather than discretionary paths might turn out to be the more practically useful type of policy research. Such research might suggest ways in which the policymaking process (rule) should be modified in order to improve the performance of the economic system.

#### CONCLUDING REMARKS

This overview has been aimed at recent theoretical research in stabilization theory. Earlier research on such issues as the choice of intermediate targets, problems of lags in the effect of policy, and the effect of parameter uncertainty on the choice of policy instrument has been omitted largely because theoretical developments in these areas have been relatively minor in recent years. It should be emphasized that these older problems continue to be of practical importance. The continuing efforts to persuade the Fed to switch to a reserve targeting procedure in their short-run operating strategy is a case in point.

The practical interpretation of these earlier stabilization issues has been changed in some cases, however, by the theoretical developments reviewed in this paper. For example, Poole's (1970) analysis of the choice of policy instrument loses most of its practical relevance in the market-clearing models where monetary policy is ineffective. But in the contracting models, where monetary policy effects on real output are significant, Poole's analysis needs only slight modifications to account for the rational expectations effects. Interest rate

pegging frequently leads to instability in rational expectations models, whether prices are flexible or temporarily rigid. This policy implication, which was emphasized by Sargent and Wallace (1975), appears to be robust to change in the theory which is used.<sup>11</sup> That many other important policy implications are not robust to changes in alternative theories -- as was emphasized here for the policy objective of price stabilization -- suggests that additional theoretical and empirical work to sort out and test these theories should be high on any agenda for future research on stabilization policy.

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<sup>11</sup>Such instability can occur in the model used by Phelps and Taylor (1977) for example. Because prices are set at levels which clear markets on average, market-clearing conditions are used to determine expected future prices which in turn are used to determine the current price setting. Extreme interest rate pegging can make future prices and hence the current price level undetermined.

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## EMPIRICAL EVIDENCE ON THE EFFECTS OF STABILIZATION POLICY

Laurence H. Meyer and Robert H. Rasche

Macroeconometric research in the 1970s has been dominated by the refinement of large-scale income-expenditure macroeconometric models, the attempt to reconcile the policy multipliers derived from these models with those yielded by simple reduced-forms, the refinement and estimation of the relation between inflation and unemployment, and the application of optimal control techniques to macroeconometric models. These four themes provide the focus for this paper.

The first section reviews the implications of various macroeconometric models for monetary and fiscal multipliers. We are particularly concerned here with the degree of consensus across models and the evolution of estimated models over time. The second section discusses attempts to reconcile the divergent implications of income-expenditure structural models and the St. Louis reduced-form for fiscal policy multipliers. In the third section we develop the implications of estimated Phillips curve equations and monetarist models for the response of unemployment, output, and inflation to traditional demand management policies. And in the fourth section we consider the accumulated evidence on the gains from policy activism, drawing on the results of optimal control simulations with a variety of macroeconometric models.

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During the last half of the '70s increased attention has been focused on the way in which economic agents form expectations, particularly inflation expectations, and on "equilibrium" macroeconomic models embodying "rational expectations." These models yield dramatic conclusions about both the costs of eradicating inflation and the gains from activism. We therefore consider the implications of rational expectation models in both the third and fourth sections, although there is as yet only a small literature on empirical applications of these models to draw upon.

#### A COMPARISON OF POLICY MULTIPLIERS ACROSS MODELS AND TIME

In this section we review the evidence from structural models and reduced-forms about the size and time pattern of policy multipliers. We are interested in the average size of multipliers, the consensus across models, and the evolution over time in the estimated multipliers.

##### A Comparison of Multipliers Across Models

Christ (1975) has summarized the consensus across models rather pessimistically: "... though models forecast reasonably well over horizons of four to six quarters, they disagree so strongly about the effects of important monetary and fiscal policies that they cannot be considered reliable guides to such policy effects, until it can be determined which of them are wrong in this respect and which (if any) are right." (p. 54)

Tables 1, 2, and 3 present policy multipliers from seven econometric models (Bureau of Economic Analysis (BEA), Brookings (B), University of Michigan (MQEM), Data Resources, Inc. (DRI), Federal Reserve Bank of St. Louis (St.L), MIT-Pennsylvania-SSRC (MPS), and Wharton (W))

as reported in Fromm and Klein (1976). The multipliers are reported for the first quarter and fourth, eighth, twelfth, sixteenth, and twentieth quarters and for three policy changes -- an increase in real government expenditures on goods and services, a decline in personal taxes, and an increase in either the money supply or nonborrowed reserves. The mean and coefficients of variation for the various multipliers are also reported.<sup>1</sup>

TABLE 1  
Fiscal Policy - Increase in Government Expenditures

Model	IC*	RMSE(4Q)*	Multiplier					
			1Q	4Q	8Q	12Q	16Q	20Q
BEA	62	6.94	1.1	2.2	2.2	1.8	1.6	1.3
B	56I	5.13	1.8	2.8	2.7	2.4	2.0	1.5
MQEM	62I	6.20	1.4	1.7	1.4	1.0	1.0	1.1
DRI 74	61I	4.60	1.3	2.1	2.2	2.0	1.7	1.7
St.L	62I	4.98	0.5	0.5	-0.2	-0.2	-0.2	-0.2
MPS		4.23	1.2	2.2	2.2	0.7	-0.5	
W	65I	4.64	1.3	2.0	2.4	2.6	2.4	1.9
Mean (w/o St.L)			1.35	2.17	2.18	1.75	1.37	1.17
St. dev. (w/o St.L)			0.24	0.36	0.43	0.76	1.03	0.86
s.d./mean			0.18	0.17	0.20	0.43	0.75	0.74
Mean (w/St.L)			1.23	1.93	1.84	1.47	1.14	.97
St. dev. (w/St.L)			.39	.71	.98	1.01	1.11	.94
s.d./mean			0.32	0.37	0.53	0.69	0.97	0.97

\* IC = initial conditions for policy simulation; RMSE = root mean square error for four quarter forecast of real GNP (billions of dollars at 1958 prices) over 1961-1967 period.

<sup>1</sup>The multipliers are reported with and without the St. Louis model multipliers. The latter are based on a reduced-form income equation rather than on a structural model and, particularly in the case of the fiscal multipliers, differ substantially from the multipliers based on the structural models.

The mean fiscal expenditure multiplier is just over 1-1/4 in the first quarter and builds to 2-1/4 by the end of year two; however, the cumulative multiplier is still over one after five years. While there is considerable consensus about the multipliers through the first three years, the agreement deteriorates sharply. Note that in all cases the multiplier peaks within three years, generally within four to eight quarters; and cumulative fiscal multipliers fall to zero or below by the fifth quarter for the St. Louis model, by the 12th to 16th quarter for the MPS model and by the 24th quarter for the BEA model. But it

TABLE 2  
Fiscal Policy - Tax Cut

Model	Multiplier				
	1Q	4Q	8Q	12Q	16Q
BEA	0.4	1.2	1.4	1.1	0.8
B	1.0	1.6	1.6	1.6	1.5
MQEM	0.6	1.2	1.1	1.1	1.2
DRI 74	0.9	1.3	1.2	0.9	0.6
St.L*	0	0	0	0	0
MPS	0.4	1.3	2.1	2.2	1.8
W	0.5	1.2	1.7	1.9	1.6
Mean (w/o St.L)	0.63	1.30	1.52	1.47	1.25
St. dev. (w/o St.L)	0.26	0.16	0.37	0.52	0.47
s.d./mean	0.41	0.12	0.24	0.35	0.38
Mean (w/St.L)	0.54	1.11	1.30	1.26	1.07
St. dev. (w/St.L)	0.34	0.51	0.66	0.73	0.64
s.d./mean	0.63	0.46	0.51	0.58	0.60

\* Multipliers reported for St. Louis model are based on absence of a tax variable in the model's reduced-form equation for income.

takes eight to ten years for the cumulative multiplier to reach zero in the Wharton and Michigan models and still longer in the Brookings and DRI models.<sup>2</sup>

The tax multipliers are smaller than the expenditure multipliers; they build from an initial mean value of 0.63 to a peak of 1.5 at the end of the second year. In the case of a tax change, there is less consensus in the first quarter, but no deterioration in later quarters. The tax multipliers tend to peak a bit later than the expenditure multipliers, generally between the 8th and 12th quarters, and then decline.

TABLE 3  
Monetary Policy

Model	MV*	Multiplier				
		1Q	4Q	8Q	12Q	16Q
BEA	RU	0	0.2	0.4	0.7	0.7
DRI	RU	0.3	4.1	8.3	6.5	2.8
St.L	M1	1.1	4.4	2.8	1.2	-0.4
MPS	RU	0.3	3.2	8.4	12.4	14.5
W	RU	1.4	4.5	7.2	8.6	8.0
Mean (w/o St.L)		0.5	3.0	6.08	7.05	6.50
St. dev. (w/o)		1.24	0.65	0.63	0.69	0.95

\* MV = monetary variable (M1 = narrow money supply; RU = nonborrowed reserves; initial conditions same as in Table 1.

<sup>2</sup>Note also that the fact that the cumulative multiplier turns negative does not guarantee a negative long-run multiplier since these models are subject to oscillatory convergence to their long-run values.

There are only four comparable multipliers for monetary policy (those using nonborrowed reserves). The initial quarter mean multiplier is small and the mean multiplier peaks at the end of the third year at a value of 7. There is less consensus about monetary compared to fiscal policy; the coefficient of variation is larger in all but one quarter for monetary policy multipliers. While the St. Louis cumulative multiplier peaks in the fourth quarter and goes to zero by the 16th quarter, large scale model multipliers generally peak after 8 to 12 quarters and the MPS multiplier reported by Fromm and Klein is still rising from the 12th to 16th quarters. The large scale models thus suggest that monetary policy has a more persistent effect on output than is the case in the St. Louis model. The exception is the DRI model in which the cumulative monetary policy multiplier falls to zero by the 20th quarter.

While the multiplier results do differ across models there is clearly considerable consensus particularly over the first two years in the case of fiscal policy when we exclude the St. Louis results. The problem is evaluating how much divergence in the multipliers is consistent with using the models for policy recommendations. Later we will discuss the use of stochastic simulations which allow for multiplier uncertainty within a particular model. Here we want to note the valuable approach suggested by Chow (1977). Chow notes that while policy recommendations derived from alternative structural models differ from each other, they may nevertheless be closer to each other than to a passive policy of constant growth rates in the policy instruments. The comparison Chow suggests and implements is the improvement in economic performance in one model using optimal policy derived from

a second model relative to the economic performance under passive policy. Chow uses the multiplier properties of the Wharton and Michigan models to construct reduced-form equations for real and nominal GNP including government expenditures and nonborrowed reserves as the policy instruments and employs a conventional quadratic loss function involving deviations in real and nominal GNP from their targets (in each case average historical values over the period in question).

The results of this experiment are mixed. If the Michigan model were the true structure and the policy recommendations were derived from the Wharton model, active policy would improve performance relative to a passive policy; costs under the active policy would be under 25 percent of those under a passive policy although they would be 70 percent greater than if the policy were derived using the true structure. On the other hand, if the Wharton model were the true structure and the policy recommendations were derived from the Michigan model, the cost under an active policy would be three times the cost of a passive policy and about 17 times the cost when the true model was used. And, of course, the Michigan and Wharton multipliers are quite close at least for fiscal policies, compared to say the Brookings and the St. Louis models. Thus there are other comparisons that would lead to even less favorable results for activism.

#### A Comparison of Policy Multipliers Over Time

We expected to find a secular decline in the value of fiscal multipliers and a secular rise in monetary policy multipliers for large scale econometric models from the late '60s versions to the versions of the mid- to late '70s. However, published information on such

multipliers is relatively scarce and what is available is frequently not constructed on a comparable basis. This, of course, increases the value of the NBER/NSF model comparison studies but makes multiplier comparisons pieced together from the literature hazardous. Perhaps the most serious problems for comparing multipliers across models or over time are differences in initial conditions and differences in the specification of policy instruments, particularly for monetary policy. The large scale models are invariably nonlinear, implying that their multipliers are sensitive to initial conditions, particularly the degree of economic slack. But there is painfully little reported evidence of the degree of this sensitivity. There are a bewildering number of possibilities for a change in tax rates and even differences in multipliers for different government expenditure components. The most serious problem, however, may be differences in assumptions about the monetary policy instrument. Monetary policy, particularly in the late 60s versions, has been identified with changes in short-term interest rates. In other cases, monetary policy is identified with either the money supply or some reserve aggregate, most often nonborrowed reserves. The choice affects both monetary and fiscal multipliers since fiscal multipliers assume unchanged monetary policy; fiscal multipliers will, of course, be much larger under fixed short-term interest rates than under fixed values of the money supply or nonborrowed reserves.

In Tables 4 and 5 we have pieced together some policy multipliers for alternative versions of Michigan, Wharton, and MPS models. The Michigan '70 and Wharton '68 models assume constant short-term interest rates while the others assume constant unborrowed reserves. It is surprising (to us at least) that the fiscal multipliers in the late '60s

TABLE 4  
Real Non Defense Government Expenditure Multipliers - Real GNP

Q	Michigan 70 <sup>a</sup>	Michigan 75 <sup>b</sup>	Wharton 68 <sup>c</sup>	Wharton 75 <sup>b</sup>	Wharton 79 <sup>d</sup>	MPS 69 <sup>e</sup>	MPS 75 <sup>b</sup>
1	1.5	1.4	2.0	1.3	1.1	1.3	1.2
4	2.1	1.7	2.0	2.0	1.7	1.8	2.2
8	1.9	1.4	2.0	2.3	1.8	1.6	2.2
12	n.a.	1.0	2.1	2.6	1.7	1.1	0.7

- a S. H. Hymans and H. T. Shapiro, "The DHL-III Quarterly Model of the U.S. Economy," Research Seminar in Quantitative Economics, University of Michigan, 1970, Table 4, p. 22.
- b C. Fromm and L. R. Klein, "The NBER/NSF Model Comparison Seminar: An Analysis of Results," in L. R. Klein and E. Burmeister (ex), Econometric Model Performance, Pennsylvania, 1975, Table 6, p. 402.
- c M. K. Evans and L. R. Klein, The Wharton Econometric Forecasting Model, Economics Research Unit, University of Pennsylvania, 2nd ed., 1968, Table 5, p. 58.
- d Unpublished Wharton multiplier simulations kindly provided by R. M. Young, Wharton Econometrics Forecasting Associates.
- e F. DeLeeuw and E. M. Gramlich, "The Channels of Monetary Policy," Federal Reserve Bulletin, June 1969, Table 4, p. 489. Shock applied fully to federal real wage payments.

versions of the three models (including the two with constant short-term rates) are so small; they peak at 2.0 or less. One important difference in the later versions of Michigan and MPS models is the sharp decline in the cumulative multiplier from its peak value by the 12th quarter. There was a tendency in earlier versions for multipliers to stabilize at about 1.5-2.0 for a longer period. This continues to be the case in the Wharton model; in both the '75 and '79 versions the fiscal multipliers are stable or rising during the first three years.

We have been able to find comparable unborrowed reserves multipliers at different points in time for only two models: the Wharton model and the MPS model. These are reported in Table 5. In these models there is a fairly dramatic evolution of the monetary policy multiplier. In the 1968 Wharton model the unborrowed reserves multiplier for real GNP reached a fairly constant level in the 1.5 to 2.0 range after about one year. In the MPS model the multiplier is stable in the 10.0 range during the second and third years. In the later

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TABLE 5  
Unborrowed Reserve Multipliers  
(Real GNP/Nominal Reserves)

	Wharton 68 <sup>c</sup>	Wharton 75 <sup>b</sup>	Wharton 79 <sup>d</sup>	MPS 69 <sup>e</sup>	MPS 75 <sup>b</sup>
1	0.0	1.4	1.2	0.7	0.3
4	1.5	4.5	4.8	5.4	3.2
8	2.1	7.2	9.1	10.0	8.4
12	1.7	8.6	13.3	12.4	9.4

Notes - See Table 4.

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versions of both models, the multiplier is continually growing over the first three years. Note also the substantial increase in the size of the monetary policy multipliers in the Wharton model from the '68 version to the '75 and '79 versions. We view the Wharton '68 multipliers as fairly typical of the conventional wisdom of the mid- to late '60s, prior to the development of the MPS model.

#### COMMENTS ON THE "ST. LOUIS" EQUATION

Since the original Andersen-Jordan article (1968) (AJ) that proposed a single equation test of the relative importance of monetary and fiscal policies on nominal GNP, numerous replications have been performed, across time, across countries, and across functional forms and a number of criticisms, mostly statistical in nature, have been levied against the equation. The purpose of this section is to review the criticisms that have been raised against the equation and to evaluate how robust the equation appears to be against these criticisms.

The conclusions of the Andersen-Jordan investigation are by now almost universally known. The conclusion that remains most controversial is the zero cumulative fiscal multiplier for nominal GNP. This conclusion did not conform well to the conventional wisdom of the late 1960s, nor was it consistent with other econometric results. Consequently, for the past decade there has been considerable skepticism of the specification that yields this conclusion.

#### Time Periods, Functional Forms, and Distributed Lags

The AJ equation was estimated over the period 52/I-68/II and subsequently reestimated by Andersen and Carlson (1970) (AC) over the 53/I-69/IV period as part of the St. Louis model. In each case

monetary policy had a powerful and significant effect while the tax variable (change in high employment receipts) was insignificant and excluded from their preferred regression and the government expenditure variable had only a small and transitory effect. Silber (1971) subsequently split the period into Republican (53/I-60/IV) and Democratic (61/I-69/IV) administrations and found that fiscal variables were significant in the latter but not in the former. Silber argued that these results are consistent with the more systematic use of fiscal policy in the latter period. At a minimum, these results suggest that the time period used in the estimation can dramatically affect the conclusions and that the estimates may reflect the particular policies pursued over the estimation period.

More recently Friedman (1977) has extended the sample period employed by AC through 76/II and concluded that "even the St. Louis equation now believes in fiscal policy." In Table 6 we report the results of the AJ and AC equations along with estimates over alternate time periods including Silber's two subperiods (S1 and S2), Friedman's extended period (F), and the period 1960/I-1976/II (MR). The results suggest that both money and the time period matter! The size and significance of fiscal policy multipliers is not definitely settled by these results.

In response to Friedman, Carlson (1978) has pointed out that the first difference form of the estimated equation, while appropriate over the AC period, is not appropriate over the longer period because of heteroskedasticity, implying that the t values of coefficients reported by Friedman are unreliable. When all variables are defined as rates of change, Carlson finds that the results of the two periods are

TABLE 6

## Time Periods

Sample	AJ 52/I-68/II	AC 53/I-69/IV	S1 53/I-60/IV	S2 61/I-69/I	F 53/I-76/II	MR 60/I-76/II
M	5.83 (7.25)	5.57 (8.06)	5.58 (.43)	9.20 (2.35)	4.94 (6.3)	5.72 (1.07)
G	0.17 (0.54)	0.05 (0.17)	-1.77 (.90)	1.75 (2.11)	1.42 (4.3)	2.44 (5.57)
T			2.36 (.67)	-3.92 (2.78)		-1.67 (2.90)
R <sup>2</sup>	.60	.66	.652	.73	.66	.69
Se	4.01	3.84	4.23	3.30	7.54	7.84

consistent with the hypothesis that the specification is stable and, like the original AC equation, indicate that any effect of government expenditures is small and temporary. Allen and Seaks (1979), using the growth rate specification, find that the fiscal variable sums to zero in both Silber subperiods (Eisenhower and Kennedy-Johnson) but is significant in the Nixon-Ford era (69/II-77/I). Over the period 60/I-76/II we find that both expenditure and tax variables enter significantly into both first difference and rate of change specifications. In Table 7 we report the results of the AC equation in difference form over both the original period (AC) and over Friedman's extended period (F) and in rate of change form over Friedman's extended period (C) along with the Allen-Seaks results over the Nixon-Ford period (AS) and both functional forms over the 1960/I-76/II period (MR1 and MR2). From these results we can conclude that money, time period, and functional form matter.

The results of AJ type equations are estimated using polynomial distributed lags. This technique requires selection of lag length, degree of polynomial, and end point constraints. Schmidt and Waud (1973) caution that introduction of inappropriate constraints can result in biased and inconsistent estimates and demonstrate how changes in degree of polynomial and end point constraints can substantially alter the conclusions about policy multipliers. Others have found length of lag can affect conclusions also.

We can conclude, therefore, that the choice of time period, functional form, and lag constraints matters a great deal. The results for money appear very robust. The results for fiscal policy are dramatically affected by these factors.

TABLE 7

## Functional Form

Sample	AC 53/I-69/IV		F 53/I-76/II		C 53/I-76/II		AS 69/II-77/I		MRI 60/I-76/II		MR2 60/I-76/II	
	Form*	Delta	Delta	Delta	Dot	Dot	Dot	Delta	Delta	Dot	Dot	
M		5.57 (8.06)	4.94 (6.3)	1.06 (5.59)	.90 (1.93)			5.72 (5.31)		.75 (3.08)		
G		0.05 (0.17)	1.42 (4.3)	.03 (.40)	.36 (2.07)			2.44 (5.57)		.37 (2.82)		
T								-1.67 (2.90)		-.29 (2.25)		
R <sup>2</sup>		.66	.66	.40	.56			.69		.42		
Se		3.84	7.54	3.75				7.84		3.02		

\* Delta: first difference specification  
 Dot: rate of change specification

### Biases Associated With Choices of Independent Variables

The inconsistency between the AJ/AC reduced-form multipliers and the multipliers in large-scale econometric models generated a search (on both sides of the controversy) for an explanation. Monetarists criticized large-scale econometric models for failing to capture the crowding-out phenomenon through misspecification of the money demand equation (e.g. excluding a wealth effect) and failure to explicitly include a government financing constraint. The income expenditure counterattack focused on the unreliability of reduced-forms due to a variety of problems, some more easily correctable than others, associated with the choice of independent variables. The key issues have been: What are appropriate measures of the policy instruments? How can the possibility of reverse causation be avoided? What biases are introduced by omission of nonpolicy exogenous variables?

### The Measurement of Policy Instruments

There are two interrelated problems with specifying the policy instruments. The first is the problem of specifying the instrument that the policy authority directly controls. For example, if the Fed sets policy by controlling the value of the monetary base, employing a monetary aggregate other than the monetary base as a proxy for the policy instrument may bias the policy multipliers if the other aggregate varies endogenously relative to the base. A second problem arises even if the instruments themselves are included if policy itself systematically responds to economic developments. In this case, the policy instruments themselves become endogenous and reverse causation again may bias the multiplier results. In this section we take up the

problem of specifying the policy instruments and in the next the problem of endogeneity of policy.

The problem of reverse causation was noted in a DeLeeuw-Kalchbrenner (1969) comment on the AJ paper. Indeed it was the concern over this issue that arose out of the Friedman-Meiselman debates that motivated the choice of the high employment fiscal policy measures by Andersen and Jordan. DeLeeuw and Kalchbrenner's main concern is with the choice of the monetary base or money supply as the variable the Fed directly controls. They point out that the choice among the monetary base, the nonborrowed base, total reserves, and nonborrowed reserves depends on whether the Fed offsets the effect of movements in member bank borrowing on the base and of movements in currency holdings on reserves. They express no special preference among these alternate measures suggesting only that results which hold for some measures and not for others should be viewed with great caution. Their empirical results indicate that fiscal multipliers are affected by the choice of monetary instrument; in particular, fiscal multipliers of approximately the size produced in the MPS model result when nonborrowed reserves are substituted for the monetary base.

The treatment of fiscal instruments in the AJ/AC equations has also drawn considerable comment. In order to avoid the bias associated with the income induced movements in tax revenues and expenditures (mostly transfer payments) under preexisting schedules of tax and transfer rates, the AJ/AC equations use high employment expenditures. High employment receipts were tried but dropped from the preferred equation due to lack of significance. The high employment surplus was also employed in an alternate specification.

The latter is clearly an inappropriate measure of stimulus associated with fiscal actions because it groups components which are expected to have different multiplier responses. The same problem arises even in the case of high employment expenditures because this variable includes both expenditures on goods and services and transfers while economic theory suggests that transfers should be netted against taxes. Suggestions for improved specification of fiscal variables have been made by DeLeeuw-Kalchbrenner (DK), Gramlich (1971), and Corrigan (1970). Gramlich employs government purchases of goods and services rather than high employment expenditures, and assumes no adjustment is necessary to purge it of effects of changes in income. Government expenditures are employed in a composite variable including grants-in-aid and exports with an adjustment introduced for defense inventory accumulation.

DeLeeuw and Kalchbrenner suggest adjusting high employment receipts to purge changes in this variable of the effects of endogenous movements in prices. Gramlich uses high employment net tax revenues (taxes minus transfers) also adjusted along lines suggested by DK. The difficulty with all these series for tax revenues is that the series for changes include nonzero entries in periods during which no changes in tax rates or transfer programs occurred. Corrigan has suggested an alternate tax variable, the initial stimulus measure, that indicates the tax revenues released or absorbed by tax rate changes. This series has plenty of zeros! For each tax, the initial stimulus measure is the change in tax rates times the lagged tax base. An unweighted sum for all taxes is the variable Corrigan used and it continues to be used in the New York Fed version of the St. Louis equation.

The discussion above suggests that the simple specification of both monetary and fiscal instruments employed in the AJ and AC equations may be improved upon and that such improvements might alter the relative importance of monetary and fiscal multipliers. However, the modifications suggested above have not generally resulted in dramatic changes in the estimated multipliers in simple reduced-form equations. While many of these suggestions seem valid, they have not helped to resolve the differences between the St. Louis equation and econometric models.

#### Endogeneity of Policy

Even if we obtain measures of direct policy actions, our estimates of their effects will be biased if these actions themselves are systematically related to economic developments. This problem has widely been noted in comments on the AJ equation, but most critics including DeLeeuw and Kalchbrenner considered the problems in measuring the instruments the more likely source of bias. The biases associated with endogenous policy are easy to illustrate. If a policy instrument varies in response to disturbances so as to eliminate completely the instability in income, the regression of the change in the policy variable on changes in income (zero by assumption) will yield a zero coefficient on the policy instrument. Thus, endogeneity of policy may result in a downward bias in the policy multiplier, with the downward bias a function of the effectiveness of policy. We can, therefore, interpret the zero multiplier on fiscal instruments as evidence of their effectiveness rather than of their insignificance! While the endogeneity of policy may introduce biases into the estimates of policy

multipliers from both reduced-form equations and structural models, Goldfeld and Blinder (1972) suggest on the bases of simulation results that the bias is much more serious for reduced-forms. If policy responds to economic developments with a lag, the bias is reduced but not eliminated.

#### Omitted Exogenous Variables

The third major source of bias in the choice of independent variables in the AJ/AC equation is alleged to be the omission of non-policy exogenous variables. Andersen and Jordan explained in an appendix to their original paper why they believed that the omission of other exogenous variables did not bias their measured impact of the monetary and fiscal policy variables: these variables are presumed to be independent of monetary and fiscal policies and their average effect is registered in the constant term. Modigliani (1971) made the first detailed critique of the St. Louis reduced-form model on the grounds of omitted variables and Modigliani and Ando (1976) reported a more extensive set of simulation results supporting their view that omission of exogenous variables may severely bias the results of reduced forms.

The ingenious simulation experiments involved estimation of an AJ type equation on data generated by non-stochastic simulations of a model. The model represents the known structure of a hypothetical economy. The simulated values of nominal income from the model are the "actual" values of income in the hypothetical economy. A reduced-form is estimated using these simulated values for income, and the resulting estimated multipliers are compared with their "true" values (the values implied by the structural model). The comparison of the reduced-form

multipliers with their "true" (structural model) values tests the ability of simple reduced-forms, including only a couple of policy instruments, to replicate the true value of the policy multipliers.

In the 1971 paper, Modigliani emphasized the finding that the estimate of the St. Louis equation on MPS simulated values yielded a money multiplier in excess of the "true" MPS multiplier and reached the "unequivocal conclusion" that reduced-form money multipliers are upward biased. This bias was attributed to positive correlation between the money supply and omitted exogenous variables. For example, if the Fed attempts to stabilize interest rates (as monetarists assert they often do), then the money supply will be positively correlated with real sector exogenous demand variables and the monetary policy multiplier can be expected to be biased upward.

Modigliani and Ando (1976) turned their attention to biases in the estimates of fiscal effects and suggested that correlation between omitted exogenous variables and fiscal instruments in this case might account for the small size and transitory effects of fiscal instruments in the St. Louis equation. Estimates of the AJ type equation on values of the change in nominal income based on simulations with the MPS model yield fiscal multipliers like the original AJ equation and contrary to the structure of the MPS model. They concluded that the St. Louis approach is "a severely biased and quite unreliable method of estimating the response of a complex economy to fiscal and monetary policy actions" (p. 42).

To demonstrate the role of omitted variables in the bias in the AJ equation, they remove any correlation between policy instruments and nonpolicy exogenous variables in the structural models by assuming all

nontrended exogenous variables are constant at their means and all trended exogenous variables grow along a constant trend. The predicted value of nominal income for this adjusted structure is computed and used to reestimate the AJ equation. Fiscal multipliers now of appropriate size and magnitude confirm the crucial role of omitted exogenous variables in biasing the estimates of the policy multipliers in the initial AJ equation.

In both papers, Modigliani and Modigliani and Ando (MA) are careful to note that the evidence they present does not permit them either to accept the MPS multipliers or reject the St. Louis ones. But their results should make those who use St. Louis type reduced-form equations uneasy about the validity of the multiplier results, particularly those for fiscal instruments.

While the analysis demonstrates that omitted variable bias may be a source of serious inferential error in the impact of policy actions, the conclusion appears to be nonconstructive in the sense that it does not provide any evidence on the particular source of the bias in the experiments that were conducted and it suggests abandoning the entire approach without attempting to investigate the issue of biases in the St. Louis results directly. It would be useful to identify the sources of bias in the estimated multipliers by introducing the most important exogenous variables directly into the reduced-form equation.

A number of studies have attempted to address the alleged biases in the St. Louis approach directly by including nonpolicy exogenous variables. Gordon (1976), for example, added a "shock proxy," consisting of the sum of net exports, consumer expenditures on automobiles and non-residential fixed investment to the St. Louis specification.

Although monetary multipliers decline and fiscal multipliers increase over his longer sample period, the multiplier results with and without the shock proxy remain qualitatively alike; monetary multipliers are significantly positive while the sum of the lag coefficients on the government expenditure variable is not significantly different from zero.

Recently, Dewald and Marchon (1978) have estimated expanded St. Louis equations for six different countries, including the United States. They included exports as a separate independent variable, dismissing the conglomerate variable constructed by Gordon as including too many endogenous influences. For the United States, the Gordon result is replicated; the impact of monetary policy is reduced, the impact of fiscal policy is left essentially unchanged, and the exports variable has a significant contemporaneous impact. A major monetarist contention is that the influence of a maintained change in the monetary growth rate should be a proportional change in the growth rate of nominal income. This hypothesis is alleged to be a universal phenomenon. However, while Dewald and Marchon cannot reject this hypothesis for the U.S. data, the monetary response for the U.S. is the strongest of any of the six countries investigated. The long-run elasticities of nominal GNP with respect to the money stock in the other five countries never exceed .5. In France they found this elasticity to be only .07 and in two countries (France and the U.K.) this estimated elasticity is not significantly different from zero.

### Resolving the Puzzle: Reduced-Form Versus Structural Model Multipliers

Two further tests by Modigliani (1977) attempt to resolve the puzzle of conflicting multiplier results. First of all, he suggests that despite the apparent large differences in the AC and MPS multipliers, the two sets of multipliers may not be significantly different! To test for significance of the difference in multipliers, Modigliani presumes that the MPS multipliers are the true ones and tests whether the AC multipliers differ significantly from the MPS multipliers. The result is that they are not significantly different at the 5.0 percent level. Modigliani concludes, "This test resolves the puzzle by showing that there is really 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 two estimates is produced by a more reliable and stable method, and is generally more sensible. To me, these criteria call, without question, for adopting the econometric model estimates." (p. 10)

For those who would still opt for the reduced-form multipliers, Modigliani compares the post-sample prediction performance of the AC equation with one in which the coefficients of government expenditures plus exports were constrained to equal those based on multipliers derived from simulations with the MPS models. The post sample simulation begins in 1970II. For the first four years, the MPS based equation dominates: the AC equation yields "distinctly larger" errors in eight quarters, smaller errors in only three quarters, and results in a squared error 1/3 larger than for the MPS based equation. Over the next two years, both equations perform "miserably" but the MPS based equation is still "a bit better."

## Conclusion

The income expenditure counterattack on reduced-forms, particularly the Modigliani-Ando results on the implications of omitted exogenous variables, and the ability to dramatically alter the fiscal policy multipliers by choice of time period and functional form, have substantially weakened the case based on reduced-form equations for small and transitory fiscal effects on nominal income. The implied monetary policy multipliers, on the other hand, have proven robust, at least for the United States.

### ASSESSING THE CUMULATIVE OUTPUT LOSS OF ERADICATING INFLATION

A prominent policy issue of the '70s and one that seems certain to dominate at least the early '80s is the appropriate policy response to a prevailing high rate of inflation. The view that there is a long-run trade-off between inflation and unemployment, widely held at the end of the '60s, is now held by only a small minority. The key issues are the nature of the short-run relation between inflation and unemployment and the process by which economic agents form inflation expectations. Macroeconomic models, both income expenditure and monetarist versions, suggest that while the traditional demand management techniques remain quite capable of reducing the rate of inflation, the cost of such a policy in terms of cumulative output loss would be great. Despite the importance of the issues, there is substantial disagreement about the cost of eradicating inflation and little evidence on the benefits derived as a consequence.

In this section we present evidence on the cumulative output loss associated with reducing inflation based on both estimated Phillips

curves and monetarist models. Then we discuss the most serious limitation of these results -- the failure to allow the results to be influenced by the degree to which the public believes policy authorities are committed to a consistent anti-inflation policy. In the final analysis, the cost of anti-inflation policies in the form of output loss must be balanced against the benefits associated with a reduced rate of inflation. Empirical evidence on the cost of inflation and hence the benefits of reducing inflation is quite limited. Our discussion of the benefits of anti-inflation policies is therefore confined to determining how large the per period gains would have to be in order to justify incurring the cumulative output loss which we calculated from the Phillips curves and monetarist models.

#### Econometric Evidence on the Size of the Cumulative Output Loss

Three alternative sources of evidence on the cumulative output loss associated with the use of demand management policies to moderate inflation are discussed below. The first is evidence directly from estimated Phillips curves. Here we calculate how long unemployment must be increased by either 1 percentage point or 3 percentage points above the rate consistent with steady inflation to reduce inflation by 7.5 percentage points. The second and third sources use monetarist models which include either a Phillips curve or a reduced-form equation relating inflation to monetary change. Here we simulate the effects on inflation and output of a phased deceleration in monetary growth.

#### Results Based on Estimated Phillips Curves

Three recent studies have considered the cost of reducing inflation in the context of traditional Phillips curve regressions (Perry

(1978), Okun (1978), and Cagan (1978)). Perry's results are based on a wage change equation using the inverse of his weighted unemployment rate and lagged wage change estimated using annual observations over the 1954-77 period. His preferred equation yielded a "nonaccelerating inflation rate of employment" (NAIRU) of 4.0 in terms of his weighted unemployment rate (corresponding to about 5.5 percent in the official unemployment rate in '77):

$$(1) \quad \Delta \ln W = -1.88 + 7.44 (1/U_w) + 0.79 \Delta \ln W_{-1} + 0.21 \Delta \ln W_{-2} + 1.07 \text{DNIX}$$

$$\quad \quad \quad (-2.2) \quad (3.5) \quad \quad (4.6) \quad \quad (1.1) \quad \quad (2.9)$$

$$\quad \quad \quad \text{S.E.} = 0.70$$

where W = adjusted hourly earnings in the private nonfarm sector and DNIX is a dummy for the controls equal to -1 in 1972 and 1973 and +1 in 1974 and 1975.

Any unemployment rate in excess of the critical unemployment rate, if maintained long enough, will permit a cycling down of inflation. To compute the cumulative output loss of eradicating inflation, we begin with  $\Delta \ln W$  set equal to 10.0 in the two lagged years and at NAIRU. Our "moderate" policy consists of increasing the weighted unemployment rate 1.0 point above NAIRU in period 1 and holding it here until  $\Delta \ln W$  declines to 2.5, the rate presumed equal to trend growth in labor productivity and, therefore, consistent with price stability. The wage inflation rate falls from 10.0 to 9.6 percent in the first year and declines about 0.3 percentage points per year thereafter taking 23 years to reach a 2.5 percent rate. An alternative "radical" policy is modeled as a 3 percent point increase in unemployment beginning in period one and again sustained until wage change declines to

2.5 percent. This takes only 11 years! Note that the nonlinearity in Perry's wage equation ensures that the cumulative excess of person years of unemployment and, hence, cumulative output loss will be greater in the more radical policy case.

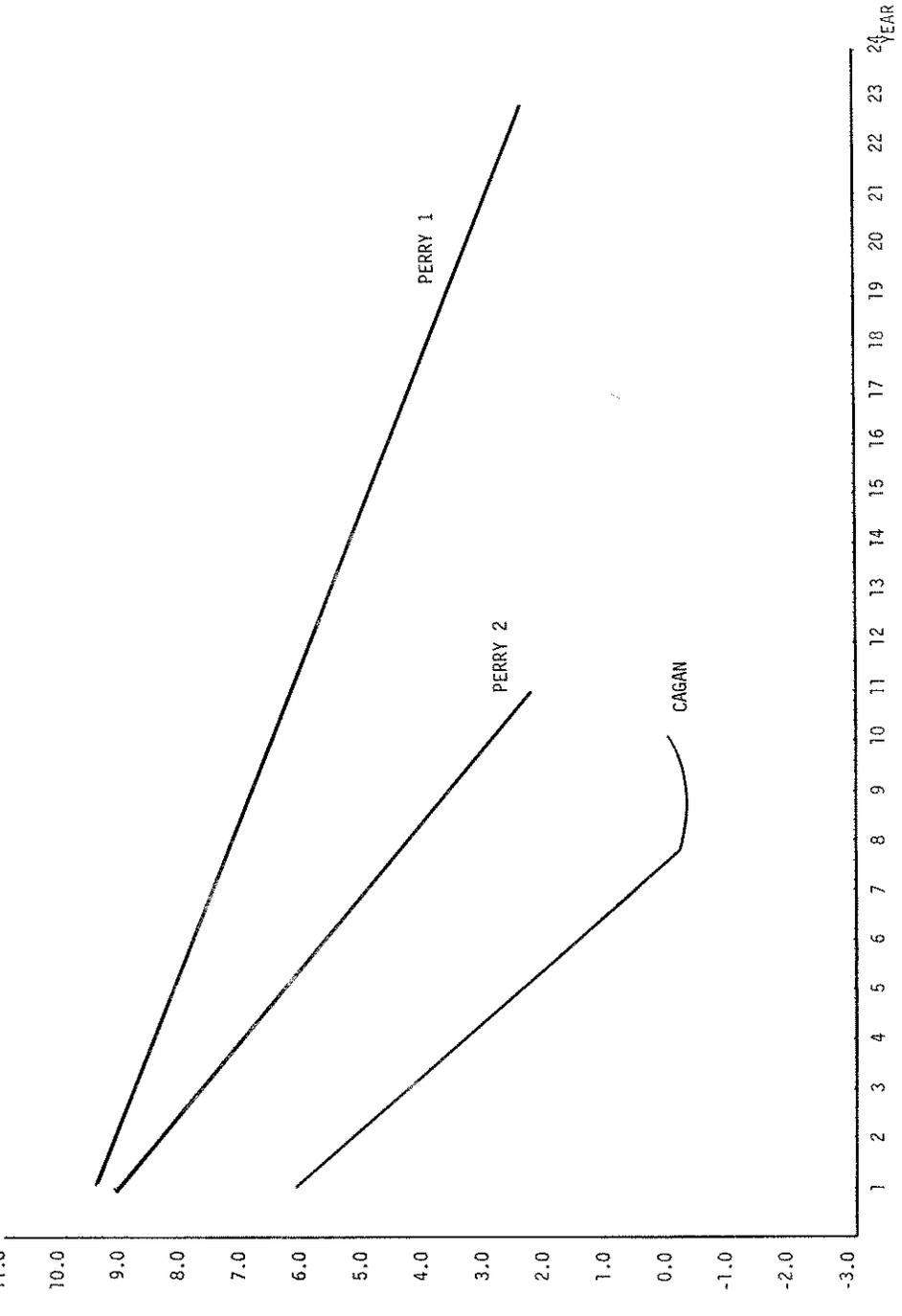
Using Okun's estimate of 3.2 as the impact on output of a 1 percent point increase in unemployment, we can convert the excess unemployment into output loss.<sup>3</sup> One percentage point increase in unemployment reduces output 3.2 percent or \$45.6 billion dollars (calculated at 1978 value for real potential GNP). The 3 percent point increase in unemployment involves an initial year output loss of \$136.7 billion. To find the cumulative, but undiscounted output loss we assume potential output will rise at a 3.3 percent rate. This yields a cumulative loss of \$1532.6 billion for the moderate policy and \$1778.0 billion for the radical policy.<sup>4</sup> The discounted output loss is essentially the product of the initial year loss and the number of years required to complete the program (not accounting for the 3.3 percent rate of growth in potential output is the same as discounting by a 3.3 percent rate); the discounted losses are \$1047.9 billion and \$1503.6 billion in the modest and radical cases, respectively. The results are depicted in Charts 1 and 2. (Perry 1 refers to the moderate case and Perry 2 to the radical case.)

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<sup>3</sup>Estimation of the Okun law relation over more recent data suggests that 3.2 may be an overestimate of the output loss associated with a one percentage point increase in unemployment; the recent estimates are about 2.5.

<sup>4</sup>If the Okun's law coefficient is 2.5 instead of 3.2, these output losses should be reduced by about 20 percent.

CHART 1 MODERATION IN INFLATION VIA MONETARY DECELERATION



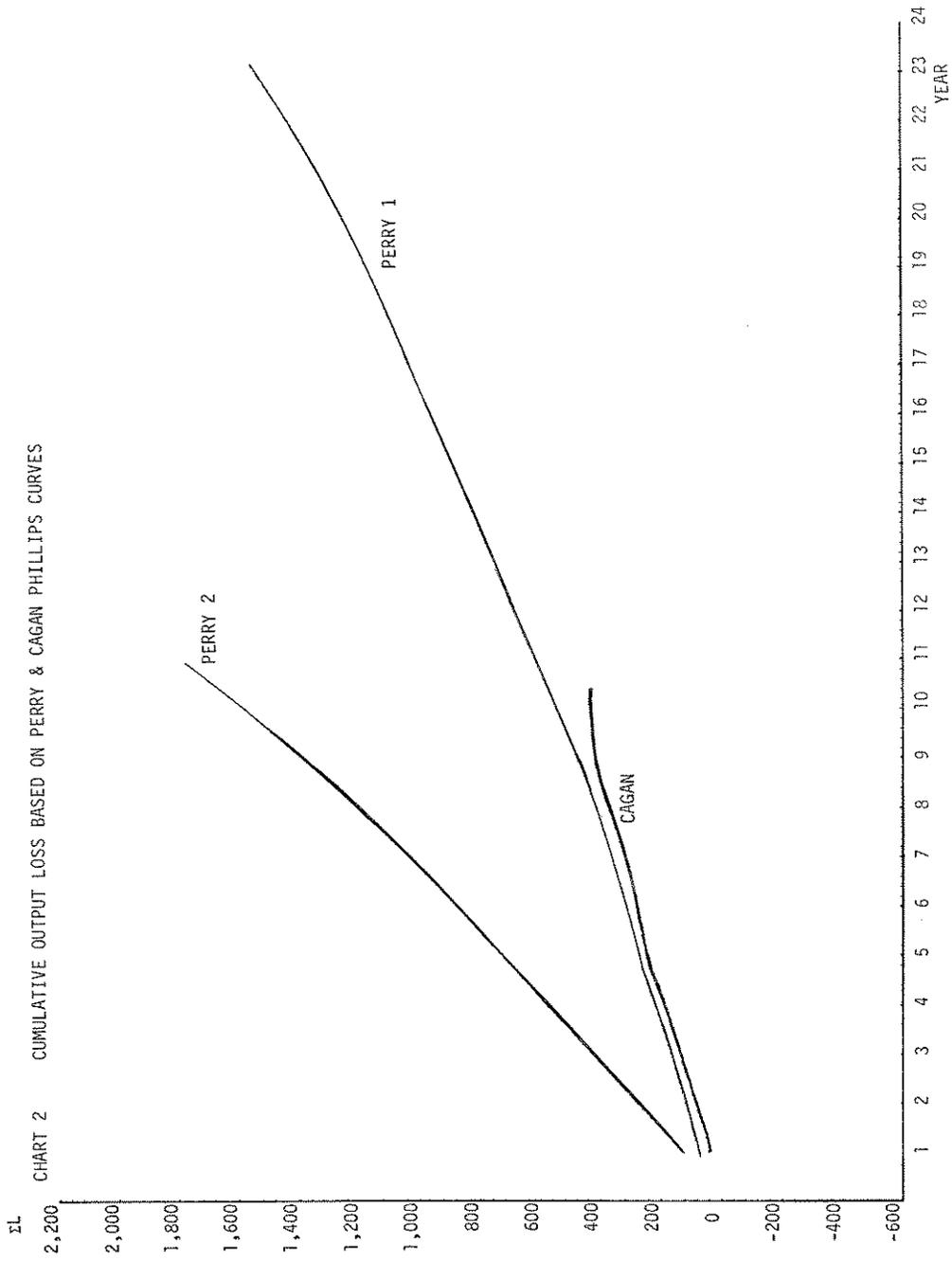
Okun finds that a variety of estimated Phillips curves (PCs) in the literature yield quantitatively similar conclusions. The six equations considered by Okun yield a first year reduction in inflation of from 1/6 to 1/2 percentage point and an average of 0.3 percentage points for a 1 percentage point increase in unemployment. Gramlich (1979) reached a similar conclusion.

There are two aspects of the Perry specification which deserve further discussion: expectations are formed adaptively and the unemployment rate enters nonlinearly. The Phillips curve is uniformly drawn as a nonlinear relation and there have been a number of theoretical explanations (including Lipsey and Tobin) and some empirical support (Perry's influential 1966 study, for example). However, nonlinear and linear specifications seem to do about as well over sample through the mid-1970s.<sup>5</sup> The existence of nonlinearity would provide a rationale for the gradual as opposed to radical policy approach; the greater the nonlinearity, the greater the cumulative output loss under the radical as opposed gradual policy.

The inflation inertia implicit in the Perry equation derives from two sources: actual inflation is built into expected inflation with a lag and actual inflation responds gradually to unemployment in excess of the critical rate. To the extent that the lag in incorporating actual inflation into future wage negotiations is long, indexation might substantially reduce the inflationary inertia. Even with indexation, there would be a lag. Assuming that the full effect occurs

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<sup>5</sup>Cagan (1977) has recently noted the surprising lack of evidence of nonlinearity and this has been confirmed in a careful examination by Papademos (1977).



within the first year would not dramatically reduce the cumulative output costs. The cumulative output loss would decline about 20 percent in each case. Thus, the critical determinant of the gradual decline in inflation is the extremely small per period deceleration in inflation associated with labor market disequilibrium (excess unemployment) in the conventional Phillips curve, not with the slow response of inflation expectations to changes in the actual inflation rate.

Cagan develops a PC equation beginning with the natural rate specification and assuming adaptive expectations Cagan's estimated PC is:

$$(2) \quad \dot{P}_t = \dot{P}_{t-1} - 0.95 \left( \frac{u_t - u_{t-2}}{2} \right) - 0.23 \left( \frac{u_t + u_{t-1} + u_{t-2}}{3} - \bar{u} \right)$$

where  $\dot{P}$  is the quarterly rate of change in the CPI,  $u$  is the unemployment rate for prime age males and  $\bar{u}$  is estimated from the constant of the regression (3.7, for this regression) and the equation is estimated using quarterly observations over the period 1953-1977.

As is clear in Charts 1 and 2, the Cagan equation generates a dramatically more rapid decline in inflation and smaller cumulative output loss. Beginning in period 0 at a 7.5 percent inflation rate (in the current and last period) and at NAIRU, a one percentage point increase in the unemployment rate reduces inflation by the full 7.5 percentage points by the eighth year with cumulative output loss of \$4.2.9 billion, about a quarter of that associated with the Perry and Okun results.

## Evidence Based on the St. Louis Model

To provide additional evidence on the output effects of using stabilization policy to reduce inflation, we ran simulation experiments with the St. Louis model.<sup>6</sup> We begin with a base run in which the rate of monetary growth is at a steady 7.5 percent rate beginning in 1968/III through 1978/IV. This builds in inflation inertia and provides the base against which we can evaluate the effects of gradual monetary deceleration. Beginning in 1973/I we gradually decelerate monetary growth by 1 percentage point in the first quarter of each year. We then compare the policy runs with base run and compute the cumulative output loss associated with the policy.

The first set of simulations with the St. Louis model employ the version of the model estimated over the sample period 1953/I-78/IV. The general practice at the Bank is to employ the estimates of the model using all available data for forecasting and policy simulations. The version estimated through 78/IV, however, has a very large coefficient on the demand slack variable in the model's Phillips curve, almost three times the size of the coefficient estimated with data through 71/II or 75/I, for example. The results are reported in Charts 3 and 4 by the lines labeled StL1. There is a rapid deceleration in inflation and a low cumulative output loss. The inflation rate begins to decline very slowly; it takes two years to reduce the inflation rate by 1 percentage point. Thereafter the deceleration speeds up so that after

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<sup>6</sup>For a description of the St. Louis model, see Andersen and Carlson (1970). The model includes a reduced-form equation for nominal income and a Phillips curve equation for price change; output is then solved for via an identity.

5-1/2 years, inflation has declined by 7.5 percentage points. The unemployment rate rises slowly at first and the maximum increase is only 1.8 percentage points, during the sixth year. The cumulative output loss is only about \$200 billion.

The output loss is, of course, sensitive to the coefficient on the demand variable in the Phillips curve. Using a version of the model estimated through 71/III, where the coefficient on the demand variable is substantially smaller than in the first version discussed, inflation decelerates much more gradually; after six years the inflation rate in the policy run is only four percentage points below that in the base run. At this point unemployment is four percentage points higher than in the base run. The cumulative output loss is \$350 billion at this point and escalating rapidly. These results are depicted in Charts 3 and 4 by the lines labeled StL2.

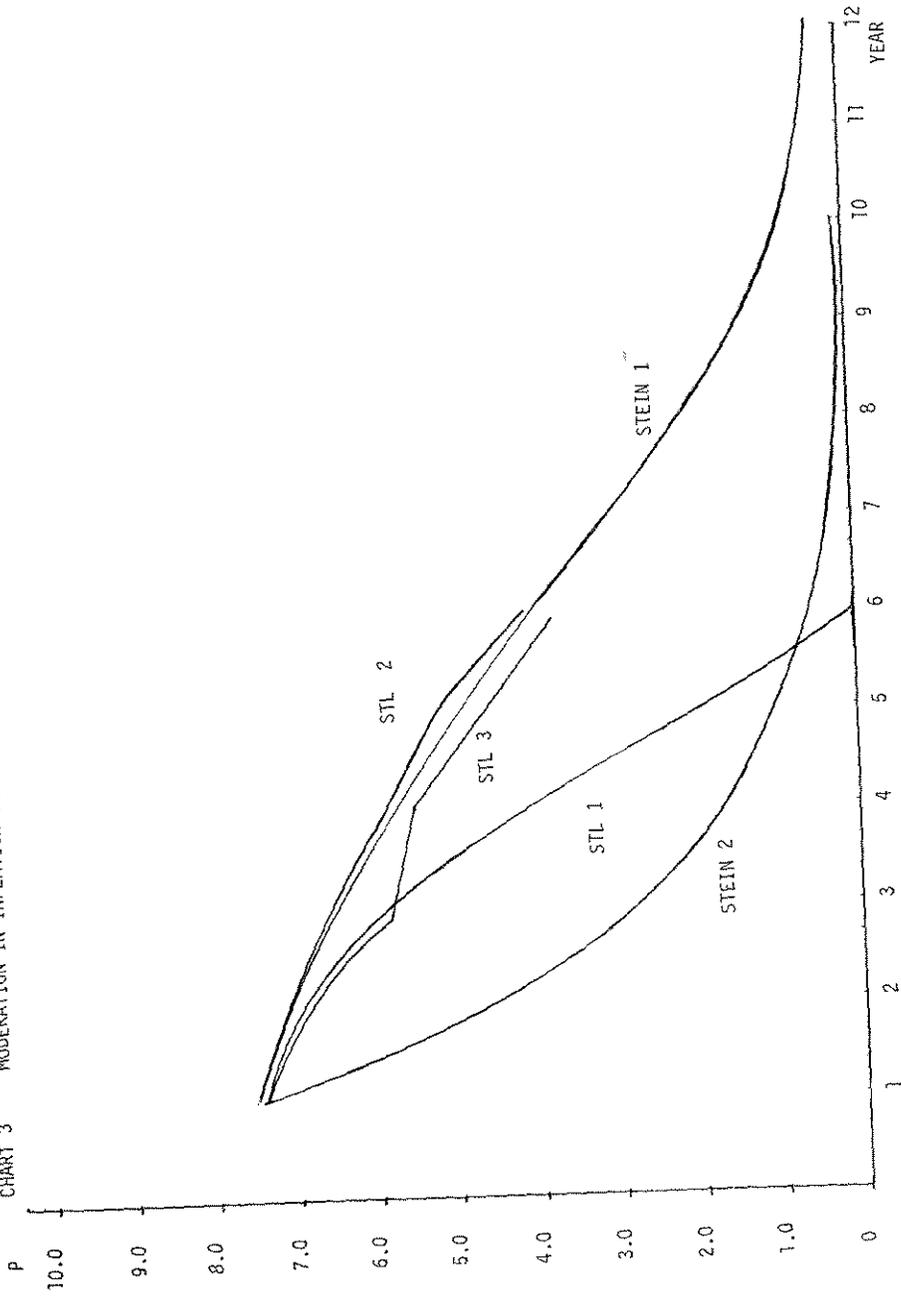
#### Evidence Based on Reduced-Form Equations

Given reasonable doubt about the validity of the Phillips curve,<sup>7</sup> it is useful to consider the implications of reduced-form models that are not tied directly to an explicit Phillips curve. We consider two examples: Stein's (1978) two equation model of inflation and unemployment and AJ type equations for nominal income and inflation. The results are depicted in Charts 3 and 4 by the lines labeled Stein (Stein 1 for the moderate case and Stein 2 for the radical case) and StL3.

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<sup>7</sup>See, for example, Stein (1978).

CHART 3 MODERATION IN INFLATION VIA MONETARY DECELERATION



The Stein model -- In the Stein model, both unemployment and inflation are driven by the rate of monetary growth. Stein's two equation model is:

$$(3) \Delta u(t) = 3 - 0.6 u(t-1) + 0.4 \pi(t-1) - 0.4 \mu_1(t-1)$$

$$(4) \Delta \pi(t) = -0.4 \pi(t-1) + 0.4 \mu_1(t-1)$$

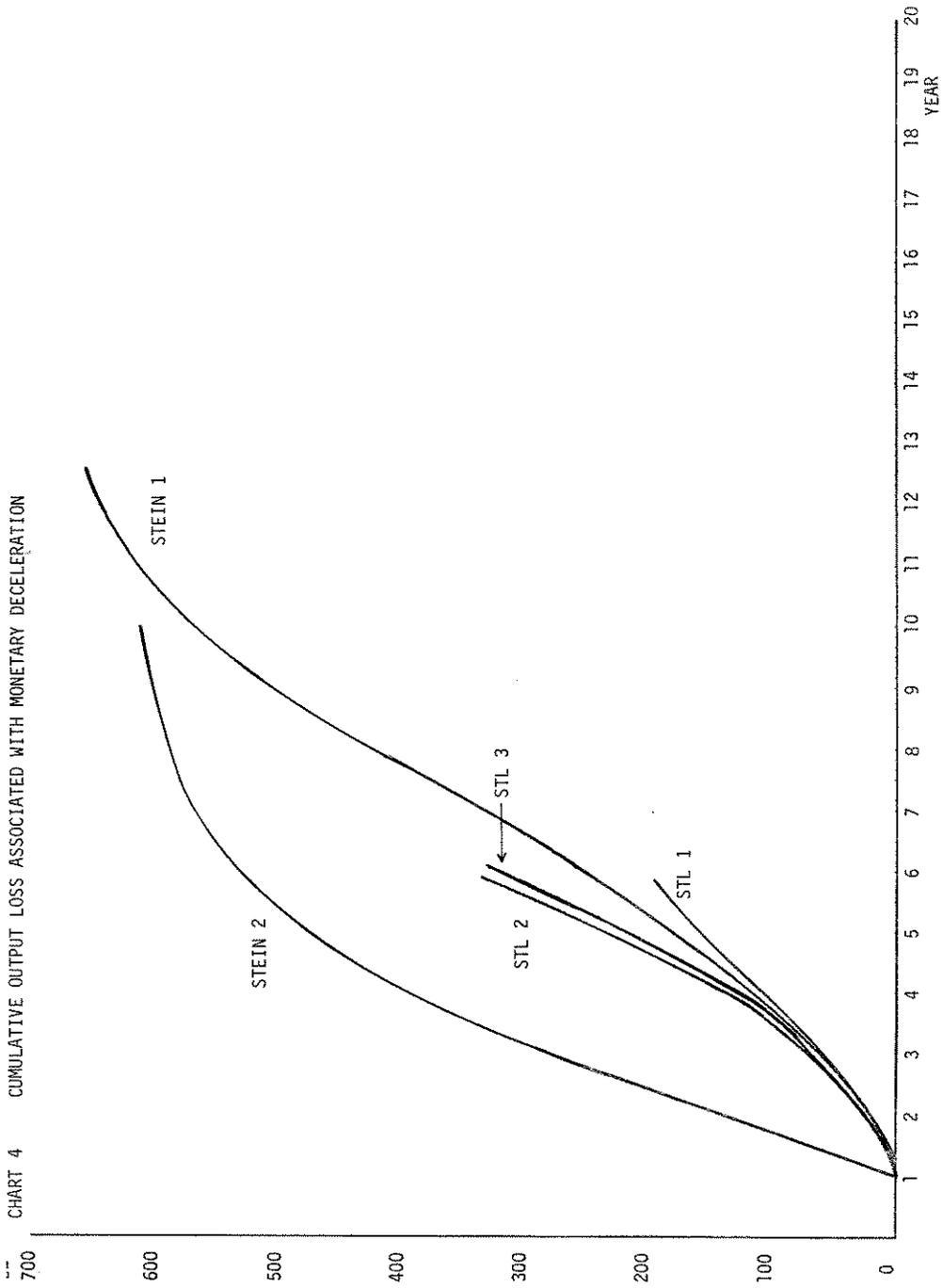
where  $u$  is the unemployment rate,  $\pi$  is the inflation rate and  $\mu_1$  is the rate of monetary growth. The critical unemployment rate is 5.0 and the equilibrium rate of inflation is the rate of monetary growth. Beginning at  $u = 5.0$  and  $\pi(t) = \pi(t-1) = 7.5 = \mu_1(t) = \mu_1(t-1)$ , we decelerate the rate of monetary growth either (a) gradually by 1 percentage point per year until  $\mu_1 = 0$  or (b) immediately to 0. In the gradual policy, unemployment rises beginning in year 2 and peaks in year 8 at 6.6 percent returning to almost 5 percent by year 16. The inflation rate begins to decelerate in year 2 initially at a 0.4 percent point a year rate but ultimately reaches 1.0 point per year by year 7. The inflation rate is down to 2 percent by year 8 and thereafter declines gradually to about zero by year 16. The cumulative output loss is \$687.5 billion. Interestingly, the gradual policy incurs a smaller cumulative output loss, \$613 billion.

The St. Louis reduced-form equation for income with a reduced-form for inflation -- A second simulation based on reduced-form equations combined the reduced-form for nominal income in the St. Louis model with a reduced-form equation for inflation.<sup>8</sup> The inflation

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<sup>8</sup>The reduced-form equation for inflation used in this section was developed by Jack Tatom of the Federal Reserve Bank of St. Louis. An earlier version of this equation was used by Tatom in "Does the Stage of the Business Cycle Affect the Inflation Rate?" Federal Reserve Bank of St. Louis Review, September 1978, pp. 7-15.

CHART 4 CUMULATIVE OUTPUT LOSS ASSOCIATED WITH MONETARY DECELERATION



reduced-form includes a twenty period distributed lag on the rate of change in the money supply and a four quarter distributed lag on the differential in the rate of change in producer prices for energy and the price index for the nonfarm business sector, and two dummies for the effects of the freeze and Phase II and for the subsequent catch up effects. The St. Louis equation yields values for nominal income; the inflation reduced form is employed to generate price level predictions; and the price level is used to deflate nominal income to yield real output predictions. The results in Charts 3 and 4 depicted by the line labeled StL3, reflect the response to the same phased monetary deceleration employed with the other St. Louis model simulations described above.

Note the similarity with the St. Louis results with a Phillips curve (based on the sample period through 71/II), StL2, in Charts 3 and 4. With the reduced-form equation inflation declines more rapidly, by about .20 - .30 percentage points per year over most of the period; correspondingly, the output loss is somewhat smaller. But the time pattern and magnitude of both the deceleration in inflation and the cumulative output loss are remarkably similar. Again note that the output loss per quarter has not peaked after six years of the phased deceleration so that the cumulative output loss is still rising rapidly at the end of six years.

#### Qualifications of the Empirical Analysis

The results reported above are derived both from explicit Phillips curves, and from monetarist reduced-forms. The existence of a cumulative output loss associated with eradicating inflation is

therefore generally consistent with both income-expenditure structural models and monetarist reduced-forms. The major deficiency of the empirical analyses on which the results described above are based is the failure to allow the public's perception of current and future policy to affect expectations about future inflation.

#### The Credibility Effect

The results reported above based on Phillips curves all related inflation in the current period to a distributed lag on past inflation rates where the latter are intended to reflect the rate of inflation expectations (and/or direct the influence of past inflation as for example via catch-up effects). This specification does not allow the degree of credibility associated with announced anti-inflation policies or even the expected influence of recent policy actions to influence inflation expectations. The estimates of cumulative output loss generated by such models are, therefore, almost certain to be overestimates. Fellner (1979), for example, maintains that "... the standard model coefficients... would change significantly for the better -- in the direction of a much more rapid rate of reduction of inflation for any given slack -- if a demand management policy... changed to a credible policy of consistent demand disinflation." But by how much does the standard model overestimate inflationary inertia? By 10 percent, 50 percent?

We do not have any reliable quantitative estimate of the degree to which policymakers can speed the deceleration of inflation by clearly defining their anti-inflation policies and convincing the public that they intend to follow through. Nevertheless, there would

be nearly universal agreement that anti-inflation policies ought to be set out clearly and supported by both the Treasury and the Federal Reserve in such a manner as to maximize the credibility effect.

#### Rational Expectations and the Cumulative Output Loss

In the extreme form of rational expectations models advocated, for example, by Sargent and Wallace (1976), the cumulative output loss associated with a credible policy of monetary deceleration should be zero. These models have two essential features: 1) they are equilibrium models in which prices respond immediately and fully to monetary change and real variables such as unemployment and output respond only to unanticipated inflation; and 2) inflation expectations are formed rationally, taking into account knowledge both about the structure of the economy and the systematic features of policy.

In such a model, inflation should moderate immediately in response to the monetary deceleration, provided, of course, that the policy was announced in advance and believed (or otherwise expected). We had thought of running simulations with an RE version of the St. Louis model along lines suggested by Andersen (1979). On a moment's reflection, the implications were sufficiently obvious that computer simulations could be dispensed with. The St. Louis model has a Phillips curve in which inflation depends on a demand variable ( $x$ ) and expected inflation ( $\dot{P}^e$ ) where the latter is determined from an adaptive expectations model with weights taken from a regression of the nominal interest rate on past inflation rates:

$$(5) \quad \dot{P} = \alpha + \beta x + \epsilon \dot{P}^e$$

Andersen's RE version imposes the condition that  $\dot{P}^e = E(\dot{P})$ ; i.e., that subjective inflation expectations equal the model's forecast for inflation. In this case:

$$(6) \quad E(\dot{P}) = \alpha + \beta x + \varepsilon E(\dot{P})$$

$$(6') \quad E(\dot{P}) = \frac{1}{1-\varepsilon} (\alpha + \beta x)$$

and Andersen substitutes

$$(7) \quad \dot{P} = \frac{1}{1-\varepsilon} (\alpha + \beta x)$$

for the St. Louis Phillips curve.

Andersen sets  $\varepsilon = .86$ , its value in the St. Louis model. However, if  $\varepsilon$  is meaningfully viewed in this case as the coefficient on expected inflation, the value of .86 estimated in the St. Louis model should not be accepted as the magnitude of that parameter in the RE version of the St. Louis model because the value of  $\varepsilon$  was estimated under the assumption that expectations were formed adaptively. Taking  $\varepsilon = 1$ , as seems essential to the RE model, equation 7 no longer is a meaningful equation for  $\dot{P}$ . Instead we obtain from (6) where  $\varepsilon = 1$

$$(6') \quad 0 = \alpha + \beta x$$

so that there is a unique value of  $x^* = -\alpha/\beta$  corresponding, of course, to the natural rate of unemployment.  $x$  can differ from  $x^*$  only on account of random disturbances (with zero mean). In this case any effect of monetary deceleration on the rate of growth of nominal income is transformed immediately and fully into a decline in inflation without any cumulative output loss. This seems to us a more

meaningful RE version of the St. Louis model than that employed by Andersen.<sup>9</sup>

Balancing the Gains from Reducing Inflation Against the Transitional Costs<sup>10</sup>

The cumulative output loss is a measure of the cost of anti-inflation policies. To evaluate the desirability of such policies we also need to assess the gains from reducing inflation. Unfortunately, the costs of inflation (and hence the benefits of reducing inflation) are not as clearcut or easily quantifiable as the cost of unemployment. Fischer and Modigliani (1978) provide a careful outline of the costs of inflation. The costs include the welfare loss associated with the incentive to economize on cash balances, the reduction in capital accumulation due to disincentives for saving and investment that reflect the way in which the tax system permits inflation to affect after-tax

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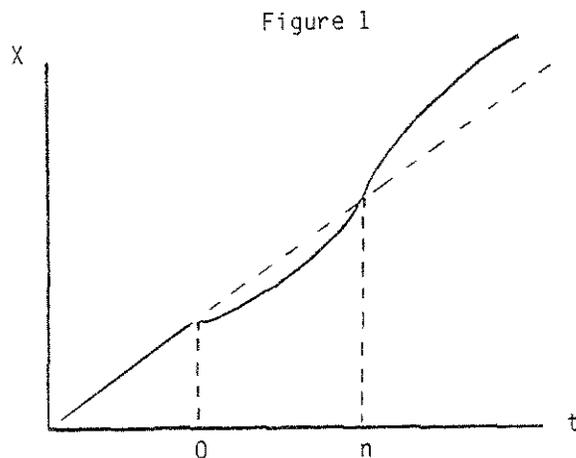
<sup>9</sup>There is a second and related objection to Andersen's approach. In the St. Louis model  $\epsilon$  is not the sum of the coefficients on lagged inflation rates. Indeed the sum of the coefficients is generally about 1.0. The reason for this is that the St. Louis Phillips curve does not estimate the weights on lagged inflation directly within the estimation of the Phillips curve itself. First, an equation for a short-term interest rate is estimated as a function of the rate of monetary growth and distributed lags on both the rate of change in output and on past inflation rates divided by the ratio of unemployment to the full-employment rate. The sum of the coefficients on lagged prices from the interest rate equation in the original Andersen/Carlson article was 1.27 so the sum of weights on lagged inflation rates in the Phillips curve is .86 ( $1.27/(u/u_f)$ ), approximately 1.0. The sum of the inflation coefficients from the interest rate equation vary considerably over different sample periods and the estimate of  $\epsilon$  always compensates to yield a sum on past inflation rates of about 1.0. This reinforces our view that the value of  $\epsilon$  in equation (6) should be taken as 1.0.

<sup>10</sup>This section was added to the original paper and was motivated by comments by Jerry Jordan and Allan Meltzer at the conference.

rates of return and the cost of capital, and the arbitrary redistribution of income and wealth due to unanticipated inflation.

While Fischer and Modigliani do provide estimates of some components of the costs of inflation, neither their study nor others permit us to compute a meaningful estimate of the benefits that would accrue from reducing inflation which could in turn be compared with the cost in terms of cumulative output loss. What we can compute is the minimum size of the permanent gain in output per year due to eradicating inflation which would just justify incurring the cumulative output loss associated with the transition to price stability. We will refer to the benefits as a gain in real output per year. Some components of the gain may, however, be welfare or utility gains that would not necessarily show up in computed measures of real output. While such welfare gains are even more difficult to evaluate than output gains, they are no less important in developing a measure of the benefits of reducing inflation.

Figure 1 depicts the comparison we wish to make. The dashed X line is the rate of growth of (potential) output if inflation remains



indefinitely at 7.5 percent. If anti-inflation policies are pursued, output is assumed to follow the solid line. The transitional costs occur between  $t = 0$  and  $t = n$  as unemployment rises above the rate associated with potential output. However, if there are costs of inflation, output will rise above the level that would have prevailed if the initial steady inflation rate had continued. We define  $G$  as the present value of the permanent per period output gain, evaluated from period  $n$  to  $\infty$ .

$$(8) \quad G = \sum_{i=n}^{\infty} \frac{g_i}{(1+r)^i}$$

This can be compared to the present value of the cumulative output loss ( $L$ )

$$(9) \quad L = \sum_{i=0}^{n-1} \frac{L_i}{(1+r)^i}$$

where  $L_i$  is the output loss in the  $i$ th period ( $i=0, \dots, n-1$ ).

Assuming that the unemployment rate is maintained above the rate consistent with potential output by a fixed amount for  $n$  periods, the loss in period  $i$  can be expressed as

$$(10) \quad L_i = \bar{L} (1+\rho)^i$$

where  $\bar{L}$  is the loss in the first period and  $\rho$  is the rate of growth in potential output. If  $r=\rho$ , the expression for  $L$  simplifies to

$$(10') \quad L = n\bar{L}$$

This is precisely the way we calculated the discounted value of the cumulative output loss above for the Perry and Cagan equations.

To simplify further, we assume  $g_i$  is a constant  $\bar{g}$  for all  $i \geq n$ . We then solve for the value of  $\bar{g}$  which first equates the cost of unemployment and the gain from eradicating inflation -- the minimum value of the permanent per period gain from eradicating inflation that would justify incurring the transitional costs. The value of  $\bar{g}$  for the Perry, Stein, and Cagan results are presented in Table 8; we calculated them under the assumption of a 3.3 percent discount rate and for two

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TABLE 8  
The Minimum Value of the Per Period Gain  
that Justifies Eradicating a 7.5 Percent Inflation Rate

Equation/ Model	Value of $\bar{g}$ (billions of 72 \$)	
	3.2	2.5
Perry 1	73.0	57.0
Perry 2	70.9	55.4
Cagan	16.6	13.0
Stein 1	31.0	24.2
Stein 2	25.4	19.8

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alternative values of the Okun's Law coefficient (3.2 and 2.5, respectively). The minimum value of  $\bar{g}$  varies from \$13 billion per year based on Cagan's Phillips curve to \$73 billion based on the Perry's Phillips curve under a moderate policy.

Note that this analysis provides an alternative perspective on the case for gradualism. Under gradualism, the costs may be reduced if the Phillips curve is nonlinear. But the benefits are also more gradual (in our analysis, postponed until inflation is eradicated). Thus, we find that although the costs are smaller under the gradual policy using the Perry equation (Perry 1), the size of the per period gain

required to justify eradicating inflation is smaller under the more radical policy (Perry 2). The radical policy also yields a smaller minimum per period gain using the Stein model, although this result was expected in this case because the cost turned out to be lower in the radical case using Stein's model.

The calculations reported above presumed that the gains from reducing inflation could be meaningfully represented as a fixed real sum per period. What if the gains are more meaningfully specified as a real sum which grows at the same rate as potential output? For example, the cost of a fully anticipated increase in inflation is generally measured by the reduction in the area under the demand curve for money balances as wealth owners reduce their demand for money in response to the associated rise in nominal interest rates. The decline in demand for real money due to a rise in the interest rate is generally viewed as proportional to the overall scale of money holdings which, in turn, is determined by the level of transactions (e.g. real income). The cost of a given rate of inflation and hence the benefits of eliminating the inflation may therefore grow at the rate of increase of potential output. In this case where  $\bar{g}$  is the value of the gain in period  $n$  (the

$$(8') \quad G = \sum_{i=n}^{\infty} \frac{\bar{g} (1+\rho)^i}{(1+r)^i}$$

first period in which a gain is registered). For  $\rho \geq r$ ,  $G \rightarrow \infty$ . This corresponds to the result recently derived by Feldstein (1979): if the cost of inflation grows at a rate equal to or greater than the discount rate, any positive initial gain (any  $\bar{g} > 0$ ) is sufficient to justify incurring any finite transitional cost!

These results suggest that the case for anti-inflation policies should not be dismissed lightly, even when there are large transitional costs of eradicating inflation. The range of the estimates of the cumulative output loss, the uncertainty about the adjustment in those results required to allow for the credibility effect, and the lack of a quantitative estimate of the cost of inflation makes it extremely difficult to make a meaningful comparison of the costs and benefits of anti-inflation policy. It should not be surprising therefore that policymakers generally seem indecisive and often lacking in commitment to reduce inflation. Narrowing the range of estimates of output loss and developing a measure of the cost of inflation should be high on the priorities for macroeconomic research in the 1980s.

#### RULES VERSUS ACTIVISM

The case against activism rests on two propositions. The first proposition is that the private sector of the economy is inherently stable. This is a major tenet of monetarism and suggests the absence of a need for stabilization policy. Indeed, monetarists generally contend that the instability observed in the economy results mainly from government rather than private sector decisions. The inherent stability of the private sector results in part from the absence of large and persistent exogenous shocks and in part from the fact that the shocks that do occur have relatively small and only temporary effects on output and employment as a consequence of the economy's built-in stability.

The second proposition in the case against activism is that even if the economy were subject to cumulative movements in output, employment and inflation relative to target levels, discretionary policy

might only compound the instability rather than dampen it. The danger that policy will turn out to be destabilizing follows from the long inside lag, the long and variable outside lag, and the general uncertainty about the effect of policy on the economy.

The case for activist policy involves a rejection of the two propositions developed above; the economy needs to and can be stabilized by appropriate manipulation of policy instruments. The first proposition in support of policy activism, then, is that the economy is subject to substantial and persistent disturbances arising from the private sector. In addition, nonmonetarists contend that policy can be implemented with sufficiently short inside lags and with sufficient precision given our understanding of the structure of the economy to yield an improvement in economic performance relative to a policy of a fixed rule.

Relevant empirical evidence on rules versus activism includes:

- (1) the relative size of exogenous impulses arising from policy and nonpolicy sources
- (2) the degree of persistence in the response to such disturbances
- (3) the ability of active policy to improve economic performance in the face of the disturbances.

#### Stability of the Private Sector

The issue of the stability of the private sector has been categorized as a fundamental difference between monetarists and the conventional Keynesian tenets (See Andersen (1973) and Mayer (1975)). Nevertheless, it appears to be an issue on which little, if any, relevant empirical evidence is available.

The evidence that is conventionally cited in response to the allegation that the Keynesian position regards the private sector as

inherently unstable is the result of simulation experiments with various econometric models. These experiments suggest that the models are stable, usually exhibiting highly damped oscillations back to equilibrium following some shock (see Klein (1973)). Such results under the postulated experimental conditions are probably a necessary condition, but not a sufficient condition to substantiate the monetarist proposition. We would need to look at the degree of damping under a policy of fixed rules relative to the damping under an endogenous policy with feedback from current economic developments. The case for rules is enhanced if endogenous policy reduces the degree to which disturbances are damped.

#### Evidence from Model Simulations

Discussions of the effectiveness of policies often focus on the size of policy multipliers. Such measures of the leverage of policy on goal variables are critical to setting policy, but do not provide any evidence on the usefulness of discretionary policy unless they are zero. Indeed as Cooper and Fischer demonstrate, even if the policy instrument has a zero cumulative multiplier it may be useful as a stabilization tool as long as it has a nonzero short-run multiplier. More important is the predictability of the outcome of policy actions which is more closely related to the errors in forecasting the goal variables. The case for discretion, therefore, has little or nothing to do with the size of policy multipliers, unless there is some concern about moving the policy variables too far or too fast such as when a "penalty function" is added to the "goal function." The time pattern of the response as well as the predictability of the policy multipliers, on

the other hand, do matter. Evidence on rules versus discretion, therefore, generally involve model simulations and these are most useful if allowance is made for uncertainty about the multipliers.

Below we review the evidence on the comparison of economic performance under rules and discretion based on simulations with macro-economic models. First we must define a set of alternative policies; four alternatives have been investigated.

1) Actual policy: Historical simulations in which policy instruments take on their historical values provide the benchmark of actual policy, discretion as it was implemented as opposed to what would have been optimal in the context of the model under consideration.

2) Fixed rules or rules without feedback: Simulations in which the policy instrument is constrained to grow at a constant rate provide evidence on the effect of fixed rules; for example, a constant rate of monetary growth as advocated by Friedman. In this case the policy instrument is totally independent of current economic developments.

3) Active rules or rules with feedback: An alternative to both discretion and fixed rules is an active rule or a rule which requires policy instruments to respond systematically to current economic developments. This approach introduces Phillips type ad hoc rules involving proportional and derivative controls. Some experimentation is undertaken to identify "good" rules but short of full optimization. Such simulations can be viewed as a way of modeling systematic discretionary policy without the blatant policy errors that in retrospect always mar the historical runs.

4) Optimal control: The benchmark for identifying the best that is possible under discretionary policy is an optimal control simulation in

which policymakers are viewed as selecting a time path for their instruments that minimizes the losses associated with deviations of their goal variables from their target levels. It, therefore, requires imposing an explicit loss function including the designation of relative weights on competing objectives and solving the model subject to minimization of the losses. The solution allows the selection of an instrument path to reflect knowledge of the structural parameters of the model and forecasts of future performance based on current and past values of exogenous variables and the dynamic structure of the model. A superior economic performance under such circumstances hardly provides convincing support for discretionary policy, although it provides evidence of the potential for discretionary policy to improve economic performance.

The various policy regimes can be simulated in a number of different ways. In a deterministic simulation the error terms in the various estimated equations are set to zero. This immediately removes a potentially important source of instability in the private economy and should be expected to bias results in favor of fixed rules. There are two basic types of stochastic simulations reflecting the two sources of random disturbances: the additive error terms in the estimated equations and the estimated coefficients. Simulations allowing for random additive error disturbances are generally labeled stochastic simulations while those that randomize both parameters and additive errors are referred to as fully stochastic simulations.

#### Actual Policy Versus Fixed Monetary Growth Rules

Modigliani reports two simulations with a fixed monetary growth rule over the period beginning in 1959 and ending in mid-1971. In each case M1 is constrained to grow at a 3 percent annual rate. In the first simulation all shocks are eliminated by substituting constant trends or means for untrended exogenous variables. In the second, historical values of exogenous variables are employed. In the first case the monetary rule stabilizes the economy, but, allowing for historical shocks the economy "was distinctly less stable than actual experience, by a factor of 50 percent [p. 12]."

Eckstein investigates the implications of smooth growth in non-borrowed reserves over the period of 1964 through 1975. (Nonborrowed reserves grow at a 4 percent rate in '64, accelerate 1/4 percent point each year until they stabilize at a 6 percent rate during and after 1972). Eckstein finds that smooth growth in reserves does result in "a more stable growth pattern" but does not dramatically alter the overall results for economic performance.

#### Active Rules Versus Fixed Rules

In a series of papers employing simulations with both the MPS and St. Louis models, Cooper and Fischer (1972a, 1972b, 1974) compare Phillips type feedback control rules with fixed growth rate rules. They conclude that there are active rules which dominate fixed rules for both models, under deterministic, stochastic and fully stochastic simulations. The dominant active rules generally involving strong derivative controls and some proportional control. The criterion was the average standard deviation in the unemployment and inflation rates.

For the St. Louis model, for example, the average standard deviations for each variable were reduced by about 20 percent in the deterministic simulations (over the period 56/I-68/IV), between 50 - 70 percent in the stochastic simulations (over the same period) and by about 50 percent in the fully stochastic simulations (over the period 55/I - 71/IV). The improvement was more modest, however, in the MPS model, where the standard deviation of unemployment fell by 4 - 24 percent and that of inflation by 7 - 32 percent in stochastic simulations over the period 1956/I - 68/IV.

#### Optimal Control Simulations

There have been numerous attempts to compare fixed rules with optimal control simulations including Chow (1972), Garbade (1975), Cooper and Fischer (1975), Crane, Havenner and Tinsley (1976), and Crane, Havenner and Berry (1978). The first four studies find that fixed rules are uniformly inferior to optimal control (and generally inferior to historical policies). These studies use stochastic simulations but actual values of exogenous variables and, with the exception of Cooper and Fischer, constant parameter values. Garbade for example finds that "discretion," in the form of optimal control, reduces the expected loss by 50 percent compared to a fixed rule, a result in close agreement with Chow. Garbade views his results as adding to the "accumulating evidence" of the gains associated with discretion "when a valid representation of the economy is available." But that, after all, is the major element in the controversy.

Cooper and Fischer find that their active rules perform quite well in relation to optimal control solutions using the St. Louis model.

Costs are reduced by about 45 percent relative to fixed rules, but fixed rules outperform historical policy in this case due in part to greater instability in instrument movements in the latter case. The Cooper-Fischer paper produces a possibly valuable insight about the relative performance of rules and discretion. Stochastic simulation requires multiple simulations for alternative realizations of the stochastic disturbances. They found that the poor overall performance of fixed rules resulted from their "spectacularly bad" performance in replications where losses turned out to be above average for all policies. Where average performance is good, on the other hand, fixed rules perform about as well as optimal control. This may imply that optimal policy is nonlinear-restrained to fixed rules within a band around target values of goal variables and active only outside those bands. Thus, "fine tuning" is rejected, but activism in the face of a major disturbance has a substantial payoff.

This conclusion is reinforced by the Crane, Havenner and Tinsley study of the 1971/I-1974/II period using a condensed version of the MPS model, MINNIE. Optimal policy is not especially volatile after an initial aggressive expansionary policy in the first two quarters to offset the recession implicit in the initial conditions. The optimal policy again dominates fixed rules, in this case by about 40 percent; and fixed rules would have increased expected losses by about 45 percent relative to historical policies.

#### Rational Expectations and the Limits of Activist Policy

The traditional arguments against activist policy focused on the implications of long inside lags, long and variable outside lags, and

multiplier uncertainty; there was a general emphasis on the limitations of policy in an environment characterized by insufficient knowledge of the economy's structure. The Lucas-Sargent-Wallace rational expectations models suggest a dramatically different basis for fixed rules. These models suggest that policy is doomed to ineffectiveness in an environment in which economic agents have knowledge both about the structure of the economy and the way in which policy authorities respond to economic developments. In this case too much knowledge rather than too little knowledge underlies the ineffectiveness of policy. Real variables according to these models respond only to unanticipated price or inflation shocks. Systematic policy, by definition, cannot produce surprises. Therefore, although there exists a trade-off between unanticipated inflation and unemployment, it cannot be systematically exploited by policy authorities; this is generally referred to as the neutrality proposition. The theoretical structure of these models and the implications of a number of qualifications, particularly the existence of nominal contracts, have been thoroughly developed in the paper by Taylor. The role, operational specification, and implications of rational expectations in macroeconomic models is the central issue in macroeconomic theory today and empirical investigations of these models is certain to be the growth industry of the '80s. There are, however, only a handful of empirical studies to date that attempt to test the neutrality proposition.

McCallum (1979) in a recent survey of this literature notes that while "the formal evidence is not inconsistent with the neutrality proposition. . . the power of existing tests is not high and, in any event, the evidence is not entirely clearcut." The two most important

empirical studies are the Barro papers (1977, 1978) on the effect of unanticipated monetary growth on unemployment and output and Sargent's paper (1976) applying Sims and Granger tests for causality to movements in the unemployment rate, the money supply, government expenditures and other macro variables.

Barro estimates a reaction function to isolate unanticipated monetary growth and then examines the role of unanticipated and anticipated monetary change on unemployment and output. His results are remarkably one sided, supporting the hypothesis that only unanticipated policy actions affect real variables. But his empirical methodology has been convincingly critiqued by Small, Fischer (1978) and Gordon (1979). Sargent is somewhat more cautious in interpreting his findings as indicating that "the causal structure imposed on the data by the classical model. . . is not obscenely at variance with the data [p. 233]." We think this means the results are mixed, which indeed they are. There is some evidence, for example, that movements in the money supply "cause" movements in the unemployment rate (using the Granger test) and some evidence that it does not (using the Sims test).

#### Summary

The evidence accumulated over the '70s has at best only a modest role in increasing the consensus over the gains associated with activist policy. The experience of the '70s has clearly eroded the optimism about the potential activist policy that characterized the apparent success of the 1964 tax cut and the long expansion of the '60s. There is wider recognition today compared to the mid-1960s among proponents of active policy of the limitations of active policy and the

difficulty of "fine tuning" the economy by responding to even small departures of output and employment from target levels. Active policy, however, continues to have wide support in situations where a sizable displacement has occurred, as in the 1973-75 recession. On the other hand, many proponents of rules, such as Friedman (1968), also allow for the use of discretionary policy to offset "major disturbances [p. 14]." Therefore, the gulf between proponents of rules and activism is not nearly so great as it might at first appear. The optimal control studies have helped to emphasize the potential usefulness of aggressive policy action when initial conditions are far away from targets and the limited potential usefulness of activist policy in response to smaller displacements. This lesson is perhaps one on which proponents of rules and activism can agree.

#### CONCLUSION

As the '70s began, the monetarist-income expenditure controversy was a dominant theme in macroeconomics. Particularly after the MPS and other large scale models began churning out large values for monetary policy multipliers, the controversy focused in on the size of fiscal multipliers, particularly the fiscal multipliers on nominal GNP. The econometric evidence of the '70s has not fully resolved this issue, i.e., there are those who continue to be persuaded by the St. Louis equation results. And while this evidence questioning the reliability of the fiscal multipliers in the St. Louis equation undoubtedly has reinforced the views of the skeptics, it has not necessarily shaken the confidence of the equation's supporters.

As the '70s began, the orthodoxy of a Phillips curve embodying a stable trade-off was under an attack it did not survive. After a transitional period, evidence mounted in support of a vertical long-run Phillips curve. Thereafter, the issues contested have been the nature and sources of any short-run trade-off and the implications for the output loss of eradicating inflation. The econometric evidence from a wide range of sources and models suggests that monetary deceleration can eradicate inflation, but not quickly and not without large costs in terms of cumulative output loss. The major unresolved issue is the significance of the credibility effect and the degree of overestimation in the cumulative output loss due to the failure to take into account the effect of recent policy actions and expected policy actions on inflation expectations.

While fine-tuning may have few advocates, the evidence from model simulations suggests there are likely to be considerable gains to activism when the economy is far away from targets and in response to very large shocks. Rules or activism remains an important issue although the case against activism has been broadened by the development of rational expectations market clearing models.

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## DISCUSSION OF THE MEYER-RASCHE AND TAYLOR PAPERS

Neil Wallace

For us at this conference, the 1970s constitute ten years of additional data and some theoretical developments that suggest new ways of interpreting those and earlier data. The two papers presented this morning -- in part, because of the assignments given the authors -- contain very different views about the lessons of the 1970s. I will come to still a third view and, as it happens, one that does not represent a compromise between them.

As I understand it, Meyer-Rasche accepted the task of summarizing lessons from the data of the 1970s, while Taylor accepted the task of summarizing lessons from the theoretical developments of the 1970s. That division of labor did not turn out well; it encouraged Meyer-Rasche to proceed as if one could learn lessons from data without invoking theory.

On the basis of the preliminary draft of the Meyer-Rasche paper made available to me and on the basis of their oral remarks this morning, I am left somewhat in the dark about the point of view of the Meyer-Rasche paper. I know what they did, but I'm not sure what their message is.

Based on what they did, one might infer that for Meyer-Rasche, the 1970s represent no more than ten years of additional data. They

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use those data and earlier data in the same way that most economists ten years ago used the data available to them. In particular, both their so-called structural models and their reduced-form models consist of regression equations that in form are the same as those most economists used in the 1960s. Moreover, Meyer-Rasche extrapolate from those regression equations for the effects of different policies in the same way that many economists in the 1960s extrapolated from their estimates. That is why I say that for Meyer-Rasche, the 1970s seem to represent no more than ten years of additional data.

Even at the level of pure empiricism, a different lesson can be drawn. The Meyer-Rasche extrapolation procedure applied in the late 1960s did badly predicting the 1970s. Why, then, believe that those same procedures applied now will do well predicting the 1980s?

Happily, though, we do not have to decide on the basis of pure empiricism. The theoretical developments of the 1970s -- many of which are described in Taylor's paper -- provide convincing arguments why we should not take seriously as "multipliers" the correlation coefficients or the functions of them presented in the Meyer-Rasche paper.

Meyer-Rasche are aware of the criticism of the multiplier interpretation of their estimates. In effect, they acknowledge the criticism and say that they are unwilling to defend such an interpretation. That, though, is what leaves me confused about their message. Nor does it help to suggest, as Meyer seemed to in his oral remarks, that their estimates of Phillips curve trade-offs provide upper bounds on the unfavorableness of this trade-off. Logically, such a claim also requires a supporting argument. Moreover, upper bounds can be interesting, or not interesting. All of GNP is an upper bound on the output

loss that accompanies a one percent cut in the inflation rate, but it is not an interesting upper bound. Meyer-Rasche must convince us that their estimates are interesting upper bounds if, in fact, they are upper bounds at all. Such convincing must take the form of a theoretical argument that says why it is legitimate to extrapolate in particular ways from particular correlations.

In the 1960s, many economists thought that their policy extrapolations from the kinds of models used by Meyer-Rasche were legitimized by existing theory. The theoretical developments of the 1970s have convinced many of us that that is not so. Although Taylor's paper describes some of those developments, his paper stops short of describing in full generality why we were led astray badly by the kind of theorizing that was used. Since that kind of theorizing still persists, it is worthwhile summarizing in a general way what is wrong with it.

Whether we are talking about most textbooks in macroeconomics or most macroeconometric models, the models from which policy implications are drawn consist of a set of relationships -- a consumption function, an investment function, a money demand function, and so on. Let us label these  $M_1, M_2, M_3, \dots, M_N$  (M for model). The style of macroeconomics textbooks is to present the complete model and its policy implications and also to present separate chapters -- one on consumption, one on investment, one on money demand, and so on -- that are meant to justify one by one the relationships of the complete model, the  $M_1$ . When builders of macroeconometric models try to justify their models, they also proceed in this way. In order to get at what is wrong with this kind of theorizing, we must describe the logical relationship

between these justifying chapters and the macroeconomic or macroeconomic model consisting of  $M_1, M_2, \dots, M_N$ .

Each justifying chapter consists of a set of assumptions. Let us label these sets of assumptions  $S_1, S_2, \dots, S_N$  (S for story), where for each  $i$ ,  $S_i$  is said to justify  $M_i$ . The most extravagant claim made about the relationship between  $S_i$  and  $M_i$  is the following: For each  $i$ ,  $S_i$  implies  $M_i$ . In particular, it is never claimed that the converse is also true. In other words, in general,  $S_i$  and  $M_i$  are not equivalent and more is implied by  $S_i$  than just  $M_i$ . This nonequivalence has two consequences.

First, it implies that consistency among the  $M_i$  does not imply consistency among the  $S_i$ . If the  $S_i$  are mutually inconsistent, then it cannot be claimed that there is an underlying theory of the  $M_i$ . Note, in this regard, that consistency among the  $S_i$  is never checked and, as I illustrate below, that inconsistency is easy to demonstrate for most macroeconomic models.

Second, if the  $S_i$  are mutually consistent, nonequivalence between  $S_i$  and  $M_i$  implies that we are missing many of the implications of the underlying theory by limiting attention to the  $M_i$ . Thus, for example, the  $S_i$  often contain at least hints of a welfare analysis of inflation. As is well known, the typical  $M_i$  provide no such analysis.

I will now briefly defend the nonequivalence claim and, at the same time, argue that inconsistencies are present in standard macro models. And, since this is St. Louis, I will begin by focusing on money demand.

The usual way to defend the money demand functions of most macroeconomic models is to appeal to a transaction cost model of the Baumol

(1952), Tobin (1956), or Miller-Orr (1966) variety. Those models explain money demand in the presence of default-free, higher-yielding securities -- Treasury bills, say -- by transactions costs, for example, trips to the bank. But the models imply more than a money demand function. They imply that if the ratio of the public's means of payments to its holdings of interest-bearing assets changes as a result, say, of open-market operations, then there is a change in the amount of resources used up in transactions. But such a change contradicts the usual resource-supply assumptions of most macro models. Those make no allowance for an altered amount of resources being used up in transactions. For this and other reasons, the implications for open-market operations of the theory of interest in the inventory models are very different from those of most macro models, particularly monetarist models (see Bryant and Wallace 1979).

It is also standard to assume that the money demand function that one derives for a closed economy holds with only minor modifications for an open economy in a world in which each of several countries issues its own money. It is this view that lies behind the attachment to (the viability of) laissez-faire floating exchange rates. But such a claim is supported neither by an acceptable theory (see Wallace 1979), nor by recent experience. That experience suggests that the demand for a particular money in a world of many monies may be very different from the demand for a single money in a closed economy.

In the 1970s, of course, inconsistencies regarding expectation formation have received the most attention. Expectation formation is important because macroeconomics is concerned primarily with aspects of behavior that depend upon views about the future -- asset acquisition

versus current consumption, the composition of assets, or nominal wage determination in those contracts that Taylor discusses at length in his paper. It has been argued convincingly that the  $M_i$  of most macroeconomic models contain, either implicitly or explicitly, forecasting schemes that are good schemes in some environments and not in others. (See, for example, Lucas 1976.) Moreover, careful examination of the  $S_i$  reveals that the particular forecasting schemes imbedded in the  $M_i$  were chosen because they were good schemes in particular environments. The inconsistency arises because the environment implied by all the  $M_i$  -- including various specifications for policy -- may not correspond at all to that assumed in the various  $S_i$ . This kind of inconsistency is avoided by using a perfect foresight (rational expectations) equilibrium concept. By using that concept, the economist avoids imposing on the individuals whose behavior is being modeled any fixed way of extrapolating from the past, and ensures that he or she is not attributing to them views about the future that make no sense for the environment they are in.

Now having said that perfect foresight is an equilibrium concept, it should be evident that it is misleading to discuss its merits or its implications in terms of a particular policy conclusion like "policy (whatever that means) does not matter." The perfect foresight equilibrium concept has been around for a long time. It would be surprising, indeed, if that concept alone implied a result like "policy doesn't matter." In general, of course, by themselves equilibrium concepts imply very little. The importance of the perfect foresight equilibrium concept has nothing to do with the validity of some vague conclusion

like "policy does not matter." Why, then, all the attention to "policy doesn't matter" in this morning's papers?

In 1975, there appeared a paper by Tom Sargent and me in which a result of that sort was obtained. We took a particular  $M_1, M_2, \dots, M_N$ , one that we argued resembled in many respects standard macro models, and replaced a fixed forecasting scheme, one of the  $M_i$ , by perfect foresight. We argued that the replacement made a great difference for the implications of the model. In particular, under perfect foresight and certain other assumptions, all policies in a certain class gave rise to the same equilibrium values for real variables. This result did not follow under the fixed forecasting scheme. Our message was, therefore, that the kind of forecasting scheme imposed matters greatly. Such a message, though, is very different from one that says that the perfect foresight version should be taken seriously as a model of this or any other economy. From the discussion above -- and from remarks in our 1975 paper -- it should be evident that the imposition of a perfect foresight equilibrium concept does not by itself turn a hodgepodge of indefensible relationships into a coherent model.

The Sargent-Wallace "policy-doesn't-matter" result is to be contrasted with a neutrality result obtained by Lucas (1972). The Lucas result was obtained from a model that is coherent in the sense that its conclusions are derived from a mutually consistent (and defensible) set of assumptions, a single  $S$ . The Lucas neutrality result, however, applies only to alternative deficits consisting of money transfers that individuals know they will receive in proportion to their holdings of money. This is neither monetary policy in the sense of open market operations -- there is, in fact, only one asset in the Lucas

model -- nor is it the kind of fiscal policy that any country ever follows. The Lucas model is important because it is the first coherent model that implies anything like Phillips curve correlations. The model implies that it is not legitimate to extrapolate from these correlations for the effects of different policies.

What is new about the 1970s and what offers bright prospects for the 1980s is not so much the view I have set out about the illogical structure of standard macroeconomics. That view can, I think, be found in Leontief (1947) and Koopmans (1947) and, I might add, in the attitude of many nonmacroeconomists toward macroeconomics. What is new and exciting about the 1970s is the progress we have made in devising defensible assumptions that can explain a wide range of macroeconomic phenomena. Lucas (1972) is an outstanding example. In the work on search and matching models (see, in particular, Mortensen 1979), we see the beginnings of a theory of unemployed resources. And, perhaps, in new work on money (see, for example, Kareken and Wallace 1979), there are ideas about how to confront long-standing problems in monetary theory. Although I think we are making rapid progress, the profession is very far from having reached a consensus.

First, not everyone, by any means, agrees that we must completely abandon the style of macroeconomic theorizing and modeling that I have described above. For many, to do that is to abandon macroeconomics. This is right if macroeconomics is defined by a style of modeling. But if, instead, macroeconomics is defined by the phenomena it seeks to explain and by the policies it seeks to analyze, then this is not a call for abandoning macroeconomics. It is a call for abandoning a fallacious style of reasoning that has evidently gotten us nowhere. Second, even

among those who agree that we must, as it were, start over in macroeconomics and monetary theory, there is little agreement about how to proceed. For example, in my very brief listing of promising developments, I did not include disequilibrium theory. In my view, disequilibrium theory is not very promising, but many economists disagree.

Given the lack of consensus on theory, it would be surprising if there were consensus on policy. And there is not. Academics, of course, thrive on controversy, which very naturally accompanies the development of substantially new theories in a field. Policymakers, in contrast, seek consensus. Since the economics profession is far from having reached consensus on macroeconomic policy, I do not envy the task of policymakers in the 1980s. The absence of professional consensus leaves policymakers in the position of having to make up their own minds.

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## DISCUSSION OF THE TAYLOR PAPER

Hyman Minsky

For this conference John B. Taylor has prepared a survey paper titled "Recent Developments in the Theory of Stabilization Policy." Such a survey is useful as it develops the critical issues in the field, indicates what progress has been made, defines the questions on the research frontier and serves as guide through an important literature. Its usefulness depends upon the competence, taste and vision of the author.

John B. Taylor holds a position and has the credentials that bespeak of competence. The paper before us is an academic exercise that illustrates the author's command over a literature which is sometimes technically demanding. The paper also shows that he is able to ignore the developments in economic theory and the economy which are especially relevant for stabilization policy and the theory thereof. Hence in reading Taylor's paper I was led to question the taste and vision that guides him and the literature he surveys.

The theory of stabilization policy is important only as it serves as a guide to action in an unstable world. The topics and the literature that Taylor has chosen to cover are not useful to anyone seriously involved in stabilization policy; one cannot derive any guide for action with respect to the serious issues of stabilization policy from

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this survey or from the underlying papers. Therefore the paper serves no useful purpose aside from being a showcase for Taylor's talents. In a similar vein, the underlying literature may be best interpreted as the products of a game played for academic advancement.

In selecting what to discuss Taylor ignores the literature which quite clearly demonstrates that neo-classical aggregate economics, which focuses on price or wage rigidities and which introduces money as an exogenous variable, will not do. The literature he focuses on looks to refining and making more precise the very neo-classical formulations whose logical consistency and empirical relevance has been demolished by developments in theory in recent years. However one rule of the game Taylor and the authors of the reviewed literature play is that research is to be carried on "within" the neo-classical model; thus taste and vision conspire to rule out the relevant and the serious because it is unorthodox.

The most important developments for the theory of stabilization policy during the late 1960s and 1970s were not in the literature but in the "world." The observations that theory has to explain and the developments in the economy that stabilization policy has to contend with changed radically in the mid 1960s. In particular, stabilization policy now has to deal with threats and partial realizations of financial instability as well as with stepwise increasing unemployment and a stepwise acceleration of inflation.

For all who take "our economy" rather than the literature about economics derived from the neo-classical (monetarist and pseudo-Keynesian) research program as the subject matter of their research, the world underwent a marked change of state around 1965. The

instability that policymakers have to contend with after 1965 is of a different order of magnitude and the potential consequences of mismanagement of stabilization policy are much more serious than earlier in the post-war period. The financial system and practices evolved from 1945 to 1965 so that the system, which had been virtually impervious to financial instability, became highly susceptible. Between 1945 and 1965 there were no threats of a financial crisis of the scale which could usher in a deep depression; in the years that have followed there have been at least three such threats within the United States, as well as a number of threats to the stability of the international financial system. Whenever financial instability threatens to trigger a debt-deflation process, policy interventions by both the government and the central bank can really make a difference in the path of the economy through calendar time. Nothing in the paper before us exhibits an appreciation of the change in the character of the "stabilization problem" over the years surveyed.

Once the potential consequences of the mismanagement of policy becomes so much more serious, the importance of economic theorizing about stabilization policy increases. In particular, economic theory needs to be relevant in the sense that the critical situations -- in this case financial instability and the way in which financial variables affect aggregate demand -- are well defined within the theory. If theory is based upon misspecifications of the economic process and the problems faced by policymakers, then theory cannot be relevant: garbage in -- garbage out applies to theory construction as well as to computer modeling.

The problems of the economy have been exacerbated because policy-makers have been guided by insights and conclusions drawn from neo-classical theory. Neo-classical theory is an inappropriate tool for dealing with instability, for financial or any other instability is foreign to this theory. In neo-classical theory any deviation from equilibrium must be due to exogenous developments and any sustaining of a disequilibrium must be due to "barriers." Neo-classical theory is able to explain instability only by postulating the existence of one or more devils, be they trade unions, OPEC, monopoly, the central bank, government or democracy. Because economic policy advising over the past decades has been largely monopolized by practitioners of neo-classical theorizing our current economic malaise is in good measure iatrogenic. The physicians, including our hosts, have served to make the disease worse.

A theory of stabilization policy is needed if and only if the economy is unstable. There is no sense whatsoever to the concept "stabilization policy" if the beast is stable. When Wallace, Sergent, et al. play their games by positing a system whose behavior is determined by elements that are independent of the variables that, in their specification, stabilization policy directly affects, then the proposition that policy does not matter is true not by demonstration but by assumption. As the instability that is so evident in the world cannot occur within their models, the games they play only serve to show that their models and the empirical tests that they perform are irrelevant for our economy. In my view the strong proposition that emerges from one literature surveyed by Taylor, is that this large body of work is irrelevant for the world in which we live. If economics is to be

anything more than an academic nit-pick, theory and theorizing has to go in other directions than those represented in the literature Taylor surveys.

If economic theory is to be relevant for stabilization policy in our economy, the questions that must be addressed are "why and in what way is our economy unstable?" Note the phrase "our economy." The subject matter of any theory that aims to be relevant is not an abstract economy devoid of institutional detail but rather an economy that is rich in specific institutional detail and which exists at a particular time and has a special history. The problem of economic theory is to select the essential details of the institutional framework to model: the aim of the theorizing is to show causal connections that lead to the observed instability. The hope is that by showing how instability is generated the theory will indicate policy interventions which can attenuate if not eliminate instability.

Although the lines of argument examined by Taylor are largely irrelevant to the topic of this conference, "Stabilization Policy: Lessons from the 1970s and Implications from the 1980s," there were developments in theory over the past decade that are relevant to stabilization policy: Taylor either is ignorant of these developments or chose to ignore them. The developments in economic theory in recent years that are relevant to the theory of stabilization policy are:

- 1) Progress in general equilibrium theory
- 2) The two-Cambridge debate
- 3) The recovery of the "lost" financial elements in Keynes.

Because I am writing a comment rather than a survey article I will just devote one paragraph to each of these developments.

During recent years progress in general equilibrium theory made the conditions that need to be satisfied for the key propositions of this theory to be valid precise. One conclusion of these developments is that the coherence and coherence-seeking theorems of general equilibrium theory are not unconditionally valid for a decentralized set of markets with capital assets, money, banking and financial institutions such as we have. An implication of this conclusion is that the introduction of money as an "exogenously determined" instrument designed to facilitate trade into a general equilibrium model in which relative prices determine consumption and production decisions throws no light whatsoever on the behavior of a capitalist economy with a "money" that is created in a banking process. There is no established microeconomics that can serve as a basis for a macroeconomic or monetary theory that is relevant to stabilization policy as long as the results in microeconomics depend upon highly artificial constructions to explain the existence of and changes in money.<sup>1</sup>

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<sup>1</sup>Of the general equilibrium theorists, perhaps F. H. Hahn has been most open about the limitations of theory. See F. H. Hahn: "On Some Problems of Proving the Existence of an Equilibrium in a Monetary Economy," in R. Clower (ed.), Monetary Theory (Penguin, 1969). "Professor Friedman's Views on Money," Economica, February 1971, 38 (149), pp. 61-80. On the Notions of Equilibrium in Economics (Cambridge: Cambridge University Press, 1973).

Also see

K. Arrow and F. H. Hahn, General Competitive Analysis, (San Francisco: Holden Day, 1971), especially Chapter 14, The Keynesian Model, pp. 347-369. In introducing their discussion they note that in their earlier proof that a temporary equilibrium always exists they "...supposed that at the moment an equilibrium was shown to exist, economic agents had no capital assets as we know capital assets. It is interesting to note that Arrow and Hahn head Chapter 14 with a quotation from W. B. Yeats, The Second Coming, "Things fall apart, the centre does not hold."

The two-Cambridge debate, ostensibly about capital theory, was really about the validity of the integration of Keynesian theory with the earlier neo-classical theory. The critical issue that the debate clarified centered around the pricing of capital assets. A capitalist economy is characterized by two price systems. One is the price system of current output, the second is the price system of capital assets. The price system of current output largely depends upon wages and mark-ups, whereas the price system of capital assets depends upon current estimates of future expected profits, current estimates of the uncertainties involved over various horizons, and current capitalization rates of profit streams. In an economy with the monetary, banking and financial systems that characterizes capitalist economies the capitalization rate is a "monetary" phenomena and the two price systems can and do vary relative to each other. Inasmuch as the ratio of the capitalized values of expected future profits to the supply price of investment output is a determinant of investment demand, aggregate demand is sensitive to the ratio of these two sets of prices. The two-Cambridge debate is of vital importance for the theory of stabilization policy because it leads to the conclusion that if the ongoing processes of an economy affect this ratio it will lead to endogenous change in the performance of the economy: i.e., variations in the ratio of employed to available resources will result. The two-Cambridge debate made it clear that the "proofs" in the literature that a growth equilibrium of an investing capitalist economy exists depend upon the assumption that the present value of future profits always equals the perpetual inventory valuation of capital assets. But the equality of the two valuations of capital assets is an attribute of equilibrium. The

"proofs" of the coherence of an investing capitalist economy does not hold; the proofs depend upon first assuming that a condition of coherence exists.<sup>2</sup>

The third theme in economic theory in the 1970s that is relevant to stabilization policy is the recovery of the financial and monetary aspects of Keynes' revolution in economic theory. There is something very queer about the standard interpretation of Keynes as embodied in the various IS-LM models. This essentially non-monetary view of the economy is paraded as a representation of the theory of the major economic theorist whose life's work was almost entirely on money and finance. In the recovery of what lost in the Hicks-Hansen-Klein-Modigliani-Patinkin tradition it became clear that underlying Keynes' theory was the premise that to understand capitalism it is necessary to model capitalism. This means that it is necessary to model the way positions in capital assets and investment are financed, the dependence of this financing upon the banking and financial system, and the effects of financing relations first upon investment and then on income, employment and prices. In this analysis, in a capitalist economy unemployment exists when the long run expectation of profits by business men together with capitalization rates that reflect portfolio preferences in an uncertain world lead to demand prices for capital assets that are "too low" relative to the supply prices of investment output. The demand price for capital assets as well as the supply price of investment

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<sup>2</sup>This is the outcome of the two-Cambridge debate on Capital Theory, although the standard discussion and summary of the debate, G. C. Harcourt, "Some Cambridge Controversies in the Theory of Capital," (Cambridge, England: The Cambridge University Press) does not make this clear.

output depend upon financing terms. Financing terms, which cannot fully be captured by a single interest rate, reflect whether or not recent and near term expected behavior of the economy lead to sufficient realized and expected profits that almost all of the payment commitments on outstanding obligations are expected to be fulfilled. By integrating money, finance, expected profitability and the supply price of current output into a theory of effective demand, Keynes developed the basis for a theory of the economic processes of a capitalist economy that explained why such an economy is "so given to fluctuations." Instability is an inherent characteristic of a capitalist economy in Keynes' theory. Furthermore, Keynes' theory is rich, for even though it does not lead to a set of policies which eliminate instability, it does lead to policy moves (fiscal policy) which offset the effects of instability upon employment and aggregate income.<sup>3</sup>

As the 1970s matured, history advanced the argument from the simple question of "why is our economy unstable?" The question that economic theory had to address if it was to be relevant to stabilization policy became "why is it that our economy is so much more unstable in the 1970s than in the 1950s?" The issues that theory had to

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<sup>3</sup>Among the "key works" in the reintegration of money are: Joan Robinson, Economic Heresies, (London: MacMillan, 1971). P. Davidson, Money and the Real World, (New York: John Wiley & Sons, 1972). J. A. Kregel, The Reconstruction of Political Economy, (London: MacMillan, 1973). S. Weintraub, A Keynesian Theory of Employment, Growth and Income Distribution, (Philadelphia, Chilton 1966). Victoria Chick, The Theory of Monetary Policy, (London: Gray-Mills Publishing Ltd., 1973). H. P. Minsky, John Maynard Keynes (New York: Columbia University Press, 1975).

address can be made even more precise by dividing the question into two parts: "Why is it that our financial system seemingly is more unstable, more vulnerable to threats and partial realizations of financial crises (both domestic and international) since the middle 1960s?" and "Why is it that inflation became more serious as the 1970s progressed?"

Once economic theory moves from the study of an economy to the study of our economy and once the various faces of the instability of our economy are taken as the problems theory must address then the need to model money, banking and capital-asset pricing moves to the foreground. In Taylor's survey, which presumably deals with stabilization policy, banks and banking are nowhere discussed. We all know that in our economy money is created by the actions of profit seeking banks and other financial institutions, that the assets acquired and liabilities issued by banks evolve in response to profit opportunities, and that the mix and activities of financial institutions also evolve. This implies that an economic theory applicable to our economy will integrate banking and financing markets into the determination of capital asset prices, investment decisions and the determination of the domain of stability of the economy. You cannot understand something by ignoring it. The literature discussed by Taylor's paper ignores banking and Taylor, by his selection of the literature to discuss, apparently believes you can understand and give guidance for stabilization policy for our economy by ignoring banks and banking.<sup>4</sup>

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<sup>4</sup>It would be useful if today's economists were acquainted with H. Simon's "Rules vs. Authorities in Monetary Policy," Journal of Political Economy, 1937.

It has long been argued that the instability of the economy is related to the structure of liabilities by which positions in capital assets are financed.<sup>5</sup> Experience during the 1970s lends substance to this argument. The relation between the debt financing of capital assets positions and the need to fulfill commitments on maturing debt by rolling over debts - by issuing new debts - is a critical determinant of the stability of an economy with sophisticated finance. As a result of the maturing of the flow of funds data (poorly designed as the set of accounts may be) it is possible to relate the evident instability of our economy to the growth of the debt structure relative to income and the increased complexity of financial relations. In order to answer questions about why our economy is unstable it is necessary to fully integrate the monetary mechanism with system behavior. The literature Taylor surveys is "vague" or "silent" on the processes by which positions in capital assets are financed.

One striking characteristic of our economy that became evident in the 1970s is the link between financial instability and accelerating inflation. Since the mid 1960s whenever the Federal Reserve follows the rules for monetary policy to constrain inflation that were developed on the basis of the experience of the 1940s and '50s, a financial crisis develops; when the Federal Reserve and the government succeed in containing the crisis so no deep and long recession follows, the financial base is laid down for inflation at a higher rate. Since the middle 1960s we have had three "cycles" of inflation, constraint,

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<sup>5</sup>I. Fisher, "The Debt-Deflection Theory of Great Depressions," Econometricas, 1933.

incipient financial crises, lender of last resort intervention, federal deficits, renewed expansion, financial innovation and accelerated inflation. In each cycle this "sequence" took four to five years to work its way through the system.

Any theory that is useful for stabilization policy will need to explain why the economy reacted to variations in the rate of growth of the reserve base, or in one manner in the years prior to 1965 and in another manner in the years since 1965. For an economic theory to do this it need contain a sub-theory of "financial stability and instability." Nowhere in Taylor's survey or in the literature he surveys is this aspect of the stabilization problem addressed.

Any theory of the capitalist process needs to focus in the decisions to own capital assets, the techniques used to finance control over capital-assets and the investment and investment financing processes. Obviously a theory, if it is not merely mechanistic, which explains decisions today that are based upon future revenues and costs will include a theory of expectation formation. The fundamental problem in the making of decisions today that involve revenues and costs over a significant time horizon is that the future is uncertain; the future cannot be represented by a set of nice stable probability functions over well-defined outcomes.

The need to make decisions in an uncertain world leads to one question, "how does one behave rationally in an irrational world?" An "irrational world" is one in which what happens is not explained with the requisite precision by the accepted theory. As long as theory does not explain a phenomena with the exactness required for decision, then the world of that phenomena is irrational. If, for a capitalist

economy, the world conforms to expectations derived from standard theory a large part of the time, even as it behaves in a manner (instability) inconsistent with this theory a part of the time, then decision making formulas that use the accepted theory will not determine the behavior of a rational man. In a world where diverse types of behavior can occur, theory is effective as a guide to decision and policy exactly as it yields information as to which of the diverse types of behavior of the economy is likely to rule. If economic theory is to be an ingredient in the formation of expectations by a rational man, it needs to relate the expected behavior of the economy to history and the evolving institutional arrangements.

The Franklin National bankruptcy of 1974 and what followed is a concrete example of a situation in which policy actions truly affected the behavior of the economy. In May of 1974 the Federal Reserve, under Arthur Burns, opened the discount window wide to Franklin National so that all of Franklin National's overseas and money market liabilities were validated. The Federal Reserve by this action aborted a wave of withdrawals from the international banks and assured the "world of international finance" that the offshore liabilities of large, if not respectable, American banking institutions were implicit contingent liabilities of the Federal Reserve. This and related interventions by the Federal Reserve and cooperating institutions in 1974-75 together with massive government deficits made it virtually certain that the recession of 1974-75 would be contained and that the subsequent recovery would lead to serious balance of payments difficulties and inflation at an accelerated rate. Policy may not always matter, but

there are junctures in the history of an economy when policy really matters: 1974-75 was one such juncture.

It is the duty of economists who parade as knowing something about stabilization policy to be aware of such issues. Neither the literature Taylor discusses nor Taylor in his paper seem to be aware of these problems. Theory that is useful for stabilization policy needs to offer guidance to central bankers and other policymakers when they are faced with the need to act in a situation such as ruled in 1974-75. By this criteria, neither Taylor's paper nor the literature he chose to report on are useful.