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THE ROLE OF DEMAND MANAGEMENT IN THE MAINTENANCE OF FULL EMPLOYMENT

Bennett T. McCallum

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ABSTRACT

This paper begins by identifying nominal price stickiness as the logical basis for the Keynesian or activist point of view concerning demand management policy. It then characterizes two alternative approaches to policy analysis that have been adopted by adherents of the Keynesian position, the "disequilibrium" and "Phillips curve" approaches. The former is inherently defective, it is argued, while the latter has yet to be satisfactorily implemented. Indeed, implementation that is not open to Lucas-critique weaknesses is not in sight. In response to the implied dilemma for policy makers, the paper describes a rule for the conduct of monetary policy that relies upon minimal understanding of price-adjustment dynamics and which should be robust to regulatory and technological change in the economy's financial and payments institutions. A bit of evidence is presented to suggest that the rule would, if adopted, lead to approximately zero inflation (on average) and to output/employment fluctuations that are small by historical standards. Possible criticisms relating to recent European experience and to recent theoretical developments are considered.

Bennett T. McCallum
GSIA
Carnegie-Mellon University
Pittsburgh, Pennsylvania 15213
(412)268-2347

I. Introduction

In his General Theory, Keynes (1936) put forth the radical proposition that competitive market economies have no automatic mechanism that tends, in the absence of governmental policy guidance, to eliminate or prevent unemployment. After a lengthy period of debate that was often confused, in part because of Keynes's reliance on non-traditional concepts and terminology, it came to be widely agreed that this proposition was false as a matter of pure economic theory.[1] But it also came to be widely agreed that the economy's self-correcting forces work slowly, so that well-designed demand management policy actions can be helpful in reducing the magnitude and duration of departures of employment and output from their full equilibrium levels. An influential expression of this point of view--the activist demand-management position--was provided by Patinkin (1951).

But, as all readers are well aware, a number of challenges to this position have arisen in the past 20 years. Arguments by influential analysts including Friedman (1968), Lucas (1972), Sargent and Wallace (1975), and Barro (1979) have claimed that the intellectual foundations of the activist position are seriously flawed, and have suggested that activist policies are apt to be counterproductive. Several weaknesses in these arguments have been detected,[2] however, and there has recently been something of a resurgence of Keynesian sentiment among macroeconomic researchers.[3] At present, consequently, there exists substantial disagreement among leading scholars concerning the nature of macroeconomic phenomena and the kind of policy that should be pursued.

The present paper begins by identifying nominal price stickiness as the logical basis for the Keynesian or activist point of view concerning demand policy. It then characterizes two alternative approaches to policy analysis that have been adopted by adherents of the Keynesian position, the

"disequilibrium" and "Phillips curve" approaches. The former is inherently defective, it is argued, while the latter has yet to be satisfactorily implemented. Indeed, implementation that is not open to Lucas-critique weaknesses is not in sight. In response to the implied dilemma for policy makers, the paper proposes a rule for the conduct of monetary policy that relies upon minimal understanding of price-adjustment dynamics and which should be robust to regulatory and technological change in the economy's financial and payments institutions. A bit of evidence is presented to suggest that the rule would, if adopted, lead to approximately zero inflation (on average) and to output/employment fluctuations that are small by historical standards. Possible criticisms relating to recent European experience and to recent theoretical developments are considered.

II. Disequilibrium Analysis

It is widely agreed that the Keynesian rationale for activist demand-management policy is based on a presumption that there exists a significant extent of nominal price stickiness somewhere in the macroeconomic system. This stickiness may pertain to product prices or wages or both, but some type is necessary for the Keynesian diagnosis and remedy to be applicable. For without any price stickiness, real demands and supplies for commodities including labor will be equated in a fashion that leaves no clear-cut role for demand management. And since demand-management actions are effected by way of nominal instrument variables, policy manipulation of real aggregate demand is itself dependent upon a significant degree of nominal stickiness.[4] It is nominal aggregate demand that is generally open to manipulation and with which demand-management policy is properly concerned.

The crucial status of price stickiness in the context of demand-management analysis leads directly to a significant issue: how is the concept of "price stickiness" to be represented analytically? The concept is evidently one that is inherently dynamic in nature but, as we all know, the formal analysis of Keynes (1936) and of those writers[5] who clarified the message of the General Theory was conducted in a comparative-static framework. Accordingly, some means had to be found for representing a dynamic concept in a static setting. The device adopted by Keynes and the other early contributors was that of conditional equilibrium analysis--comparative statics in which the economy's slowly-adjusting prices are treated as if they were fixed quantities. Policy experiments conducted under this approach are comparative-static exercises carried out conditional upon "given" values of the prices that are hypothesized to adjust slowly. For some given value of the nominal wage rate, for example, the analyst could compare values of

endogenous variables that would obtain under alternative hypothetical magnitudes of the money stock or government purchases and use this comparison as the basis for analysis of an economy in which the nominal wage adjusts slowly.

But of course actual economics are not static entities, but ongoing dynamic systems. So the question remains of how to relate these conditional comparative-static exercises to actual problems of demand management. One conceivable approach would be simply to pretend that static analysis provides a satisfactory approximation. According to that approach, the analyst would use the model in choosing policy actions at time t by treating the current value of the sticky price (e.g., W_t) as historically given and ignoring the future (which can perhaps be attended to when it becomes the present). Then later in period t+1 the new value W_{t+1} could be treated as historically given and new policy actions selected conditional upon that value. By proceeding period after period in this fashion, it would be possible for the analyst to use the static model in practice without ever developing any explanation for the W_{t+j} values that are "given" in the successive periods.

It would seem to be indisputable, however, that such a way of proceeding is highly suboptimal. For even if W_t were actually a given magnitude in t, in the sense of being unresponsive to current policy actions, its current value would certainly have been influenced by economic conditions and policy actions of the past. Any (temporarily) fixed price should be classified as a predetermined variable, not as one that is literally exogenous. Policy actions taken in t will accordingly have effects on future prices--on W_{t+1}, W_{t+2}, etc.--and these effects are ignored in the procedure under discussion. That procedure is consequently bound to be suboptimal.

As well as I can determine, this suboptimal approach to policy analysis

is implicity recommended in most of the literature that has passed under the title of "disequilibrium" or "fixed-price" macroeconomics.[6] The technically sophisticated contributors to that literature might deny any intention that their work be used in such a manner, but it is unclear that there is any other way to proceed with a model that provides no explanation for the evolution over time of the system's sticky prices. The primary objection to these models, according to my argument, is not that they treat prices as temporarily rigid, but that they include no explanation of price adjustment between periods. From a practical policy perspective, these models are crucially incomplete.[7]

A rather vivid illustration of the potentially misleading nature of policy analysis conducted with an incomplete, fixed-price model was provided by an example developed in McCallum (1980). In the model used for this example, real aggregate demand y_t^d is assumed to be dependent upon real money balances, real government purchases, and a stochastic shock term while (for simplicity) aggregate supply is taken to be a constant, $y_t^s = \bar{y}$. Prices are set at the first of each period and are unresponsive to developments occurring within the period, i.e., to shock realizations. Consequently, y_t^d and y_t^s will typically fail to coincide in which case the quantity actually transacted--the output forthcoming--is determined as in the disequilibrium literature as the smaller of the two: $y_t = \min (y_t^d, \bar{y})$. When there is a negative shock to to demand, there will be a tendency for $y_t^s = \bar{y}$ to exceed y_t^d implying Keynesian unemployment proportional to $\bar{y} - y_t^d$.

Clearly this model is such that within any period in which $y_t < \bar{y}$, it is the case that if the money stock or government purchases were larger in magnitude, then y_t^d would be greater and $\bar{y} - y_t$ would be smaller--perhaps zero. So from the perspective of conditional comparative statics the model seems to

be supportive of the idea that activist demand management can be effective in terms of preventing unemployment.

But in order to discuss the average effects over time of a sustained policy strategy, one needs to complete the model by specifying how prices adjust between periods and adding policy rules that determine policy instrument settings. In the example under discussion, the price adjustment specification is an augmented Phillips relation in which the proportionate price change is determined by the previous period's excess supply $\bar{y} - y_{t-1}$ and the expected proportionate change of the market-clearing price level.[8] The policy instruments, finally, are set by feedback rules that take account of all relevant variables realized in the past. Current magnitudes are assumed unknown, however, to the policy authorities.

In this setting, to come to the point, it is demonstrated that if expectations are rational the famous (or infamous) policy-ineffectiveness proposition obtains. That is, the evolution of y_t (and thus $y_t - \bar{y}$) is independent of the coefficients of the policy feedback rules: whether the instrument settings feature strong responses or none at all to (e.g.) past excess supply values makes no difference whatsoever in the time series behavior of y_t or $y_t - \bar{y}$.

The purpose of citing this example is <u>not</u>, it should be emphasized, to suggest that the policy ineffectiveness proposition is applicable to actual economies. It is, rather, to illustrate the potentially misleading nature of conditional comparative-static policy analysis with incomplete fixed-price models. Such analysis is prone to overstate the potential effectiveness of demand management policy by failing to take account of dynamic considerations concerning the manner in which currently "given" prices reflect previous responses to past economic conditions.

III. Price Adjustment Models

Many practical Keynesian analysts--especially those working with quantitative models--have recognized the point of the previous section, of course, and have adopted instead a second approach. Instead of treating the model's sticky price or prices as if they came out of the blue, this second approach adds[9] to the static Keynesian model another equation or set of equations--a "Phillips curve" or a "wage-price sector"--designed to explain movements over time in the slowly-adjusting price or prices. This step converts the model into one that is dynamic and complete, and renders it usable for policy analysis that avoids the particular source of suboptimality described above.

But while the inclusion of price-adjustment equations makes the second approach more suitable than the first, as a method of adapting Keynesian models to demand management purposes, the price adjustment equations that have been used in practice are open to a number of objections. At the most sympathetic level, one objection is that most of the utilized specifications fail to satisfy the natural rate hypothesis, i.e., the hypothesis that there is no path of price level or nominal demand values that will keep unemployment permanently below its natural-rate value.[10] This hypothesis, which expresses the notion that it is not possible for a society to permanently enrich itself in real terms by monetary means, is generally accepted by neoclassical theorists and is paid lip service by most Keynesian writers, but is violated by most econometric specifications. Models incorporating the concept of a non-accelerating-inflation-rate-of-unemployment (NAIRU), for example, do not satisfy the natural rate hypothesis. For if there is a stable relationship between the unemployment rate and the inflation acceleration variable, then there are evidently price level time paths that represent an acceleration

magnitude that would yield a permanently lowered unemployment rate.[11] Of course the builders of such models do not intend that they be applicable to "impractical" conditions such as a maintained acceleration of inflation. But this type of disclaimer amounts to an admission that the relation in question is not structural--i.e., is not invariant to policy regimes.

A more fundamental criticism of existing price adjustment specifications is expressed by proponents of the equilibrium approach to business cycle analysis. All readers will be aware that this line of work began with Lucas's (1972) celebrated theory of a Phillips-type relationship between nominal and real variables that results from confusion due to information gaps, not from price stickiness per se. Most readers will also know that Lucas's theory has recently suffered a decline in popularity as a consequence of its reliance, for real effects of monetary shocks, on an implausible degree of ignorance concerning current monetary conditions on the part of rational private agents. Since information regarding various aggregate nominal magnitudes--price indices as well as money supply figures--is available both promptly and cheaply, the Lucas "monetary misperceptions" model has come to be viewed as inapplicable to today's developed economies.[12]

Disenchantment with the misperceptions model has not, however, led to the demise of the equilibrium school of business cycle analysis. Indeed, an important group of researchers has in a sense retained the Lucas model[13] despite its failure to rationalize output and employment effects of monetary shocks. Specifically, this group has developed a real business cycle (RBC) approach which denies that there is in fact any significant effect of monetary policy actions (even if unanticipated) on output. The money-output correlations that appear in the data are attributed, by RBC proponents, to "reverse causation," i.e., policy and/or banking sector responses to output

fluctuations. These fluctuations, to complete the story, are brought about by real shocks, primarily exogenous shocks to technology.

The RBC approach--which stems from the work of Kydland and Prescott (1982), Long and Plosser (1983), and King and Plosser (1984)--has gained a considerable amount of support in part because of the elusiveness of a rigorous theoretical account of money-to-output influences, but also because of quantitative work supportive of the RBC hypothesis. The pioneering study in this regard is that of Kydland and Prescott (1982), which demonstrates that a surprisingly good quantitative match to actual business cycle facts[14] can be obtained (via simulations) with a quantitative equilibrium model in which a stochastic technology shock provides the only source of fluctuations. In particular, the RBC models imply procyclical fluctuations in labor productivity and real wages, an implication that is more consistent with actual data than those of many traditional models that attribute cycles to demand fluctuations. Also, the relative variability of consumption and investment expenditures is well explained, as well as the serial correlation in output and employment magnitudes. These implications require the assumption that technology shocks are highly persistent, but that is entirely plausible.

Other types of evidence have also been put forth as supportive of the RBC hypothesis. I have argued (McCallum, 1986) that much of this is inconclusive if not irrelevant, but it remains a striking fact that money stock and other demand-related variables have very little predictive content for output fluctuations, especially in data series that have been first-differenced.

Probably the most serious weakness of the RBC approach is the lack of a convincing description of the unobserved "technology shocks" that it posits as the source of cyclical fluctuations. If the term is interpreted literally as

referring to shifts in the state-of-knowledge physical frontier relationship between inputs and outputs, then it would seem implausible that there could exist much variability at the aggregate level; specific technological improvements should impact on the production functions for only a few of the economy's many products. And independent shocks to different productive sectors would tend to average out, yielding a relatively small variance in the aggregate.

For this and other reasons, most macroeconomists have found the RBC hypothesis unconvincing. But the vitality of the research being conducted by the RBC school is a testimony to the attraction of the equilibrium approach and to the dissatisfaction of many economists with existing models of price stickiness. Let us then return to our main theme by reviewing the basic rationale for the equilibrium approach.

Existing equilibrium models are ones in which all prices are perfectly free to adjust within each period, but that is not the defining characteristic of the approach. The latter's basic requirement, rather, is that a model's behavioral relations should all be rationalized in terms of explicit maximizing optimizing agents--households and firms--in response to their own objectives and the constraints they face (Lucas, 1980). The motivation for this modelling strategy is the objective of producing a model that is well-designed for the guidance of economic policy. The presumption is that by focussing on agents' objectives and constraints, it might be possible to construct a model consisting entirely of relations that are truly structural. Relations derived in this way would, because of the autonomy of preferences and technology, stand a reasonable chance of being invariant to policy changes.

As stated above, this strategy does not necessarily rule out price stickiness. One can conceive of a model, for example, in which multiperiod

nominal contracts are endogenously explained as the response of rational agents to adjustment, bargaining, or marketing costs--in which case the model could be of the equilibrium variety. But the approach does not permit the inclusion of relations describing sluggish price adjustments effected by "the market" or by some fictitious "auctioneer" with ill-defined or nonexistent objectives. Being poorly understood--not based on well-posed choice problems--such relations would not be structural. They would not, in other words, provide the analyst with any basis for knowing whether they would remain in place or shift if policy were substantially altered. But such knowledge is clearly crucial for designing policy, as a shift would invalidate the model's predictions about the effects of a contemplated policy change. In summary, it is necessary, according to the equilibrium-approach viewpoint, to understand the nature of price-adjustment sluggishness to know if its quantitative characteristics will remain intact in the face of altered conditions.

The forgoing argument is of course an application of the "Lucas critique" developed in Lucas (1976). In principle, its considerations are applicable to most components of a macroeconomic model. But because of the crucial role of expectational considerations in the price-adjustment sectors of these models, it is these sectors that would seem to be especially susceptible to the critique. Relations among variables all of one type, either nominal or real, would seem to be less likely to break down in response to demand-management policy changes.[16]

The foregoing discussion suggests that, in principle, the modelling strategy of the equilibrium approach could provide a satisfactory basis for demand-management policy analysis. In practice, however, it has proved to be extremely difficult to model sluggish price adjustments in the manner

required. Tangible resource costs of making price changes seem to be negligibly small, while "bargaining" and "marketing" costs of price adjustment are poorly understood. Consequently, to the present time all equilibrium models have been ones with complete price flexibility and, therefore, no role for demand management. No model of sticky prices has been devised that combines empirical veracity with an adjustment specification that is clearly based on individuals' objectives and constraints.

As a result, a sizeable group of researchers has reacted against Lucas's suggestion that price stickiness needs to be explained along equilibrium-approach lines. In reality, these researchers contend, prices do not adjust promptly for a variety of complicated strategic and semi-institutional reasons that are not amenable to taste-and-technology analysis. Consequently it is better (according to their view) to use a poorly understood but empirically justifiable Phillips-type relation than to pretend--counterfactually--that all price adjustment take place promptly, as equilibrium analysts have assumed in practice. An econometric model based on this presumption will track data better than if it incorporated the hypothesis of perfectly flexible prices. And policy predictions provided by the model could be satisfactory if the adjustment relation did not shift sharply when policy changes were undertaken.

It is hard not to have considerable sympathy for this last suggestion. Yet, on the other hand, the logic of the Lucas critique is inescapable: how can one know that the adjustment relation will not shift sharply if he does not understand its nature? Finding a way out of the implied dilemma is perhaps the most crucial task confronting policy-oriented macroeconomists today.

IV. A Strategy for Monetary Policy

In light of the policy dilemma just described, the appropriate response would seem to be one that is not excessively ambitious. My proposed approach begins by accepting the idea that the nature of price adjustment relations--and thus the connection between nominal and real variables--is poorly understood. There is no compelling basis for selecting any one of the numerous competing theories of this mechanism, and no good prospect for better understanding in the near future. But the proposed approach reflects optimism nevertheless, for it involves a strategy for monetary policy behavior[17] that gives promise of being effective regardless of the nature of the mechanism.

The basic idea is that, in whatever way it is that monetary (or fiscal) actions affect output, they do so through an intermediate influence on nominal aggregate demand. Evidence suggests, furthermore, that cyclical fluctuations in real output and employment are strongly related to those in nominal demand. Real GNP growth is usually strong, that is, when nominal GNP growth is above average.[18] Consequently, there is some basis for belief that cyclical fluctuations in real output would be significantly dampened if nominal GNP were kept on a smooth and steady growth path.

Of course the last statement would be questioned by proponents of the RBC hypothesis. But according to their theory, the behavior of output is independent of nominal variables in any event, and the behavior of nominal variables is of no concern--except to the extent that inflation imposes an inefficient tax on the holders of real money balances. Consequently, RBC proponents should have no objection to a policy strategy that yields a steady growth rate for nominal GNP,[19] provided that it is not inflationary.

At what rate should nominal GNP be made to grow? While a mild deflation in accordance with the "Chicago Rule" of Friedman (1969) is perhaps preferable

in principle, from a practical point of view there is much to be said for an average inflation rate of zero.[20] Taking that as a goal, then, I suggest that nominal GNP should be made to grow at a rate equal to the long-term average rate of real output growth for the economy in question--about 3% per year, for example, for the United States.

My suggested approach does not, however, consist merely of the adoption of a target path for nominal GNP. Equally essential is the mechanism for achieving that path. In that regard three considerations are extremely important. First, the mechanism should involve a policy rule that dictates each period's setting of the policy instrument. It is important to have a rule, rather than relying on "discretionary" period-by-period choices of the instrument setting, in order to avoid dynamic inconsistency of the type described by Kydland and Prescott (1977) and Barro and Gordon (1983). Those authors show that period-by-period attempts to optimize, by a monetary authority who seeks to avoid both inflation and unemployment, will lead to more inflation and no less unemployment (on average) than could be obtained by adherence to a rule. It is my opinion that this type of inconsistency offers the best available explanation for the unprecedented postwar inflationary experience of most developed countries, experience which has seen the CPI climb to 4.5 times its 1950 level in the U.S. and nearly 11 times in the U.K.[21]

Second, the rule needs to pertain to a directly controllable variable, rather than one such as the M1 money stock (or any broader measure). Otherwise, the rule will not be operationally specified. Third, the rule should be designed in a manner that does not rely upon the absence of regulatory change and technical innovation in the payments and financial industries. While these processes may not produce as much turmoil in the

future as they have in the recent past, it would be unreasonable to presume that they will not be present again to a significant extent.

Following up on previous suggestions of mine (McCallum, 1984), I have recently developed in quantitative terms a rule for U.S. monetary policy based on these considerations. This rule dictates quarterly settings for the monetary base that are designed to keep nominal GNP close to a 3% growth path.[22] It does not rely on any specific model of the economy or any details regarding the financial system; all it presumes is that an increase in the growth rate of the monetary base tends to have a stimulative effect on nominal GNP. Defining b_t = log of monetary base (for quarter t), x_t = log of nominal GNP, and x_t^* = target-path value of x, the rule is as follows:

$$\Delta b_t = 0.00739 - (1/16)\,[x_{t-1} - x_{t-17} - b_{t-1} + b_{t-17}] + 0.25\,(x^*_{t-1} - x_{t-1}).$$

Here the constant term is simply a 3% annual growth rate expressed in quarterly logarithmic units, while the second term subtracts from this the average growth rate of base velocity over the previous four years.[23] Finally, the third term adds a gentle adjustment in response to cyclical departures of GNP from its target path.

To determine whether this rule would indeed keep nominal GNP close to the desired growth path, one must experiment with the economy or with a model. The former possibility is too expensive and the latter suffers from the absence of any reliable model. But it is my conjecture that the proposed rule would perform well with a wide variety of models. Here I will briefly summarize results for three extremely simple models. The first is an atheoretic regression of Δx_t on Δb_t and Δx_{t-1}; for the sample period 1954-85 the estimates are as follows:

$$\Delta x_t = \underset{(.002)}{0.00749} + \underset{(.079)}{0.257}\,\Delta x_{t-1} - \underset{(.121)}{0.487}\,\Delta b_t + e_t$$

$R^2 = 0.23$ $\sigma = 0.0010$ DW = 2.11.

Generating b_t and x_t values from the proposed rule and this model, with residuals fed in each period to represent shocks, one estimates that the root-mean-square value of $x - x_t^*$ for 1954-85 would have been only 2.0% had the rule been in effect. Actual historical policy, by contrast, yielded a 77.1% root-mean-square error (RMSE) relative to the x_t^* target path [24] and a 8.5% RMSE relative to a fitted linear trend. The second model differs from the first only in lagging Δb_t one quarter, to reduce the possibility of reverse causation in the estimated effects. The resulting coefficient estimates are not much different and the simulated RMSE for 1954-85 is 2.2%. The third model explored to date is a four variable vector autoregression system in which the variables are growth rates of the base, the price level, and real GNP, plus a nominal interest rate. With this system, the estimated RMSE value is again 2.2%.

Of course each of these experiments is in principle subject to the Lucas critique. I would argue that the first two are less susceptible--for the reason sketched above--than if the models included both real and nominal variables. But my main line of defense in this regard is to be based on the robustness of the rule to widely different models.[25]

My contention is not only that the suggested policy rule would keep nominal GNP close to its target path and thereby eliminate inflation, but would also result in smaller cyclical fluctuations in real output and employment than the U.S. economy has experienced in the postwar era. But we know that these fluctuations have been small relative to those of previous historical eras, and reasonably small in absolute terms. Thus the contention is that the proposed rule would, if utilized in a developed economy,[26] result in macroeconomic performance of a high standard.

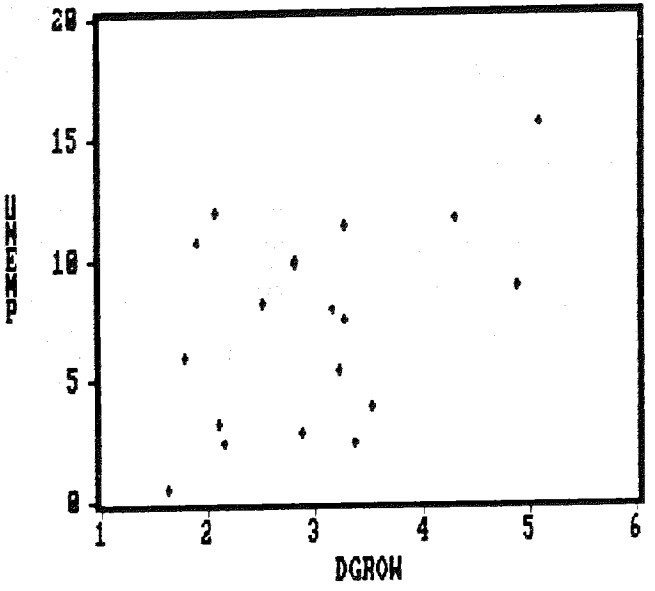
V. Topical Issues

Before concluding, it will be useful briefly to address a few topical issues concerning the proposed policy rule and, more generally, Keynesian views on the need for activist demand management. The first of these issues pertains to the unusually high unemployment rates experienced during recent years in many European nations, including the U.K. While some economists have attributed this unemployment primarily to inadequate demand, our proposed policy rule would have dictated substantially less nominal demand growth than was actually experienced over the last decade or so. Should this be regarded as a mark against the rule?

To answer that question properly one would have to identify the source of the unusual unemployment. Clearly, such a task is beyond the scope of the remainder of the present paper. Nevertheless, as a crude check on the notion that demand inadequacy bears the primary responsibility, let us conduct a cross-nation comparison. To that end, Figure 1 plots average unemployment rates for 1980-84 against nominal GDP growth over the decade 1975-85 for 19 OECD nations.[27] If relatively high unemployment were associated with relatively slow demand growth in this cross section, the points would indicate a downward-sloping relationship. But a glance at Figure 1 shows that no such relationship is present. It is also the case that, for many of the individual countries considered, nominal GDP growth has been more rapid in the 1975-85 period than during the low-unemployment years of 1950-70. At this level of extremely simple comparisons, then, the evidence does not support the notion that demand inadequacy is the source of the problem.

The other issues to be considered relate to recent theoretical developments that have been interpreted as supportive of the hypothesis that activist demand management is both needed and feasible. In particular, the

Figure 1



Unemployment Rates, 1980-84, and nominal GDP growth, measured as a ratio of 1985 to 1975 values, for 19 OECD countries.

Sources: OECD Economic Outlook and IMF International Financial Statistics.

so-called "efficiency wage" model has been touted as justifying this hypothesis, while the phenomenon of "hysteresis" has been used to justify calls for demand expansion. Influential papers on the two subjects have been written by Yellen (1984) and Akerlof and Yellen (1985) and by Blanchard and Summers (1986), respectively, while both developments have been drawn upon in a recent argument by Buiter (1987).

With regard to the efficiency wage idea, it is important to understand that this model does not itself rationalize any role for demand management. As Yellen (1984, p. 204) and Akerlof and Yellen (1985, p.825) recognize but do not emphasize, the model is concerned with the configuration of equilibrium employment and output magnitudes in relation to their socially optimal levels. These equilibrium quantities are determined in a block of the macroeconomic system that is exogenous to nominal variables, just as in the textbook classical model. Changes in nominal aggregate demand therefore result in price level changes, with no effect on output or employment.

To illustrate that point, consider the following version of the static classical model, in which the symbols are y = output, n = employment, n^S = labor supply, w = real wage, r = interest rate, g = real government purchases, M = money supply, and P = price level:

(1) $y = f(n)$ [Production function]

(2) $f'(n) = w$ [MPL condition]

(3) $n^S = h(w)$ [Labor supply]

(4) $n = n^S$ [Market clearing]

(5) $y = d(y,r) + g$ [IS function]

(6) $M/P = L(y,r)$ [LM function]

With M and g set by policy, the first four equations in this system determine w, y, n, and n^S with (5) and (6) then yielding r and P. Now the efficiency

wage model alters this system by replacing (4) with the condition that labor cost per efficiency unit is minimized, which can be expressed as

(4') $\eta(w) = 1$.

Also, (1) and (2) are replaced with

(1') $y = f(\phi(w)n)$

and

(2') $f'(\phi(w)n)\,\phi(w) = w$.

But with these changes, equations (1'), (2'), (3), (4') continue to determine w, y, n, and n^S. Changes in M or g then have effects only on r and P.[28]

In order to obtain an effect of M on n and y, Akerlof and Yellen (1985) replace w with W/P, divide firms into two types, and assume that one type does not change its nominal price and wage rates when M is altered.[29] This permits a fall in the average level of w, so has the effect of replacing (4') with a sticky-price condition. But of course an effect of M on w (and n) could have been obtained without the efficiency wage apparatus by directly adopting some sticky-price assumption in place of (4).[30] The principal role of the efficiency wage apparatus is to rationalize an assumption that the initial equilibrium is one with $n < n^S$. I find that suggestion dubious, but that is a topic for another paper.

Turning even more briefly to the topic of hysteresis, we find that a rather similar comment is applicable. In particular, acceptance or rejection of the hysteresis hypothesis--which suggests that the natural rate of unemployment adjusts upward or downward in response to past actual rates--has no bearing on whether aggregate demand policy can systematically influence the discrepancy between the two. In other words, if the specification of the wage-price sector is (is not) one that permits anticipated demand actions to affect the discrepancy in the absence of hysteresis, it will be one that does

(does not) imply such effects in its presence.[31] Furthermore, it needs to be noted that the presence of hysteresis would not be sufficient, for the reason mentioned in footnote 10, to contradict the natural rate hypothesis. And it should be kept in mind that empirical models designed to represent the hysteresis phenomenon (e.g., Blanchard and Summers, 1986, pp. 50-55) are observationally equivalent to expectational Phillips relations in which lagged as well as current unemployment measures appear. For these reasons, it is unclear that the concept of hysteresis is a crucial one in the context of demand management issues.

VI. Conclusion

It has been argued in this paper that, despite fifty years of active research, leading scholars continue to disagree about the need for, and potential efficacy of, activist demand management policy. In my opinion, this situation does not result entirely from ideological predilictions or obstinacy on the part of either group of scholars; it is exceedingly difficult to acquire firm knowledge about the workings of a dynamic system as complex as an economy when experimentation is infeasible. But whatever the reason, while it is likely that some activist measures could be useful, this cannot be concluded with complete certainty. And even if the case were firmly established that activist policy can in principle be useful, it would remain true that its workings depend upon features of the economy that have not been modelled in a reliable fashion.

In these circumstances, the paper suggests, a judicious way to conduct demand policy would be by adoption of a rule that promises to yield reasonably satisfactory results under a variety of assumptions regarding the nature of the economy's critical features. A particular rule designed in that spirit is here described--a semi-activist rule that would provide some stabilizing adjustments but in an automatic manner that should do no harm if such adjustments were unnecessary.

Footnotes

1. Even if the liquidity trap was empirically relevant, the real-balance effect would (as prices fall) automatically stimulate aggregate demand as needed. Some writers have questioned this standard conclusion on the grounds that it neglects dynamic considerations involving expectations. In McCallum (1983) it is shown, however, that with rational expectations and flexible prices the standard conclusion obtains when expectational dynamics are taken into account.

2. Reviews have been provided by many writers. A recent version of my own account appears in McCallum (1987).

3. For one example of this resurgence, see Blanchard (1987).

4. Even if it were the case that the government directly controlled real government purchases--its actual instrument is nominal government purchases--it would not follow that real aggregate demand could be manipulated, as reference to the textbook model of a classical (i.e., flexible price) system indicates. A qualification to this statement, mentioned below in footnote 28, does not overturn the point.

5. In particular, Hicks (1937), Modigliani (1944), and Patinkin (1956).

6. Prominent examples are Barro and Grossman (1976) and Malinvaud (1977).

7. It should be said that price adjustment relations are discussed in various places by Barro and Grossman (1976). But this part of their work has not been adopted by subsequent contributors to the disequilibrium literature, which Barro and Grossman have abandoned.

8. Ironically, this is the form of price adjustment postulated by Barro and Grossman (1976) in their Chapter 4.

9. As in my 1980 example.

10. This statement does not require that the natural-rate value be a constant over time, nor that it be trend-stationary or even independent of past unemployment rates. Also, the phrasing in the next sentence of the text is not meant to deny that different maintained inflation rates have different welfare implications, such as those discussed by Friedman (1969).

11. For elaboration and some examples, see McCallum (1983, pp. 400-401).

12. It is possible, however, that misperceptions of the type featured in Lucas's theory were of greater significance in the years before World War II, when aggregate data was not readily available.

13. But with agents assumed to possess knowledge of current monetary aggregates.

14. For postwar U.S. quarterly data, detrended by a specific smoothing filter.

15. If it is not, much of the impact and novelty of the RBC approach is lost.

16. Consider, for example, the effects of substantial but steady inflation on correlations between real variables as compared with correlations bwtween one real and one nominal variable. More analysis is needed, however, to determine the extent to which the suggestion in the text is valid.

17. Implications for fiscal policy are briefly mentioned below.

18. In the seasonally-adjusted quarterly U.S. data for 1954-85, the correlation is 0.81.

19. Here and elsewhere I refer to GNP rather than GDP as an American habit. The precise measure of nominal output/income to be used in the policy rule is an issue on which I mean to take no position. Gordon (1985) has suggested that nominal final sales would be better than GNP.

20. One reason is that it seems likely that official price indices overstate inflation to a small extent.

21. In the pre-World War II era, monetary authorities were kept from this type of behavior by the requirement of adherence to a commodity-money standard.

22. Or, to be more precise, a path growing at a rate equal to the economy's long-term average rate of output growth. Estimates of this rate could, if desired, be updated periodically in some specified manner. The monetary base, it might be mentioned, is a controllable variable since the central bank can read its value from its own balance sheet and make adjustments whenever required.

23. Note that $x_{t-1} - x_{t-17} - b_{t-1} + b_{t-17} = \sum_{j=1}^{17} (\Delta x_{t-j} - \Delta b_{t-j})$. This type of velocity correction was suggested by Meltzer (1987). The averaging period is set at four years since this term is not intended to pick up cyclical effects, but long periods would unduly slow the rule's response to non-cyclical institutional changes.

24. This huge RMSE value reflects average nominal GNP growth well in excess of 3%, i.e., reflects the inflation that was experienced.

25. Since drafting this paper, I have verified that the rule yields good results in four more VAR systems and in small "structural" models representative of three different theories of cyclical fluctuations, namely, the RBC theory, the monetary misperceptions theory, and a version of Keynesian theory as expressed in the MPS quarterly econometric model. Details are reported in McCallum (1988).

26. Even for a highly open economy the appropriate objective for macroeconomic policy is to keep nominal demand growing at a noninflationary rate. With regard to fiscal policy variables, one point is that the traditional automatic stabilizers provided by progressive tax schedules, etc., are helpful in promoting smooth growth of nominal GNP. Whether tax <u>rates</u> should be adjusted

in response to deviations of x_t is debatable.

27. The GPD growth measure is the ratio of nominal GDP for 1985 to its value for 1975. That different periods are used for the two variables can be explained as follows. The 1980-84 period is used for unemployment rates so as to focus on the greatly increased levels of the 1980's, with 1984 the concluding year because comparable data are not available for all countries for more recent years. In the case of demand growth, earlier years were included to take account of the possibility that effects occur with a substantial lag. The choice of precise dates is clearly quite arbitrary; it is my belief that the basic finding is not sensitive to this choice.

28. This statement should be qualified as follows. There would be real effects of changes in g if the model were modified to permit direct government employment and production. In such a case, however, changes in g would not strictly represent "demand management" actions.

29. They provide no justification for the assumption that these firms keep their nominal prices unchanged; one is attempted in McCallum (1986).

30. While his emphasis is very different, this conclusion is consistent with the analysis of Buiter (1987).

31. The "core inflation" case presented by Buiter (1987) is one in which anticipated demand influences are effective. This case provides an example of a specification in which the natural rate hypothesis does not obtain: an accelerating inflation will keep unemployment (expectationally) below the natural rate permanently.

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