NBER WORKING PAPER SERIES

ON "REAL" AND "STICKY-PRICE" THEORIES OF THE BUSINESS CYCLE

Bennett T. McCallum

Working Paper No. 1933

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 1986

Paper presented at Ohio State University on October 23, 1985, as the Money, Credit, and Banking Lecture for 1985. I am indebted to Michael Darby, Martin Eichenbaum, Marvin Goodfriend, Herschel Grossman, Robert King, Finn Kydland, Stephen McCafferty, Huston McCulloch, Edward Prescott, Matthew Shapiro, and Daniel Thornton for helpful discussions and comments. Partial financial support was provided by the National Science Foundation (SES 84-08691). The research reported here is part of the NBER's research program in Economic Fluctuations. Any opinions expressed are those of the author and not those of the National Bureau of Economic Research.

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ABSTRACT

This paper begins by identifying the distinguishing characteristic of the "real business cycle" (RBC) class of macroeconomic models. It then scruitinizes existing evidence, presented in support of the RBC approach, of three types: calibrated general equilibrium models with no monetary sector, vector-autoregression variance decomposition results, and univariate measurements of trend and cyclical components. It is argued that, in fact, these types of evidence have so far provided little support for the RBC hypothesis. Finally, with regard to an important alternative hypothesis concerning macroeconomic fluctuations, the paper proposes a partial rationalization for the stickiness of nominal product prices.

Bennett T. McCallum
Graduate School of Industrial
Administration
Carnegie-Mellon University
Pittsburgh, PA 15213

1. Introduction

It has now been 50 years since J.M. Keynes published his incomparably controversial General Theory of Employment, Interest, and Money. After a few apparent trends and a number of cycles in professional opinion, the macroeconomic debates of today have much in common with those of 1936 and the years that followed shortly after. In particular, with the recent downturn in popularity of the Lucas-Barro theory of cyclical fluctuations induced by monetary misperceptions,[1] the main competing explanations for these fluctuations are provided by the "real business cycle" and "sticky price" (or "nominal rigidity") types of models, which are rather strongly representative of Classical and Keynesian viewpoints, respectively. Allied with these two views, moreover, are sharply divergent notions concerning the nature of unemployment and the seriousness, in terms of individuals' welfare, of fluctuations in measured unemployment rates.

As part of the ongoing effort to achieve an understanding of the macroeconomic phenomena with which this debate is concerned, the present paper begins in Section 2 by characterizing the real business cycle class of theories and scrutinizing one type of evidence that has led some researchers to embrace this approach. Sections 3 and 4 are then devoted to somewhat longer discussions of two other types of evidence pertaining to the real business cycle hypothesis. Then in Section 5 the discussion turns to a leading problem for sticky-price theories, viz., the difficulty of rationalizing the abundance of contracts set in nominal terms. Finally, Section 6 includes some conclusions and reflections on the nature of macroeconomic fluctuations.

2. Real Business Cycle Models

Let us begin by stating explicitly what will here be meant by the real business cycle—henceforth, RBC—class of theories. In that regard, it seems clear that the distinguishing characteristic of RBC models is a denial that monetary policy actions have any significant impact on aggregate output and employment magnitudes. Admittedly, that hypothesis is not explicitly expressed in some of the significant papers in the RBC literature, and is possibly disbelieved by some of the main contributors. But if the class of models is distinctive enough to warrant a special label, it must have some distinguishing characteristic and there would seem to be no other contenders. There is in the literature a lot of emphasis on "propagation mechanisms"—sources of serial correlation in output or employment—but that is also true of earlier contributions such as Lucas (1975), Sargent (1979, Ch. 16), Blinder and Fischer (1981), and others that are not regarded as comprising RBC models. The fact that Lucas (1972), Barro (1976), and other rational-expectations models ignored serial correlation does not indicate that the authors believed such correlations to be nonexistent. The reason, rather, was that they wanted to concentrate, without severe distractions, on the single issue that seemed most interesting and difficult—why the aggregate data exhibited a Phillips relationship, i.e., a positive association between output/employment levels and the rate of change of nominal magnitudes such as the money stock. Emphasis on propagation mechanisms, to return to the point, does not provide a line of demarcation between RBC and other classes of models; denial of monetary effects does.

The RBC point of view does not deny, of course, that there is any association between output and monetary magnitudes. But it attributes the observed money-output correlation to so-called "reverse causation," i.e., responses of the money stock, via the monetary authority and/or the banking sector, to variations in aggregate output. Thus, the RBC theories in effect claim that observed Phillips-type correlations stem from the monetary system's reaction to output fluctuations that are induced by real shocks to tastes or technology—not from the non-bank private sector's reaction to monetary shocks.

Encouragement to the adoption of the RBC view has come from both theoretical and empirical studies. With respect to the former, this encouragement has been primarily negative—involving disenchantment on theoretical grounds with both of the leading alternative theories, the monetary misperception theory of Lucas and Barro on the one hand and the price-stickiness or nominal-contract approach of Fischer (1977), Taylor (1980), et. al. on the other hand. Of equal importance, however, has been the compilation of statistical evidence that appears—at least on the surface—to support the idea that monetary shocks have no significant output/employment effects. In this regard, three major types of evidence have been provided. First, there are the studies of Sims (1980) (1982) and Litterman and Weiss (1985) which show that there is very little explanatory power for output variations provided by money stock <u>innovations</u> in vector autoregression (VAR) systems when nominal interest rates are included among the system's variables. Second, there is the notable study of Kydland and Prescott (1982), which shows that several business-cycle correlations can be mimicked reasonably well with a competitive equilibrium model in which neither money nor govenment policy plays any role whatsoever. Finally, there is a line of argument developed primarily by Nelson and Plosser (1982) that relies entirely on the univariate time-series properties of aggregate output, employment, and other real variables. Briefly, the Nelson-Plosser argument is that most of the fluctuations in these variables should be attributed to the trend component, in a trend vs. cyclical decomposition, which would presumably be unaffected by monetary shocks.

In this section and the two that follow, I will argue that in fact none of these types of evidence actually provides much support for the RBC position: the statistical results that have been interpreted as favorable evidence are actually just as consistent with other models as they are with RBC models. To demonstrate such consistency does not, furthermore, require tortuous analysis relying upon highly indirect effects of questionable magnitude. In the process of developing this argument, I will suggest that there is presently in existence evidence that, while inconclusive, is awkward for the RBC class of models.

Let us begin, because of the brevity of the necessary discussion, with the evidence provided in the much-discussed study of Kydland and Prescott (1982). As mentioned above, this study demonstrates that it is possible to match several important features of actual postwar U.S. quarterly data with a model that includes no monetary (or government) sector—indeed, no nominal variables. The model is a one good, representative household, competitive equilibrium model in which intertemporal non-separability of preferences and investment gestation lags are quantitatively important. The only source of cyclical fluctuations is a technology shock--a random disturbance to the aggregate production function--that is composed of white-noise and autocorrelated components in a mix that cannot be observed by the agents (households and firms).[2]

The sense in which the fluctuations implied by the Kydland-Prescott model match actual U.S. data is as follows. With parameters estimated by means of a minimum-distance estimator with a metric that is unconventional[3]--the authors term the estimation procedure "calibration"--variances and correlations with output are calculated for several variables (consumption, investment, inventory stocks, manhours employment, etc.) and compared with actual U.S. quarterly values for 1950-79. The same is also done for output autocorrelations at lags one through six. These comparisons are reasonably favorable to the model, although Altug (1985) has shown that its fit is not good enough to avoid strong rejections when subjected to formal hypothesis testing.[4]

The results of Altug are not, however, the basis for my suggestion that the Kydland-Prescott model provides little if any evidence in favor of the RBC hypothesis. My reasons are twofold. First, there are no tests carried out or proposed of the proposition that addition of monetary variables would not significantly improve the model's explanatory power. Second, and more importantly, the Kydland-Prescott results do not show that technology shocks are adequate to generate output, employment, etc. fluctuations of the magnitude actually observed. Instead, as Lucas (1985) has noted, Kydland and Prescott "simply choose the variance of the technology shock so as to be consistent with the observed GNP variability."[5] Consequently, if someone believes that the variance of actual technology shocks is only 1/10 (say) as large as the value implied by the Kydland-Prescott model, he will find nothing in the Kydland-Prescott results that would require him to alter this belief.[6]

3. Vector Autoregression Studies

Of the various studies under discussion, the first to appear in print was that of Sims (1980), which was followed by Sims (1982). In these papers Sims begins by estimating VAR systems that include among their variables measures of aggregate production and the money stock.[7] He then solves for the implied moving-average representations and finally uses the latter to decompose the variance of each variable into portions attributable to the innovations of each of the system's variables.[8] It transpires that when a system including only money, output, and the price level is examined using postwar U.S. data, the money stock innovations contribute a substantial fraction of the total explanatory power for output.[9] But when some nominal interest rate is added to such a system, the fraction of output variability attributable to money stock innovations declines sharply—to 4% and 14% in the two cited studies. The interpretation put forth by Sims is that irregularity in monetary policy behavior has not been an important source of postwar fluctuations in aggregate output. In Sims's words, "monetary policy surprises are not important in explaining the real component of postwar business cycles," so that "imposition of a monetarist rule to make the quantity of money more predictable would have had little real effect" toward reducing these fluctuations (Sims, 1980, p. 253).[10]

It is my contention that this conclusion is not warranted by the reported findings. As I argued in McCallum (1983), it is not valid in this context to use money stock innovations to represent the surprise component of monetary policy actions. The basic reason is that during only a part of the period studied has the Fed paid attention to money stock targets, and in that part it has utilized operating procedures that permit money stock control only by way of interest rate manipulations. Thus to decrease the rate of M1 growth the Fed would use open-market operations to increase the federal funds rate, this increase affecting the money stock by reducing the quantity of money demanded. But with this type of operating procedure, irregular components of monetary policy behavior—unsystematic actions by the monetary authority--will show up as innovations in the VAR system's interest rate, in addition to (or instead of) its money stock.[11] Consequently, to conclude that irregular behavior by the Fed was not contributing to output fluctuations, it would have to be shown that neither money stock nor interest rate innovations had appreciable explanatory power for output. But this is not the case in Sims's data; the interest rate innovations tend to pick up the explanatory power lost by the M1 innovations when the former variable is added to the system.[12]

A related type of consideration, it should be added, is applicable to studies that focus on the monetary base. While the Fed could use the base (or total reserves) as its operating instrument if it chose to do so, in fact it has not. Even during the so-called "monetarist experiment" of 1979-82, the Fed operated in a way that amounts to an indirect usage of the federal funds rate as its instrument, with the base then adjusting endogenously (within the intermeeting control period) in response to shocks.[13] Consequently, empirical analyses—such as King and Plosser (1984)—built on the assumption that the base has been used as the Fed's instrument (for implementing monetary policy) are inappropriately designed and therefore unlikely to yield results that are useful in measuring the impact of actions by the monetary authority.

There are some similarities between the Sims findings and those in a notable recent study by Litterman and Weiss (1985). In particular, the latter authors also find that the portion of output variance attributable to money stock innovations declines sharply when a nominal interest rate is added to a small VAR system, and they also discuss matters as if the money stock were directly controlled by the Fed. They carry out a considerable amount of additional analysis, however, much of which concerns movements of the (ex ante) real rate of interest. This variable is unobservable, of course, but Litterman and Weiss are able to test various hypotheses concerning its behavior by application of cross-equation restrictions on the VAR system, restrictions that are implied by the definition of the real rate (at period t) as the nominal rate less the rationally predicted inflation rate (between t and t+1). Here "rational" means the forecast value implied by the VAR system.

One of the more prominent findings in the Litterman-Weiss paper is that the real rate r_t is not significantly Granger-caused in the quarterly U.S. data by any of the other variables involved, which are M_t(log of money), Y_t(log of output), P_t(log of price level), and R_t(the nominal interest rate). On the basis of this finding, Litterman and Weiss suggest that theories of the Lucas-Barro and sticky-price types are contradicted by the data, as both transmit monetary impulses to real variables by way of the real rate. The impact of this suggestion is weakened, but not eliminated, by the non-equivalence of (i) the absence of Granger-causality from other variables to r_t and (ii) the exogeneity of r_t—the latter requiring the former and also the absence of within-period effects from other variables to r_t.

Other results reported in the Litterman-Weiss paper appear, upon first consideration, to provide evidence that is literally inconsistent with the RBC hypothesis. In particular, as the hypothesis implies that real variables (excepting real money balances) are block exogenous, it would appear that a finding that Y_t (and/or r_t) is Granger-caused by any nominal variable would require rejection.[14] And figures in the Litterman-Weiss Table VII indicate that Y_t is in fact Granger-caused by nominal variables. For example, line 18 on p. 152 shows that the hypothesis, that Y_t is explained only by past values of itself and r_t, can be rejected at the marginal significance level 0.0013—i.e., is very strongly rejected.[15] Since the system's other variables are M_t, P_t, and R_t, it then follows that the log of output is strongly Granger-caused by some nominal variable (or variables).

Litterman and Weiss provide an example, however, which demonstrates that an empirical finding of Granger-causality from nominal to real variables does not actually imply that the latter set fails to be block-exogenous to the former. This possibility, also mentioned by Eichenbaum and Singleton (1986), can arise if the true system includes an important variable that is unobservable to the econometrician and is consequently omitted from the empirical analysis. In the Litterman-Weiss example, there is such a real variable Z_t that affects <u>future</u> output, Y_{t+1}. But Z_t is also correlated with the current nominal interest rate R_t, so study of a system omitting Z_t will indicate that R_t Granger-causes Y_t even though it does not when all relevant variables are recognized. Thus the anti-RBC evidence discussed in the previous paragraph could be spurious.[16]

Indeed, Litterman and Weiss go on to present evidence that they claim to be supportive of this foregoing interpretation. The basis of their argument is the absence of Granger causality from other variables to a vector consisting of r_t, Y_t, and $\pi^\star_t$, the last of which is "that component of the expected inflation innovation [that is] orthogonal to the contemporaneous innovations in the real variables" (1985, p. 147). But while this finding is consistent with their example, it does not actually imply a structure of the RBC type: it is also consistent (for example) with a direct dependence of Y_t on lagged values of nominal variables. All in all, then, the Litterman-Weiss evidence neither supports nor contradicts in a convincing way the RBC point of view.

4. Trend and Cyclical Components

In this section we continue by discussing the third type of pro-RBC evidence mentioned above, not only for completeness but also because the argument is itself of considerable interest. This argument, which was developed initially by Nelson and Plosser (1982),[17] consists of two parts. The first of these concludes, on the basis of statistical considerations to be scrutinized below, that fluctuations in the cyclical component of aggregate output (or employment) are small in comparison with fluctuations in the trend component of that variable. The second part relies on the presumption that "monetary disturbances have no permanent real effects" (1982, p. 159) and so can contribute only to the cyclical component. In that case the maximum extent of monetary effects on output (or employment) is limited by the variability of the cyclical component, and since that is small—according to the first part of the argument—it follows that output variability due to monetary fluctuations must be small.

Now in principle one could object to the second half of this argument, basing his objection on the theoretical possibility of a "Tobin effect" of sustained inflation on the steady-state capital stock.[18] But even if one accepts the assumption that such effects are quantitatively unimportant, he need not accept the overall Nelson-Plosser argument, for the first part is also debatable. Indeed, the remainder of this section will be devoted to the counterargument that it is not in fact possible to determine, in the manner suggested by Nelson and Plosser (1982),[19] that cyclical contributions to observed fluctuations in real variables are small.

It will be useful to begin by reviewing the part of the Nelson-Plosser argument concerning cyclical variability in some detail. To that end, consider an observable variable y_t, such as the log of real GNP, whose values can in principle be decomposed according to

$$(1) \qquad y_t = \bar{y}_t + c_t,$$

where $\bar{y}_t$ and c_t represent unobservable "secular" and "cyclical" components, respectively.[20] On the basis of a priori understanding of what is meant by a cyclical component, Nelson and Plosser take it as given that c_t is generated by a process that has the property of stationarity. That presumption, which will be retained here, then implies that any nonstationarity (such as a trending mean) in y_t must be attributed to the secular component $\bar{y}_t$.

For the next step in their argument, Nelson and Plosser take it to be an established fact—established by their empirical investigation of several relevant U.S. data series—that the y_t variable under discussion is generated by a process of the "difference stationary" or DS class. In other words, they take as given the hypothesis that y_t is a variable whose ARMA (i.e., autoregressive-moving average) representation includes a unit root in the AR polynomial and no deterministic trend.[21] Since c_t is stationary, it then follows that the secular component $\bar{y}_t$ must have a unit root in the AR polynomial of its ARMA representation. Consequently, the decomposition (1) can be expressed as

(2) $y_t = (1-L)^{-1}\ \theta(L)v_t + \psi(L)u_t$

where v_t and u_t are white noise shocks driving the secular and cyclical components, respectively, and where $\theta(L)$ and $\psi(L)$ are polynomials in the lag operator L that satisfy conditions for stationarity and invertibility. Also, the meaning of $(1-L)^{-1}$ in (2) is as follows: $(1-L)^{-1}\ x_t = x_t + x_{t-1} + \ldots$.

In addition, Nelson and Plosser also utilize the fact—established by their evidence—that the differenced series Δy_t is (for many of the variables examined) appropriately represented as an invertible first-order MA process with a MA parameter that is positive and smaller than 1.0. In this case, since (2) implies that

(3) $(1-L)y_t = \theta(L)v_t + (1-L)\psi(L)u_t$,

the first-order MA character of $(1-L)y_t = \Delta y_t$ requires that $\theta(L) = 1 + \theta_1 L$ with $0 < \theta_1 < 1$ and also that $\psi(L) \equiv 1$. In other words, under the stated restrictions (3) can be specialized to

(4) $\Delta y_t = v_t + \theta_1 v_{t-1} + u_t - u_{t-1}$.

Finally, in this particular case the first autocorrelation coefficient for Δy_t is related to θ_1 and the variances of u_t and v_t in a fashion[22] that permits the conclusion to be drawn that (with this autocorrelation positive) σ_v^2 must be unambiguously larger than σ_u^2. Indeed, the addition of some plausible side assumptions gives rise to the implication that σ_v^2 is several times as large as σ_u^2. In this sense, then, Nelson and Plosser find that "the variance in actual output changes is dominated by changes in the secular component $\bar{y}_t$ rather than the cyclical component c_t" (1982, p. 155).

Now the foregoing argument is ingenious and rather appealing, but consideration indicates that it includes a link that is both crucial and weak. The link in question is the hypothesis that y_t is generated by a process of the DS class, i.e., that its ARMA representation involves a unit root in the AR polynomial and no deterministic trend component. That this hypothesis is crucial for the specific conclusion $\sigma_v^2 > \sigma_u^2$ is clear, for without that hypothesis one is not led to the special representation (2) which, when constrained by the evidence concerning autocorrelation magnitudes, yields the implication $\sigma_v^2 > \sigma_u^2$. Also, the hypothesis is critical more generally (in the context of cyclical/secular decompositions) in that it provides the basis for the Nelson-Plosser and Stulz-Wasserfallen contentions that the extent of cyclical movement is overestimated by typical trend-removal methods. An illustration of its importance is presented below.

At this point, consequently, what needs to be explained is the sense in which the DS hypothesis is dubious and therefore constitutes a weak link in the Nelson-Plosser argument. Let us then consider in turn each of the three types of evidence in favor of the DS hypothesis presented by Nelson and Plosser. The first bit of evidence is simply that sample autocorrelations for annual levels of y_t variables such as (the log of) real GNP are large and decay slowly. But while that autocorrelation pattern is entirely consistent with a random walk, it is also consistent with the behavior of a TS (trend stationary) autoregressive series with a root close to 1.0. More interesting perhaps is the second type of evidence regarding autocorrelations of annual differences (Δy_t values). For the Nelson-Plosser variables, these autocorrelations "in each case are positive and significant at lag one, but in many cases are not significant at longer lags" (1982, p. 147). Thus, for example, the first six autocorrelations for Δy_t with y_t denoting the log of real GNP are as follows: 0.34, 0.04, -0.18, -0.23, -0.19, 0.01. Now certainly that sort of pattern is reasonably well modelled by the first-order MA process adopted by Nelson and Plosser. In the GNP case, for example, the autocorrelation pattern is well matched by the process

$$(5) \qquad \Delta y_t = \gamma + \epsilon_t + 0.3\,\epsilon_{t-1}$$

where ϵ_t is white noise. But the pattern in question would also be well matched by the process

$$(6) \qquad y_t = 0.98y_{t-1} + \mu + .02\gamma t + \epsilon_t + 0.3\epsilon_{t-1}$$

and would not be too badly matched by

$$(7) \qquad y_t = \mu + .02\gamma t + 1.28y_{t-1} - 0.3y_{t-2} + \epsilon_t,$$

both of which are obviously of the TS class. The point, of course, is the elementary one that one cannot establish with any degree of certainty that a series is of the DS class simply by inspection of the autocorrelation functions for its levels and differences.

For precisely that reason, Nelson and Plosser (1982, pp. 150-2) also offer, as a third type of evidence, formal tests—based on procedures of Dickey and Fuller (1979) (1981)—of the hypothesis that the AR polynomial for the y_t variable contains a unit root. As it happens, for each of the y_t variables examined (except the unemployment rate) the Dickey-Fuller test does not call for rejection of the hypothesis that a unit root obtains. But that fact is far from conclusive, for the reported test statistics would also obviously result in non-rejection if the tested hypothesis were instead that the relevant parameter is of value 0.98 (as in (6)) rather than 1.0. Indeed, the Monte Carlo results reported in Nelson and Plosser's Table 1 indicate that (with a sample size of T = 100) standard deviations of the relevant test statistic are of the order of magnitude of 0.05. Consequently, with a significance level of 0.05, non-rejection would also be forthcoming for tests of hypotheses such as $\rho_1 = 0.95$ or even $\rho_1 = 0.90$.[23]

Of course Nelson and Plosser are very well aware of the inability of finite-sample test procedures to distinguish conclusively between DS and TS series; they "recognize that none of the tests presented, formal and informal, can have power against a TS alternative with the AR root arbitrarily close to unity" (1982, p. 152). What they do not mention, however, is the crucial role of conclusions regarding DS vs. TS processes in their overall line of argument. This argument builds upon a decomposition of series into secular and cyclical components with the latter required to be stationary. The cyclical component is then measured by whatever is left over after an estimate of a (DS) secular component—in practice, a random walk—is removed. But if the process under study is actually one of the TS class with an AR root close to unity, then the secular-component removal step can easily take out many times as much of the signal as is properly attributable to the secular component, thereby yielding a many-fold underestimate of cyclical variability. The procedure relies upon an accurate determination of the variability provided by a component of the DS class, even though it is in fact infeasible (in samples of the relevant size) to distinguish between variability resulting from a unit root and variability associated with a root close to, but distinct from, 1.0.

For an (extreme) illustration of this point regarding the removal of variation approximated by a random walk, consider the first-order AR process

(8) $y_t = \alpha y_{t-1} + \epsilon_t$

where ϵ_t is white noise with variance denoted σ^2. In this case if α is close to 1.0 the process is close to a random walk, but with $|\alpha| < 1.0$ it is nevertheless stationary—there is no DS component. But suppose that a researcher models the series as a random walk $\Delta y_t = \xi_t$, ξ_t white, and uses the residual variance $V(\xi_t)$ as his estimate of the variance of the stationary component. Then since

(9) $\xi_t = y_t - y_{t-1} = \alpha y_{t-1} + \epsilon_t - y_{t-1} = \epsilon_t - (1-\alpha)y_{t-1}$

we see that

(10) $V(\xi_t) = \sigma^2 + (1-\alpha)^2 V(y_t)$.

But of course $V(y_t) = \sigma^2/(1-\alpha^2)$ in this first-order AR case so

$$(11) \quad \frac{V(\xi_t)}{V(y_t)} = \frac{\sigma^2}{\sigma^2/(1-\alpha^2)} + (1-\alpha)^2$$

$$= (1-\alpha^2) + (1-\alpha)^2 = 2(1-\alpha).$$

If then $\alpha = 0.98$, for example, $V(\xi_t)/V(y_t) = 2(1-.98) = 0.04$. In other words, the estimated variance of the stationary portion is only 1/25 of its true magnitude. Even with a less extreme α value of 0.9, the estimated variance is only 1/5 of the true value.[24]

For many purposes, the practical impossibility of distinguishing between a random walk and a process such as (8) with α close to 1.0 is of no great consequence. If the object were to forecast near-future y_t values, for example, the predictions would be essentially the same whichever of the two representations was selected. In such cases, there is much to be said for parsimoniously setting $\alpha = 1$ and using the random walk model. But if by contrast the purpose is to estimate the forecast <u>variance</u> for the level of the series 100 periods in the future, the choice between $\alpha = 1.0$ and (say) $\alpha = 0.98$ becomes critical[25] and basing this choice on the principle of parsimony cannot be acceptable. The same is true, it would appear, when the purpose of one's study is to decompose a series into cyclical and secular components.

The foregoing analysis does not, it should be emphasized, constitute a claim that macroeconomic series such as (the log of) real GNP are members of the trend-stationary class. The claim is only that the time series evidence reported by Nelson and Plosser (1982), and likewise that developed by Stulz and Wasserfallen (1985), is inadequate to determine whether the relevant series are of the DS or TS class. This evidence itself, then, sheds little or no light on the issue of the relative variability of cyclical and secular components of typical macroeconomic time series—and consequently provides little or no support for the RBC hypothesis.[26]

5. Nominal Price Stickiness

In the preceding sections it has been argued that the main types of evidence presented to date on behalf of the RBC hypothesis actually provide very little support. Furthermore, there exists evidence that is to some extent damaging to that hypothesis. But suppose that we accept the alternative view that monetary policy actions do have significant effects on aggregate output and employment. In that case, the question remains: what theory or class of theories provides a satisfactory explanation for this influence?

In previous papers (1980) (1982) I have described a type of model that seems to be at least qualitatively consistent with the main facts.[27] The simple model that I have used to illustrate the type is one in which prices are sticky—indeed, formally rigid—within each period but adjust between periods in a manner that respects the natural-rate hypothesis.[28] It warrants emphasis that the relevant nominal stickiness in this model pertains to product prices, not wages. If wage stickiness alone was responsible for the real effects of monetary actions, with product prices adjusting flexibly, then we should observe countercyclical movements in the real wage. That we do not has recently been reconfirmed in a study by Bils (1985).

But whether in wages or prices, it remains a problem to explain why nominal stickiness exists. That agents' utility pertains to real rather than nominal magnitudes is perhaps the most fundamental axiom of neoclassical economics--its negation would destroy existing microeconomic theory--and it seems implausible that the actual resource costs of changing price tags are of significant magnitude. So what is it that accounts for nominal price stickiness?

Before attempting a partial answer, let us pause to recognize that a sizeable literature has accumulated in which multiperiod contracts between buyers and sellers are endogenously explained as the outcome of optimizing behavior by rational agents. But there are two aspects of the relevant contractual arrangements that are crucial for the issue at hand: first, that arrangements are made in advance of actual exchanges and, second, that these arrangements involve exchange ratios (prices) expressed in nominal terms. Virtually all of the existing research in the area--most of which emphasizes risk and/or informational imperfections and asymmetries--is concerned with only the first of these aspects, and thus in effect seeks to explain contracts specified in real terms. These papers leave unanswered, accordingly, the question of why contracts or prices are in so many cases preset in nominal terms. They do not, in other words, explain why sellers who set money prices in advance of sales do not make these prices contingent upon movements in some general price index--why there is in this sense so little "indexation" or, in the terminology of Eden (1983), "linkage."[29]

Some writers on the subject have suggested that an important reason for the paucity of linkage in the U.S. economy is that buyers and sellers would usually prefer to link to different price indices (or nominal aggregates such as the money stock). Blinder (1977, p. 70), for example, has put the argument as follows:

> I suggest that risk-averse firms would be happy to link factor payments to a price index which follows closely the movements of their own output prices, but shy away from contracts linking wages ... to some broad index whose movement might easily outstrip their selling prices. Conversely, workers ... may be unwilling to bear the substantial risks of linking their factor payments to the prices of firms for which they work .... [and] prefer linkage to a broad price index more or less representative of the things they buy.

But that argument evidently misses the point, for it explains why buyers and sellers might have trouble in agreeing what the best of all possible indices would be, but not why they fail to agree to link to one of the obvious candidates—e.g., the CPI. Failing to link to any such index is equivalent, it should be noted, to linking to the particular (degenerate) index whose value is constant over time. But why choose that index in preference to the CPI? It seems highly implausible that the constant index could do a better job than the CPI of eliminating risks for that vast majority of contracts that implicitly use it. The constant index is in principle preferable to the CPI only for those agents whose most-preferred index is <u>negatively</u> correlated with the CPI.[30]

An approach[31] that seems more promising begins with the observation that in most actual economies the medium of exchange is also used as the medium of account. Although the latter function could in principle be served by some commodity or commodity bundle other than money (the medium of exchange), it usually is not.[32] In a typical monetary economy, then, a seller who quotes prices in units other than the monetary unit of account forces potential buyers either to convert those prices into money prices or to agree to a bargain expressed in unfamiliar terms. Either way, this seller imposes some extra informational costs onto the buyer. Now these costs are quite small in magnitude, obviously, and would be willingly accepted by the buyer if they came along with other advantages. But the only advantage that is necessarily associated with these computational costs is the reduction in risk that is provided by the indexation. And in many cases, the value to the buyer of the maximum possible reduction in risk will be exceedingly small—even smaller than the value of the extra computational cost.

To develop credibility for this last assertion, I will proceed by posing the following question for the reader: do you personally have an indexed salary agreement with your employer? If not, why not? In my own case, the first answer is "no" and the second answer is "because it seems pretty unimportant." More specifically, my Dean and I both understand about the effects of inflation on nominal salary agreements and we both know about the guesses of economic forecasters concerning the likely course of inflation over the next year. So each year's nominal salary that we agree upon will reflect the mean of the distribution of the random variable "next year's inflation," that is, the "expected inflation rate." It is only the uncertainty concerning this rate that provides any reason for indexation. But how much uncertainty is there regarding the average inflation rate over the next year? To me it seems reasonably certain that the realized value will lie within $\pm$ 2 percentage points of the expected rate. How much, then, would I be willing to pay for insurance to remove that amount of price level uncertainty? The answer is certainly not zero, but it is also small enough that I do not bother to even raise the issue with my Dean—and my guess is that the same is true for most readers.[33]

But the crucial point is this: whatever the value of insurance against nominal risk in one's salary over a year, for the price of most products that one purchases—books, phonograph records, sacks of coffee beans, boxes of pasta—the value of insurance against nominal fluctuations must be vastly smaller, probably two or three orders of magnitude. Consequently, I would be willing to pay an insurance company exceedingly little—less, say, than 1/100 of one dollar—to insure me against the risk associated with unanticipated inflation effects on coffee bean prices over the next quarter-year. Indeed, the value to me of such insurance is less than the value of the computational costs that I would have to bear if my coffee bean supplier were to price his merchandise in indexed terms. I would rather, that is, that he simply post dollar prices for one-pound sacks of Columbian Supremo. My guess, then, is that the same is true for most buyers of most products. Sellers, accordingly, respond to their customers' preferences by offering products priced in terms of unindexed dollars.[34]

It is important to note that this argument does not predict that there will be no linkage or indexation. On the contrary, it suggests that the benefits of linkage would outweigh the costs in the case of large contracts of long duration—mortgages, for instance. One-year wage contracts are perhaps close to the magnitude/duration combination that would make indexation worthwhile. But for most final products sold to consumers, it seems clear that the potential benefits of indexation are even smaller than the computational cost due to the expression of prices in unfamiliar units.

It must be emphasized that the foregoing argument pertains only to the second aspect of the nominal price stickiness puzzle, as described on p. 19.

Thus it attempts not to rationalize the existence of price stickiness, but to explain why any such stickiness that prevails could plausibly be in terms of nominal prices. Our conclusion, however, is that there is a good reason to believe that final product prices can rationally be expressed in nominal terms. That implies that some analyses explaining the predetermination of prices—the first aspect of the puzzle—may be reasonably interpreted as pertaining to nominal prices, even if these analyses as developed are logically applicable to the real terms of exchange arrangements. Some such analysis[35] may, then, in combination with the foregoing argument, provide a satisfactory theoretical rationalization for real macroeconomic responses to monetary actions.[36]

6. The Cost of Cyclical Fluctuations

As a final topic, I would like to comment briefly on a rather striking proposition recently developed by Lucas (1985) concerning the relative unimportance of business cycle fluctuations. In particular, by adopting some plausible assumptions concerning individuals' preferences, Lucas was able to relate the utility cost of consumption variability for a representative household to the utility effect of a permanent increment to lifetime consumption. Based on the magnitude of U.S. consumption fluctuations over the postwar period, his conclusion is that "eliminating aggregate consumption variability entirely would ... be the equivalent in utility terms of an increase in average consumption of something like one or two tenths of a percentage point" (1985, p. 19). Furthermore, Lucas continues with the following: "I want to propose taking these numbers seriously as giving the order-of-magnitude of the potential marginal social product of additional advances in business cycle theory. Or more accurately, as a loose upper bound—since there is no reason to think that eliminating all consumption variability is either a feasible or a desirable objective of policy."

Now, as Lucas emphasizes, this is a number that may seem startlingly small to many of us who are accustomed to think of macroeconomic fluctuations—and stabilization policies—as of great quantitative importance for human welfare.[37] Accordingly, I want to conclude by asking whether the cited estimates by Lucas are convincing.

More specifically, we need to consider whether Lucas's estimates are dependent upon any special assumptions concerning the source of the business cycle or the type of economy in which the typical household resides. At first glance, it would appear that there are no such assumptions involved, for the only aspect of the economy that Lucas even discusses in deriving these estimates is the utility function of the representative household. In his words, the calculations are generated "without saying much more about the nature or workings of the economy than ... [that] an economic system is a collection of people and serious evaluation of economic policy involves tracing the consequences of policies back to the welfare of the individuals they affect" (1985, p. 21). And indeed the procedure is almost model-free. But there is one assumption built into the argument that warrants explicit mention. That is the assumption that cycles are generated by a process which keeps fluctuations around some reference path and the level of that path entirely separate. Stabilization policy, consequently, is by assumption unable to affect the average level of aggregate consumption. Now to me that is an attractive assumption—it is a variant of the natural-rate hypothesis mentioned above—but it must be recognized that it is not innocuous in the context of the present issue. If cyclical fluctuations were generated by an economy of the type depicted by Barro and Grossman (1976), for example, then well-executed stabilization policy could enhance the average level of consumption[38] and thereby overturn Lucas's comparisons. The assumption is a highly substantive one, not merely a matter of convention or terminology.[39]

Throughout, this paper has taken positions that must be regarded as more on the "Keynesian" than the "classical" side of the issues at hand. As this is something that makes me uneasy, I would like to point out in conclusion that the specific positions taken here do not constitute endorsement of typical macroeconomic analysis of the pre-rational-expectations variety. The two main failings of the latter were (i) reliance on models that imply permanent output-inflation tradeoffs and (ii) emphasis on point-in-time policy analysis of a type that permits dynamic inconsistency. Acceptance of the arguments of the present paper does not entail approval of either of those practices.

Footnotes

1. This downturn is mainly due to the implausibility of the theory's critical assumption that individual agents are unable to observe current nominal magnitudes. For more discussion, see McCallum (1982) and references therein. It should be said that implausibility under today's conditions does not imply that ignorance of nominal aggregates was not important in the past. Availability of macroeconomic data is now much greater than before World War II.

2. For a helpful exposition and insightful discussion of the Kydland-Prescott model, see Lucas (1985). A recent paper by Prescott (1986) is also germane.

3. And not clearly described.

4. Kydland (1984) has explored elaborations that improve performance in some ways, but do not bear on the problem discussed in the next paragraph.

5. This they can do, of course, because of the unobservability of technology shocks.

6. Elementary calculations show that the Kydland-Prescott technology shock has an unconditional standard deviation of 0.029 (quarterly data). More recently, Prescott (1986) has reported on some efforts to obtain an independent measure of the magnitude of technology shocks. The procedure is essentially that of attributing production function residuals to "technical change," as in the growth accounting literature. In that literature, however, it is usually presumed that the use of unadjusted capital and labor inputs will result in a severe overestimate of the effects of technical change (see, e.g., Dennison 1962).

7. To be more precise, Sims uses industrial production or real GNP and M1 as his measures.

8. The innovation of a variable is the one-step-ahead prediction error implied by the VAR system. The decomposition in question can only be accomplished by "orthogonalizing" the innovations, which is in principle unsatisfactory as there are various possibilities for orthogonalization and the choice among them is arbitrary. But in practice Sims ameliorated this difficulty by presenting results based on the orthogonalization that was most unfavorable to his argument.

9. The figures are 37% in the monthly-data study in Sims (1980) and 36% in the quarterly-data version in Sims (1982). These values are based on 48-month and 14-quarter forecast horizons, respectively.

10. It should be kept in mind that, since all influences are attributed in this innovation-accounting framework to the innovation in <u>some</u> variable, Sims's argument also rules out substantial effects from non-surprise monetary fluctuations.

11. An explicit example, illustrating this point, is worked out in McCallum (1983).

12. In Sims (1980), the interest rate innovations account for 30% of the explanatory power for industrial production—corresponding closely to the fall from 37% to 4% for the M1 innovations. In Sims (1982), the interest rate figure is 19% while the M1 fall is from 36% to 14%.

13. For a detailed argument and additional references, see McCallum (1985).

14. It should be noted that Granger causality evidence is potentially appropriate in this context, even though it is not in tests of the "policy ineffectiveness" proposition that received so much attention during the late 1970s. The difference arises because the RBC hypothesis is more stringent: while the policy ineffectiveness proposition contends that only surprise movements in nominal variables have effects on real output and employment, the RBC hypothesis rules out even this effect.

15. For subperiods of the 1949.2-1983.2 sample, rejections are obtained at marginal significance levels of 0.0087 and 0.0033. See the portions of their Table VII that appear on Litterman and Weiss's pages 153-154.

16. The impact of that evidence is also considerably weakened by the fact that it pertains to data that has not been detrended in any way. Eichenbaum and Singleton (1986) show that the extent of Granger-causality from nominal to real variables in the postwar U.S. data is reduced by the removal of a linear trend from the logarithmic variables, and is virtually eliminated when first differences are used instead of log levels. In this regard, however, the argument of Section 4 below is relevant.

17. It has subsequently been utilized and/or developed further by King and Plosser (1984), Wasserfallen (1984), Stulz and Wasserfallen (1985), and Nelson (1985).

18. Nelson and Plosser (1982, p. 159) recognize this theoretical possibility but assume that it is not of practical importance.

19. And also Stulz and Wasserfallen (1985).

20. For simplicity, let us abstract from the possible presence of a seasonal component.

21. This class is contrasted with that of the "trend stationary" (or TS) type, whose members include deterministic trend components (functions of calendar time) and no autoregressive unit roots. It should be noted that these two classes are not exhaustive.

22. In particular, the first autocovariance equals $\theta_1\sigma_v^2 - (1-\theta_1)\,\sigma_{uv} - \sigma_u^2$.

23. Here ρ_1 is used, as in Nelson and Plosser's equation (12), to denote the relevant parameter.

24. That there is no trend in this example is of no consequence for the point at hand. If the variable y_t reflected measurements relative to a linear trend, for instance, with ξ_t being the deviation from a random walk with drift, then the same results would be obtained.

25. Here I am neglecting coefficient uncertainty of the type stressed by McCulloch (1985).

26. It might be conjectured, moreover, that there is no purely statistical procedure that will reliably discriminate between DS and TS series. If that conjecture is correct, the task of understanding the extent of cyclical variability—and whether it stems from monetary policy or other sources—will have to rely upon the interaction of statistical analysis with substantive economic theorizing, difficult and controversial though that path may be.

27. The four critical facts listed in McCallum (1982) are as follows: (i) output and employment magnitudes exhibit significant persistence; (ii) output and employment are strongly and positively related to contemporaneous money stock surprises; (iii) output and employment are <u>not</u> strongly and positively related to contemporaneous price level surprises; and (iv) real wage movements are not countercyclical.

28. As formulated by Lucas (1972b), the natural-rate hypothesis asserts that there is no path of nominal variables that will yield a permanent increase in output (or employment) relative to its natural-rate value. Effects of the type emphasized by Tobin (1965) alter the natural-rate value, not the actual value relative to the latter.

29. Eden's (1984) own theory, incidentally, differs from the one sketched below in that it hinges upon strategic informational considerations. A key argument goes as follows: "If ... all other sellers quote prices in fixed dollar terms, the individual seller may find it difficult to make his price contingent on the money supply. The reason is that information is not prohibitively expensive, and buyers may suspect that the seller who offers a contingent price has bought the information. They will, therefore, hesitate to enter into a bet with him...." (Eden, 1984, p. 259). That Eden stresses linkages to the money stock rather than a price index is, incidentally, of little importance in the present context.

30. Though expressed differently, this argument is, I believe, basically similar to Parkin's (1977).

31. This approach, it should be noted, pertains only to the second aspect of the puzzle described above.

32. On this subject, see Niehans (1978) and White (1984).

33. Some readers have pointed out that their own attitudes are influenced by the ongoing nature of their employment relationships.

34. This argument does not presume that there is any explicit contractual agreement between seller and buyers; its purpose is to explain the absence of linkage arrangements that would provide, for example, daily adjustments in

all nominal prices in a retail outlet such as a grocery store, leaving the relative prices as implied by the shelf prices on individual items. The argument is more appealing for consumer products than for industrial goods.

35. A satisfactory analysis of the first aspect will have to take account of Barro's (1977) important objection to Fischer-style contract models--i.e., that they neglect the quantity-determination provision of the contracts. In this regard, emphasis on product markets, rather than labor markets, should be useful as the less formal nature of ongoing relationships tends to induce a tighter link between quantities exchanged and current prices.

36. It might reasonably be questioned whether the type of argument here developed, which relies on the smallness of costs to individuals of failing to index their contracts, can plausibly be responsible for cyclical fluctuations that apparently have major effects on those individuals. A way in which precisely this type of phenomenon can occur has recently been explained by Akerloff and Yellen (1985).

37. The smallness of this number provides some indirect support, it should be noted, for the argument of Section 5 above. There is a close relationship between Lucas's conceptual experiment and the one implicit in my question about salary indexation.

38. Even if expectations are assumed to be formed rationally.

39. Whether or not one works with a model in which "all markets clear" is a matter of convention, but whether this clearing pertains to auction-type markets, or to ones with nominal contracts (perhaps implicit) that have allocational effects, is not.

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