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2 Are Business Cycles All Alike?

Olivier J. Blanchard and Mark W. Watson

2.1 Introduction

The propagation impulse framework, which was introduced in economics by Frisch (1933) and Slutsky (1937) has come to dominate the analysis of economic fluctuations. Fluctuations in economic activity are seen as the result of small, white noise shocks—impulses—that affect the economy through a complex dynamic propagation system.[1] Much, if not most, empirical macroeconomic investigation has focused on the propagation mechanism. In this paper we focus on the characteristics of the impulses and the implications of these characteristics for business cycles.

It is convenient, if not completely accurate, to summarize existing research on impulses as centered on two independent but related ques-

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1. This framework is only one of many that can generate fluctuations. Another one, which clearly underlies much of the early NBER work on cycles, is based on floor/ceiling dynamics, with a much smaller role for impulses. There are probably two reasons why the white noise impulse-linear propagation framework is now widely used. It is convenient to use both analytically and empirically, because of its close relation to linear time series analysis. Statistical evidence that would allow us to choose between the different frameworks has been hard to come by.

In the standard dynamic simultaneous equation model, impulses arise from the exogenous variables and the noise in the system. In the model we employ we do not distinguish between endogenous and exogenous variables. The entire system is driven by the innovations (the one step ahead forecast errors) in the variables. A portion of what we call "innovations" would be explained by current movements of exogenous variables in large macroeconomic models. For example, we find large negative "supply" innovations in late 1974. In a larger model these would be explained by oil import prices.

tions. The first question concerns the number of sources of impulses: Is there only one source of shocks to the economy, or are there many? Monetarists often single out monetary shocks as the main source of fluctuations;[2] this theme has been echoed recently by Lucas (1977) and examined empirically by the estimation of index or dynamic factor analysis models. The alternative view, that there are many, equally important, sources of shocks, seems to dominate most of the day-to-day discussions of economic fluctuations.

The second question concerns the way the shocks lead to large fluctuations. Are fluctuations in economic activity caused by an accumulation of small shocks, where each shock is unimportant if viewed in isolation, or are fluctuations due to infrequent large shocks? The first view derives theoretical support from Slutsky, who demonstrated that the accumulation of small shocks could generate data that mimicked the behavior of macroeconomic time series. It has been forcefully restated by Lucas (1977). The alternative view is less often articulated but clearly underlies many descriptions and policy discussions—that there are infrequent, large, identifiable shocks that dominate all others. Particular economic fluctuations can be ascribed to particular large shocks followed by periods during which the economy returns to equilibrium. Such a view is implicit in the description of specific periods such as the Vietnam War expansion, the oil price recession, or the Volcker disinflation.

The answers to both questions have important implications for economic theory, economic policy, and econometric practice. We cite three examples. The role of monetary policy is quite different if shocks are predominantly monetary or arise partly from policy and partly from the behavior of private agents. The discussion of rules versus discretion is also affected by the nature of shocks. If shocks are small and frequent, policy rules are clearly appropriate. If shocks are instead one of a kind, discretion appears more reasonable.[3] Finally, if infrequent large shocks are present in economic time series, then standard asymptotic approximations to the distribution of estimators may be poor, and robust methods of estimation may be useful.

This paper examines both questions, using two approaches to analyze the empirical evidence. The first is the natural, direct approach, in which we specify and estimate a structural model. This allows us to examine the characteristics of the shocks and to calculate their contributions to economic fluctuations. In section 2.2 we discuss the struc-

2. A supplement to the *Journal of Monetary Economics* was devoted to the analysis of the sources of impulses in different countries, using the Brunner/Meltzer approach. Conclusions vary somewhat across countries, but "measures expressing an unanticipated or accelerating monetary impulse figure foremost" (Brunner and Meltzer 1978, 14).

3. A good example of the importance of the nature of the shocks for the rules versus discretion debate is given by the answers of Lucas and Solow to the question, What should policy have been in 1973–75? in Fischer 1980.

tural model, the data, and the methodology in detail. In section 2.3 we present the empirical results. We conclude that fluctuations are due, in roughly equal proportions, to fiscal, money, demand, and supply shocks. We find substantial evidence against the small-shock hypothesis. What emerges, however, is not an economy characterized by large shocks and a gradual return to equilibrium, but rather an economy with a mixture of large and small shocks.

Our second approach to analyzing the data is an indirect one, which tests one of the implications of the small-shock hypothesis. If economic fluctuations arise from an accumulation of small shocks, business cycles must then be, in some precise sense, alike. We therefore look at how "alike" they are. The comparative advantage of the indirect approach is that it does not require specification of the structural model; its comparative disadvantage is that it may have low power against the large-shock hypothesis. It is very similar to the study by Burns and Mitchell (1946) of commonality and differences of business cycles. Instead of focusing on graphs, we focus on correlation coefficients between variables and an aggregate activity index. Although these correlation coefficients are less revealing than the Burns and Mitchell graphs, they do allow us to state hypotheses precisely and to carry out statistical tests. Our conclusions are somewhat surprising: business cycles are not at all alike. This, however, is not inconsistent with the small-shock hypothesis, and it provides only mild support in favor of the view that large specific events dominate individual cycles. These results cast doubt on the usefulness of making "the business cycle" a reference frame in the analysis of economic time series. These results are developed in section 2.4.

2.2 The Direct Approach: Methodology

2.2.1 The Structural Model

Let X_t be the vector of variables of interest. We assume that the dynamic behavior of X_t is given by the structural model:[4]

(1)
$$X_t = \sum_{i=0}^{n} A_i X_{t-i} + \epsilon_t$$

$$E(\epsilon_t \epsilon'_\tau) = D \text{ if } t = \tau$$

$$0 \text{ otherwise}$$

where D is a diagonal matrix.

4. We assume that the propagation mechanism is linear and time invariant. Violation of either of these assumptions would probably lead to estimated shocks whose distributions have tails thicker than the distribution of the true shocks.

Our vector X_t includes four variables. Two are the basic macroeconomic variables, the variables of ultimate interest—output and the price level. The other two are policy variables. The first is a monetary aggregate, M_1, the second is an index of fiscal policy. We shall describe them more precisely below.

The structural model is composed of four equations. The first two are aggregate demand and aggregate supply. The other two are equations describing policy; they are policy feedback rules. The vector ϵ_t is the vector of four structural disturbances. It includes aggregate supply and demand disturbances as well as the disturbances in fiscal and monetary policy. The matrices A_i, $i = 0, \ldots, n$ represent the propagation mechanism.

We assume that the structural disturbances are contemporaneously uncorrelated and that their covariance matrix, D, is diagonal. However, we do allow the matrix A_0 to differ from zero, so that each structural disturbance is allowed to affect all four variables contemporaneously.

Leaving aside for the movement the issue of identification and estimation of equation (1), we now see how we can formalize the different hypotheses about the nature of the disturbances.

2.2.2 Is There a Dominant Source of Disturbances?

There may be no single yes or no answer to this question. A specific source may dominate short-run movements in output but have little effect on medium- and long-run movements. One source may dominate prices movements, another may dominate output movements.

Variance decompositions are a natural set of statistics to use for shedding light on these questions. These decompositions show the proportion of the K-step ahead forecast error variance of each variable that can be attributed to each of the four shocks. By choosing different values of K, we can look at the effects of each structural disturbance on each variable in the short, medium, and long run.

2.2.3 Are There Infrequent Large Shocks?

A first, straightforward way of answering this question is to look at the distribution of disturbances—or more precisely the distribution of estimated residuals. The statement that there are infrequent large shocks can be interpreted as meaning that the probability density function of each shock has thick tails. A convenient measure of the thickness of tails is the kurtosis coefficient of the marginal distribution of each disturbance, $E[(\epsilon_{jt}/\sigma_j)^4]$. We shall compute these kurtosis coefficients. In addition we shall see whether we can relate the large realizations to specific historical events and fluctuations.

This first approach may, however, be too crude, for at least two reasons. The first is that a particular source of shocks may dominate a given time period, not because of a particular large realization but because of a sequence of medium-sized realizations of the same sign. The second reason is similar but more subtle. The system characterized by equation (1) is highly aggregated. Unless it can be derived by exact aggregation—and this is unlikely—it should be thought of as a low-dimensional representation of the joint behavior of the four variables X_t. In this case the "structural" disturbances ϵ will be linear combinations of current and lagged values of the underlying disturbances. An underlying "oil shock" may therefore appear as a sequence of negative realizations of the supply disturbance in equation (1). For both reasons, we go beyond the computation of kurtosis coefficients. For each time period we decompose the difference between each variable and its forecast constructed K periods before, into components due to realizations of each structural disturbance. If we choose K large enough, forecast errors mirror major fluctuations in output as identified by NBER. We can then see whether each of these fluctuations can be attributed to realizations of a specific structural disturbance, for example, whether the 1973–75 recession is mostly due to adverse supply shocks.

2.2.4 Identification and Estimation

Our approach to identification is to avoid as much as possible overidentifying but controversial restrictions. We impose no restrictions on the lag structure, that is, on A_i, $i = 1, \ldots, n$. We achieve identification by restrictions on A_0, the matrix characterizing contemporaneous relations between variables, and by assuming that the covariance matrix of structural disturbances, D, is diagonal. We now describe our approach and the data in more detail.

Choice of Variables

We use quarterly data for the period 1947:1 to 1982:4. Output, the price level, and monetary and fiscal variables are denoted Y, P, M, and G, respectively. Output, the price level and the monetary variable are the logarithms of real GNP, of the GNP deflator and of nominal M_1. The price and money variables are multiplied by four so that all structural disturbances have the interpretations of rates of change, at annual rates. The fiscal variable G, is an index that attempts to measure the effect of fiscal policy—that is, of government spending, deficits, and debt, on aggregate demand. It is derived from other work (Blanchard 1985) and is described in detail in appendix 2.2.

Reduced-Form Estimation

Since we impose no restrictions on the lag structure, A_i, $i = 1, \ldots, n$, we can proceed in two steps. The reduced form associated with equation (1) is given by:

(2)
$$X_t = \sum_{i=1}^{n} B_i X_{t-i} + x_t$$

$$E(x_t x_\tau') = \Omega \qquad \text{if } t = \tau$$

$$= 0 \qquad \text{if } t \neq \tau$$

$$B_i = (I - A_0)^{-1}A_i \; ; \Omega = [(I - A_0)^{-1}]D[(I - A_0)^{-1}]'.$$

We first estimate the unconstrained reduced form (2). Under the large-shock hypothesis, some of the realizations of the ϵ_t and thus x_t may be large; we therefore use a method of estimation that may be more efficient than ordinary least squares (OLS) in this case. We use the bounded influence method developed by Krasker and Welsch (1982), which in effect decreases the weight given to observations with large realizations.[5] We choose a lag length, n, equal to 4.[6]

The vector x_t is the vector of unexpected movements in Y, P, M, and G. Let lower-case letters denote unexpected movements in these variables, so that this first step in estimation gives us estimated time series for y, p, m, and g.

Structural Estimation

The second step takes us from x to ϵ. Note that equations (1) and (2) imply:

(3)
$$x = A_0 x + \epsilon.$$

Thus, to go from x to ϵ we need to specify and estimate A_0, the set of contemporaneous relations between the variables. We specify the following set of relations:

(4) $y = b_1 p + \epsilon^s$ (aggregate supply)

(5) $y = b_2 m - b_3 p + b_4 g + \epsilon^d$ (aggregate demand)

(6) $g = c_1 y + c_2 p + \epsilon^g$ (fiscal rule)

(7) $m = c_3 y + c_4 p + \epsilon^m$ (money rule)

5. LAD or other robust M estimators could also have been used. In some circumstances OLS may be more efficient than the robust estimators because of the presence of lagged values.

6. Each equation in the vector autoregression included a constant and a linear time trend. When the vector autoregression was estimated without a time trend, the estimated residuals, x, were essentially unchanged.

We have chosen standard specifications for aggregate supply and demand. Output supplied is a function of the price level.[7] Output demanded is a function of nominal money, the price level, and fiscal policy; this should be viewed as the reduced form of an IS-LM model, so that ε^d is a linear combination of the IS and LM disturbances. The last two equations are policy rules, which allow the fiscal index and money to respond contemporaneously to output and the price level.[8]

Even with the zero restrictions on A_0 implicit in the equations above, the system of equations (4) to (7) is not identified. The model contains eight coefficients and four variances that must be estimated from the ten unique elements in Ω. To achieve identification, we use a priori information on two of the parameters.

Within a quarter, there is little or no discretionary response of fiscal policy to changes in prices and output. Most of the response depends on institutional arrangements, such as the structure of income tax rates, the degree and timing of indexation of transfer payments, and so on. Thus the coefficients c_1 and c_2 can be constructed directly; the details of the computations are given in appendix 2.2. Using these coefficients, we obtain $\hat{\epsilon}^g$ from equation (6).

Given the two constructed coefficients c_1 and c_2, we now have six unknown coefficients and four variances to estimate using the ten unique elements in Ω. The model is just identified. Estimation proceeds as follows: $\hat{\epsilon}^g$ is used as an instrument in equation (4) to obtain $\hat{\epsilon}^s$; $\hat{\epsilon}^g$ and $\hat{\epsilon}^s$ are used as instruments in equation (7) to obtain $\hat{\epsilon}^m$. Finally, $\hat{\epsilon}^g$, $\hat{\epsilon}^s$, and $\hat{\epsilon}^m$ are used as instruments in equation (5) to obtain $\hat{\epsilon}_d$.

The validity of these instruments at each stage depends on the plausibility of the assumption that the relevant disturbances are not correlated. Although we do not believe this is exactly the case, we find it plausible that they have a low correlation, so that our identification is approximately correct.

It may be useful to compare our method for identifying and estimating shocks with the more common method used in the vector autoregres-

7. A more detailed specification of aggregate supply, recognizing the effects of the price of materials would be:

$$\begin{aligned} y &= d_1 p - d_2(p_m - p) && + \epsilon^{ys} \\ p_m &= d_3 p + d_4 y && + \epsilon^{pm}, \end{aligned}$$

where supply depends on the price of materials, p_m, and the price level, and where in turn the nominal price of materials depends on the price level and the level of output. The two equations have, however, the same specification, and it is therefore impossible to identify separately the shocks to the price of materials and to supply ϵ^{pm} and ϵ^{ys}. Equation (4) is therefore the solved-out version of this two-equation system, and ϵ^s is a linear combination of these two shocks.

8. If money supply responds to interest rates directly rather than to output and prices, ϵ^m and ϵ^d will both depend partly on money demand shocks and thus will be correlated. Our estimation method will then attribute as much of the variance as possible to ϵ^m and incorporate the residual in ϵ^d.

sion literature. A common practice in that literature is to decompose, as we do, the forecast errors into a set of uncorrelated shocks. There the identification problem is solved by assuming that the matrix $(I - A_0)$ is triangular or can be made triangular by rearranging its rows. This yields a recursive structure that is efficiently estimated by OLS. We do not assume a recursive structure but rather impose four zero restrictions in addition to constructing two coefficients c_1 and c_2. Our method produces estimated disturbances much closer to true structural disturbances than would be obtained by imposing a recursive structure on the model.

2.3 The Direct Approach: Results

2.3.1 Reduced-Form Evidence

The first step is the estimation of the reduced form given by equation (2). The estimated B_i, $i = 1, \ldots, 4$ are of no particular interest. The estimated time series corresponding to unexpected movements of x—that is of y, m, p, and g—are of more interest. Table 2.1 gives, for y, m, p, and g, the value of residuals larger than 1.5 standard deviations in absolute value, as well as the associated standard deviation and estimated kurtosis.

The kurtosis coefficient of a normally distributed random variable is equal to 3. The 99% significance level of the kurtosis coefficient, for a sample of 120 observations drawn from a normal distribution, is 4.34. Thus, ignoring the fact that these are estimated residuals rather than actual realizations, three of the four disturbances have significantly fat tails. Since linear combinations of independent random variables have kurtosis smaller than the maximum kurtosis of the variables themselves, this strongly suggests large kurtosis of the structural disturbances.[9] We now turn to structural estimation.

2.3.2 The Structural Coefficients

The second step is estimation of A_0, from equations (4) to (7). We use constructed values for c_1 and c_2 of -0.34 and -1.1 respectively. Unexpected increases in output increase taxes more than expenditures and lead to fiscal contraction. Unexpected inflation increases real taxes but decreases real expenditures, leading also to fiscal contraction. We are less confident of c_2, the effect of inflation, than we are of c_1. In

9. A more precise statement is the following: Let X_1 and X_2 be independent variables with kurtosis K_1 and K_2, one of which is greater than or equal to 3. Then if Z is a linear combination of X_1 and X_2, $K_Z \leq \max(K_1, K_2)$. We do not, however, assume independence but only assume zero correlation of the structural disturbances.

Table 2.1 Large Reduced-Form Disturbances

Date	y	g	m	p
1948:4				−2.6
1949:1				−2.2
1949:4	−2.4			
1950:1	3.2	2.6		
1950:2		−5.1		1.6
1950:3	1.8	−1.6		5.1
1951:1				3.7
1951:2		4.2		−2.8
1951:3		2.2		−1.6
1951:4			1.6	
1952:2		1.6		
1952:3		1.7		
1952:4	1.6			
1953:1		1.6		
1953:4				−1.6
1954:1	−1.7			2.1
1958:1	−2.2			
1959:1		−1.8		
1959:3	−2.7			
1959:4			−2.9	
1960:1	2.2	−2.7		
1960:4	−1.9			
1962:3			−1.5	
1965:4	1.6			
1966:3			−2.2	
1967:3			1.8	
1970:4	−1.8			
1971:3	−1.6			
1972:2				−1.5
1972:4		1.7		
1974:4	−1.6			1.7
1975:1	−3.1			
1975:2		3.6		−1.7
1975:3		−3.1		
1975:4			−1.6	
1978:2	2.2			2.1
1979:2			1.7	
1980:2	−2.5		−4.2	
1980:3	2.4		4.7	
1981:3			−3.5	
1982:4			3.0	
Standard error	.0085	.0431	.0244	.0182
Kurtosis	4.0	10.2	8.6	8.2

Note: Ratios of residuals to standard errors are reported.

appendix 2.1 we report alternative structural coefficient estimates based on $c_2 = -1.3$ and $c_2 = -1.0$.

The results of estimating equations (4) to (7) are reported in table 2.2. All coefficients except one are of the expected sign. Nominal money has a negative contemporaneous effect on output; this is consistent with a positive correlation between unexpected movements in money and output because of the positive effect of output on money supply. Indeed the correlation m and y is .32. (Anticipating results below, we find that the effect of nominal money on output is positive after one quarter.) Aggregate supply is upward sloping; a comparison with the results of table 2.A.1 suggests that the slope of aggregate supply is sensitive to the value of c_2.

Given our estimates of the reduced form and of A_0, we can now decompose each variable (Y, P, M, G) as the sum of four distributed lags of each of the structural disturbances ϵ^d, ϵ^s, ϵ^m, and ϵ^g. Technically, we can compute the structural moving average representation of the system characterized by equation (1).

2.3.3 One or Many Sources of Shocks? Variance Decomposition

Does one source of shocks dominate? We have seen that a natural way of answering this question is to characterize the contribution of each disturbance to the unexpected movement in each variable. We define unexpected movement as the difference between the actual value of a variable and the forecast constructed K periods earlier using equation (1). We use three values of K. The first case, $K = 1$, decomposes the variance of y, p, m, and g into their four components, the variances of ϵ^d, ϵ^s, ϵ^m and ϵ^g. The other two values, $K = 4$ and $K = 20$, correspond to the medium run and the long run respectively.

The results are reported in table 2.3. Demand shocks dominate output in the short run; supply shocks dominate price in the short run. In the

Table 2.2 **Structural Estimates**

Fiscal[a]	$g = -.34y - 1.1p$	$+ \epsilon^g$	
Money supply	$m = 1.40y + .19p$	$+ \epsilon^m$	
	(1.4)[b] (.7)		
Aggregate supply	$y = .81p$	$+ \epsilon^s$	
	(1.1)		
Aggregate demand	$y = -.10p - .20m + .06g$	$+ \epsilon^d$	
	(−3.1) (−2.2) (2.4)		

Standard deviations

ϵ^g	ϵ^m	ϵ^s	ϵ^d
.041	.024	.017	.011

[a]Coefficients constructed, not estimated.
[b]t-statistics in parentheses.

Table 2.3 **Variance Decompositions**

	Structural Disturbance			
	ϵ^g	ϵ^s	ϵ^m	ϵ^d
Contemporaneously				
$Y - E_{-1}Y$	.03	.19	.04	.74
$G - E_{-1}G$	.78	.14	.00	.08
$M - E_{-1}M$	.01	.01	.74	.25
$P - E_{-1}P$	.01	.74	.01	.24
Four quarters ahead				
$Y - E_{-4}Y$	.15	.16	.16	.54
$G - E_{-4}G$	.70	.13	.00	.16
$M - E_{-4}M$	.13	.03	.67	.17
$P - E_{-4}P$	.01	.65	.01	.33
Twenty quarters ahead				
$Y - E_{-20}Y$	.27	.20	.17	.37
$G - E_{-20}G$	.66	.12	.05	.17
$M - E_{-20}M$	.28	.04	.64	.05
$P - E_{-20}P$	.15	.22	.36	.26

medium and long run, however, *all four shocks are important in explaining the behavior of output and prices*. There is no evidence in support of the one dominant source of shocks theory.

2.3.4 Are There Infrequent Large Shocks? I

Table 2.4 reports values and dates for all estimated realizations of ϵ^d,ϵ^s,ϵ^m and ϵ^g larger than 1.5 times their respective standard deviation. We can compare these with traditional, informal accounts of the history of economic fluctuations since 1948 and see whether specific events that have been emphasized there correspond to large realizations. A useful, concise summary of the events associated with large postwar fluctuations is contained in table 1.1 in the paper by Eckstein and Sinai in this volume (chap. 1).

The first major expansion in our sample, from 1949:4 to 1953:2, is usually explained both by fiscal shocks associated with the Korean War and by a sharp increase in private spending. We find evidence of both in 1951 and in 1952. From 1955 to the early 1970s, large shocks are few and not easily interpretable. There are, for example, no large shocks to either fiscal policy or private spending corresponding to either the Kennedy tax cut or the Vietnam War. In the 1970s, major fluctuations are usually explained by the two oil shocks. There is some evidence in favor of this description. We find two large supply shocks in 1974:4 and 1975:1; we also find large fiscal and large demand shocks during

Table 2.4 Large Structural Disturbances

Date	Fiscal	Supply	Money	Demand
1948:3	1.9			
1948:4		2.5		
1949:1	−1.5			−1.9
1949:4				−1.8
1950:1	3.0	1.8		2.0
1950:2	−4.6	−1.6		
1950:3		−3.7		3.6
1951:1	1.7	−3.6		
1951:2	3.1	3.2		
1951:3	1.6	1.8		
1951:4			1.6	
1952:2	1.5			
1952:3	2.0			
1952:4				1.7
1953:4				−1.6
1954:1		−2.8		
1954:3		1.8		
1957:4				−1.7
1958:1		−1.5		−1.7
1958:3	1.7			
1959:1		−1.6		
1959:3				−2.3
1959:4			−2.6	
1960:1	−2.6			2.4
1960:3			1.5	
1960:4				−2.0
1966:3			−2.2	
1968:4			1.5	
1971:2			2.1	
1971:3				−1.8
1972:2		1.6		
1972:4	1.7			
1974:4		−2.4		
1975:1		−2.5		−2.4
1975:2	3.1	1.9		
1975:3	−3.1			
1975:4			−1.8	
1978:2				2.7
1979:2			1.6	
1980:2		−2.1	−3.2	−2.7
1980:3			3.4	3.4
1981:2			1.6	
1981:3			−3.8	
1982:1			1.6	
1982:4			3.7	

the same period. The two recessions of the early 1980s are usually ascribed to monetary policy. We find substantial evidence in favor of this description. There are large shocks to money supply for most of the period 1979:2 to 1982:4 and two very large negative shocks in 1980:2 and 1981:3.

The overall impression is therefore one of infrequent large shocks, but not so large as to dominate all others and the behavior of aggregate variables for long periods. To confirm this impression, we report the kurtosis coefficients of the structural disturbances in table 2.5A; in all cases we can reject normality with high confidence. In table 2.5B we use another descriptive device. We assume that each structural disturbance is an independent draw from a mixed normal distribution, that is for $x = g, d, s$, or m:

$$\epsilon^x = \epsilon_1^x \quad \text{with probability } 1 - P_x$$
$$\epsilon^x = \epsilon_2^x \quad \text{with probability } P_x$$

where

$$\epsilon_1^x \sim N(0,\sigma_{1x}^2)\ ,\ \epsilon_2^x \sim N(0,\sigma_{2x}^2)\ .$$
$$\sigma_{1x}^2 < \sigma_{2x}^2$$

The realization of each disturbance is drawn either from a normal distribution with large variance, with probability P, or from a normal distribution with small variance, with probability $1 - P$. The estimated values of σ_{1x}, σ_{2x}, P_x, estimated by maximum likelihood, are reported in table 2.5B. The results suggest large, but not very large, ratios of the standard deviation of large to the standard deviation of small shocks; they also suggest infrequent, but not very infrequent, large shocks. The estimated probabilities imply that one out of six fiscal or money shocks and one out of three supply or demand shocks came from the large variance distributions.

Table 2.5 Characteristics of Structural Disturbances

A. Estimated kurtosis	ϵ^g	ϵ^s	ϵ^m	ϵ^d
K	7.0	5.4	5.9	4.6
B. Disturbances as mixed normals				
σ_1	.68	.63	.72	.68
	(.08)[a]	(.10)	(.09)	(.13)
σ_2	2.01	1.62	1.97	1.50
	(.64)	(.41)	(1.03)	(.41)
Ratio	2.95	2.57	2.73	2.21
Probability	.15	.27	.14	.30
	(.09)	(.15)	(.15)	(.22)

[a]Standard errors in parentheses.

The dating of the large shocks in table 2.4 suggests two more characteristics of shocks. First, large shocks tend to be followed by large shocks, suggesting some form of autoregressive conditional heteroskedasticity as discussed in Engle (1982). Second, there seems to be some tendency for large shocks to happen in unison. In 1950:1, for example, we find large fiscal, supply, and demand shocks, whereas in 1980:3 we find large supply, money, and demand shocks. To confirm these impressions we present in table 2.6 the correlations and first autocorrelations between the squares of the structural shocks.[10] The table shows a large positive contemporaneous correlation between the square of the supply shock and the square of the demand shock. A weaker contemporaneous relationship between supply and the fiscal shock is present. The squares of all shocks are positively correlated with their own lagged values; there is also significant correlation between demand, the lagged fiscal and supply shocks, and the fiscal shock and lagged supply shock. All in all, these results suggest an economy characterized by active, volatile periods followed by quiet, calm periods, both of varied duration.

2.3.5 Are There Infrequent Large Shocks? II

We discussed in section 2.2 the possibility that a specific source of shocks may dominate some episode of economic fluctuations, even if there are no large realizations of the shock. To explore this possibility, we construct an unexpected output series, where the expectations are the forecasts of output based on the estimated model corresponding to equation (1), eight quarters before. We chose eight quarters because the troughs and peaks in this unexpected output series correspond closely to NBER troughs and peaks. We then decompose this forecast

Table 2.6 **Correlations between Squares of Structural Disturbances**

	$(\epsilon^g)^2$	$(\epsilon^s)^2$	$(\epsilon^m)^2$	$(\epsilon^d)^2$
$(\epsilon^g)^2$	—	.27	−.05	.08
$(\epsilon^s)^2$		—	−.01	.36
$(\epsilon^m)^2$			—	.28
$(\epsilon^d)^2$				—
$(\epsilon^g_{-1})^2$	.33	.43	.00	.33
$(\epsilon^s_{-1})^2$	.35	.38	.03	.13
$(\epsilon^m_{-1})^2$	.02	−.09	.23	.21
$(\epsilon^d_{-1})^2$	.15	.08	.13	.16

10. Although the contemporaneous correlation between the levels of the shock is zero by construction, the same is not true of the squares of the shocks.

error for GNP into components due to each of the four structural disturbances. This decomposition is represented graphically in figure 2.1; the corresponding time series are given in table 2.A.2 in appendix 2.1.

No single recession can be attributed to only one source of shock. Post-war recessions appear to be due to the combination of two or three shocks. The 1960:4 trough, for example, where the GNP forecast error is −6.7%, is attributed to a fiscal shock component (−2.4%), a supply shock component (−1.1%), a money shock component (−1.7%), and a demand shock component (−1.4%). The 1975:1 trough, where the GNP forecast error is also −6.7%, seems to have a large supply shock component (−3.6%) and a demand shock component (−2.9%). The 1982:4 trough, where the GNP forecast error is −4.5%, is decomposed as −1.4% (fiscal), 1.1% (supply), −1.4% (money), and −2.8% (demand).

To summarize the results of this section, we find substantial evidence against the single source of shock hypothesis. We find some evidence of large infrequent shocks; however, they do not seem to dominate economic fluctuations.

2.4 The Indirect Approach

If economic fluctuations are due to an accumulation of small shocks, then in some sense business cycles should all be alike. In this section we make precise the sense in which cycles should be alike and examine the empirical evidence.

The most influential contribution to the position that cycles are alike is the empirical work carried out by Burns and Mitchell (1946) on pre–World War II data. Their work focused not only on the characteristic cyclical behavior of many economic variables but also on how, in specific cycles, the behavior of these variables differed from their characteristic cyclical behavior. Looking at their graphs, one is impressed at how similar the behavior of most variables is across different cycles; this is true not only of quantities, for which it may not be too surprising, but also, for example, of interest rates.

We considered extending the Burns/Mitchell graph method to the eight postwar cycles but decided against it. Many steps of the method, and in particular their time deformation, are judgmental rather than mechanical. As a result, it is impossible to derive the statistical properties of their results. When comparing the graphs of short rates across two cycles, for example, we have no statistical yardstick to decide whether they are similar or significantly different. As a result also, we do not know which details, in the wealth of details provided in these graphs, should be thought of as significant.

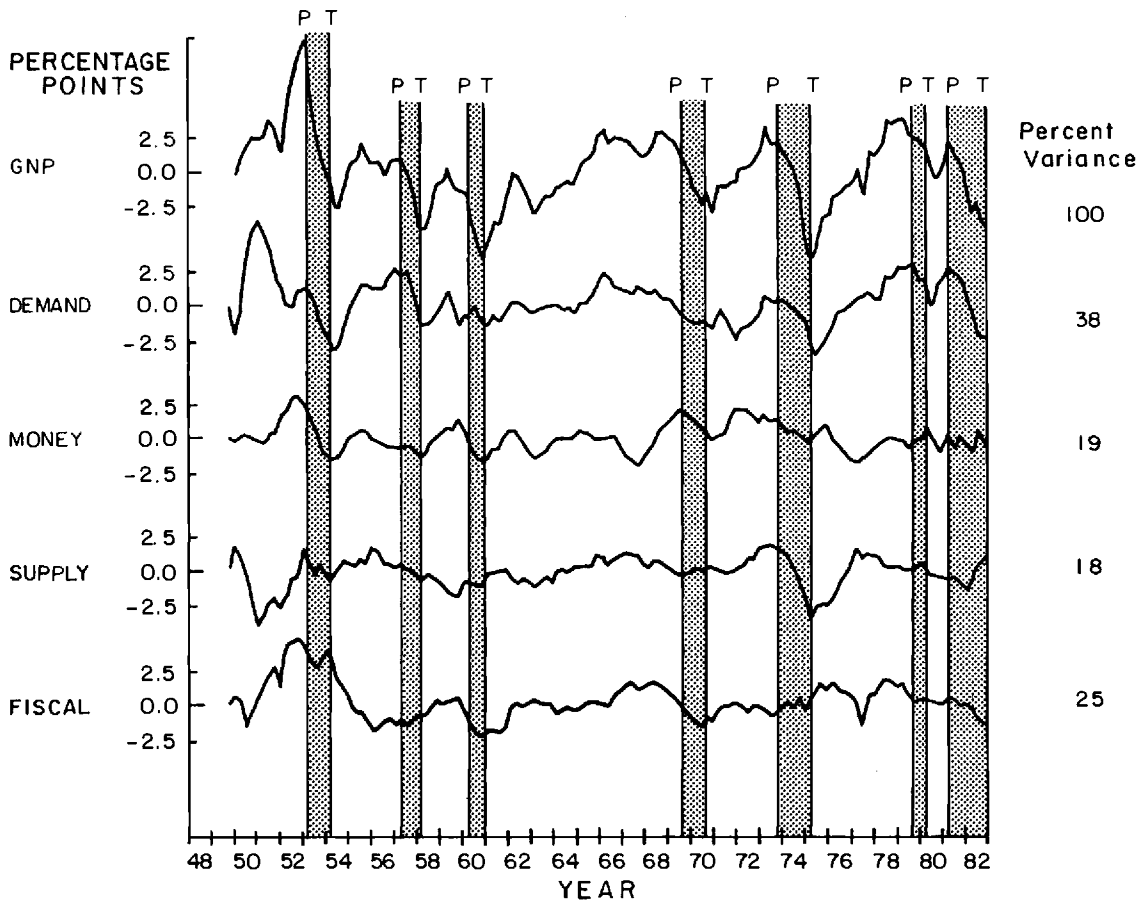

Fig. 2.1 Components of GNP forecast error.

Therefore we use an approach that is in the spirit of Burns and Mitchell but allows us to derive the statistical properties of the estimators we use. The trade-off is that the statistics we give are much less revealing than the Burns/Mitchell graphs. Our approach is to compute the cross-correlations at different leads and lags between various variables and a reference variable such as GNP, across different cycles.

2.4.1 The Construction of Correlation Coefficients

The first step is to divide the sample into subsamples. We adopt the standard division into cycles, with trough points determined by the NBER chronology. This division may not be, under the large-shock hypothesis, the most appropriate, since a large shock may well dominate parts of two cycles. It is, however, the least controversial. Defining the trough-to-trough period as a cycle, there are seven complete cycles for which we have data; their dates are given in table 2.7. This gives us seven subsamples.

For each subsample, we compute cross-correlations at various leads and lags between the reference variable and the variable considered. Deterministic seasonality is removed from all variables before the calculation of the correlations. A more difficult issue is that of the time trend: the series may be generated either by a deterministic time trend or by a stochastic time trend or by both. In the previous two sections, this issue was unimportant in the sense that inclusion or exclusion of a deterministic trend together with unconstrained lag structures in the reduced form made little difference to estimated realizations of the disturbances. Here the issue is much more important. Computing deviations from a single deterministic trend for the whole sample may be very misleading if the trend is stochastic. On the other hand, taking first or second differences of the time series probably removes nonstationarities associated with a stochastic trend, but correlations between first or second differences of the time series are difficult to interpret.

In their work, Burns and Mitchell adopt an agnostic and flexible solution to that problem: they compute deviations of the variables from subsample means. Thus they proxy the time trend by a step function. Although this does not capture the time trend within each subsample, it does imply that across subsamples, the estimated time trend will track the underlying one. We initially followed Burns and Mitchell in their formalization but found this procedure to be misleading for variables with strong time trends. During each subsample, both the reference and the other variable are below their means at the beginning and above their means at the end; this generates spuriously high correlation between the variables. We modify the Burns/Mitchell procedure as follows: for each subsample, we allow for both a level and a time trend; the time trend is given by the slope of the line going from trough to trough. This should be thought of as a flexible (perhaps too flexible) parameterization of the time trend, allowing for six level and slope changes over the complete sample.

The cross-correlations are then computed for deviations of each of the two series from its trend. We compute correlations of the reference variable and of the other variable, up to two leads and lags.

2.4.2 The Construction of Confidence Levels

For each variable we calculate cross-correlations with our reference variable, GNP, for each of the seven cycles. We then want to answer the following questions: Should we be surprised by the differences in estimated correlation across cycles? More precisely, under the null hypothesis that fluctuations are due to the accumulation of small shocks, how large are these differences in the correlation coefficients likely to be? Thus, we must derive the distribution of the differences between the largest and smallest correlation coefficients, at each lag or lead for each variable. This distribution is far too difficult to derive analytically; instead we rely on Monte Carlo simulations.

The first step is to estimate, for each variable, the bivariate process generating the reference variable and the variable under consideration. We allow for four lags of each variable and a linear time trend, for the period 1947:1 to 1982:4. The method of estimation is, for the same reasons as in section 2.2, Krasker-Welsch.

The second step is to simulate the bivariate process, using disturbances drawn from a *normal* distribution for disturbances. (Thus we implicitly characterize the "small-shock" hypothesis as a hypothesis that this joint distribution is normal.) We generate 1,000 samples of 147 observations each. We then divide each sample into cycles by identifying troughs in the GNP series. Let x_t denote the log of real GNP at time t. Time t is a trough if two conditions are satisfied. The first is that $x_{t-1} > x_t < x_{t+1} < x_{t+2} < x_{t+3}$, and the second that x_t be at least 0.5 % below the previous peak value of x. The first ensures that expansions are longer than three periods, and the second eliminates minor downturns. (When applied to the actual sample, this rule correctly identifies NBER troughs, except for two that differ from the NBER trough by one quarter.) Given this division into cycles, we compute, as in the actual sample, cycle-specific correlations and obtain, for each of the 1,000 samples, the difference between the largest and the smallest correlation. Finally, by looking at the 1,000 samples, we get an empirical distribution for the differences.

What we report in table 2.7 for each variable and for correlations at each lead and lag are probabilities that in the corresponding empirical distributions the difference between the largest and smallest correlation exceeds the value of this difference in the actual sample. This probability is denoted p. *A very small value of p indicates that the difference observed in the actual sample is surprisingly large under the small-shock hypothesis.* It would therefore be evidence against the small-shock hypothesis.

2.4.3 The Choice of Variables

Most quantity variables, such as consumption or investment, appear highly correlated with real GNP. Most of the models we have imply that it should be so, nearly irrespective of the source of shocks. Most models imply that correlations of prices and interest rates with GNP will be of different signs depending on the source of shocks. We report results for various prices, interest rates, policy variables, and quantities.

We look at *three real wages*. In all three cases, the numerator is the same, the index of average hourly earnings of production and nonsupervisory workers, adjusted for overtime and interindustry shifts, in manufacturing. In table 2.7A, the wage is deflated by the GNP deflator. In table 2.7B, it is deflated by the CPI and is therefore a consumption real wage. In table 2.7C, it is deflated by the producer price index for manufacturers and is therefore a product wage. In all three cases, we take the logarithm of the real wage so constructed.

We then look at *two relative prices*. Both are relative prices of materials in terms of finished goods. Because of the two oil shocks, we consider two different prices. The first is the ratio of the price of crude fuel to the producer price index for finished goods and is studied in table 2.7D. Table 2.7E gives the behavior of the price of nonfood, nonfuel materials in terms of finished goods.

We then look at the behavior of *interest rates*. Table 2.7F characterizes the behavior of the nominal three month treasury bill rate. Table 2.7G gives the behavior of Moody's AAA corporate bond yield.

We consider the two *policy variables:* the fiscal index defined in the first section, and nominal M_1. The results are given in tables 2.7H and I.

Finally, we consider three quantity variables. Table 2.7J shows the behavior of real consumption expenditures. Table 2.7K and L shows the behavior of nonresidential and residential investment.

2.4.4 General Results

In looking at table 2.7, there are two types of questions we want to answer. The first is not directly the subject of the paper but is clearly of interest. It is about the typical behavior of each variable in the cycle. The answer is given for each variable by the sequence of average correlation coefficients at the different lags and leads.

Table 2.7 Correlations

Cycle	Trough to Trough	Peak
1	1949:4 to 1954:2	1953:2
2	1954:2 to 1958:2	1957:3
3	1958:2 to 1961:1	1960:2
4	1961:1 to 1970:4	1969:4
5	1970:4 to 1975:1	1973:4
6	1975:1 to 1980:2	1979:4
7	1980:2 to 1982:4	1981:2

ρ_i = correlation between the reference variable, logarithm of real GNP at time t, and the other variable at time $t + i$.

Real Wages

A. Real wage in terms of the GNP deflator (in log)

Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1	−.81	−.70	−.36	−.25	.09
2	−.06	−.41	−.48	−.18	.44
3	−.17	.02	.03	−.35	−.59
4	−.11	−.13	−.01	−.04	−.00
5	.85	.90	.90	.65	.37
6	.75	.84	.84	.75	.63
7	.62	.61	.06	−.29	−.38
Average	−.15	−.16	.14	.04	.08
Difference	1.67	1.61	1.38	1.10	1.22
p	.04	.07	.27	.65	.52

B. Real wage in terms of the CPI (in log)

Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1	−.53	−.58	−.57	−.64	−.57
2	.09	.44	.79	.85	.76
3	−.15	.29	.75	.47	−.07
4	.56	.57	.63	.56	.49
5	.84	.67	.47	.02	−.31
6	.78	.89	.88	.78	.65
7	.57	.32	−.31	−.53	−.24
Average	.30	.37	.37	.21	.10
Difference	1.37	1.47	1.45	1.49	1.34
p	.48	.31	.32	.22	.49

C. Real wage in terms of the PPI (in log)

Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1	−.68	−.71	−.63	−.55	−.28
2	.17	.60	.91	.88	.63
3	−.29	.45	.87	.62	.27
4	−.46	−.56	−.62	−.72	−.76
5	.88	.74	.52	.08	−.27
6	.78	.86	.82	.71	.59
7	−.42	−.70	−.72	−.60	.01
Average	−.02	.09	.16	.06	.02
Difference	1.57	1.57	1.62	1.61	1.40
p	.17	.18	.13	.11	.44

Table 2.7 (continued)

Relative Prices

D. Relative price of crude fuels in terms of finished goods (in log)

Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1	−.65	−.61	−.45	−.43	−.19
2	−.25	−.04	.09	.31	.41
3	−.07	.45	.42	.46	.17
4	−.61	−.75	−.86	−.91	−.91
5	−.66	−.86	−.91	−.81	−.63
6	.47	.46	.35	.34	.44
7	−.56	−.39	−.23	−.16	−.01
Average	−.33	−.24	−.22	−.17	−.10
Difference	1.13	1.33	1.33	1.37	1.35
p	.56	.39	.39	.30	.38

E. Relative price of nonfood/nonfuel materials in terms of finished goods (in log)

Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1	.62	.66	.56	.30	−.12
2	.17	.69	.92	.78	.51
3	.32	.75	.89	.64	.24
4	.09	.06	.02	−.16	−.35
5	−.06	.28	.62	.82	.89
6	−.75	−.77	−.58	−.40	−.23
7	−.02	.59	.92	.82	.32
Average	.05	.32	.47	.40	.18
Difference	1.38	1.53	1.51	1.22	1.24
p	.32	.16	.15	.37	.56

Interest Rates

F. Three-month treasury bill rate

Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1	−.20	.22	.68	.86	.88
2	−.30	.02	.56	.83	.84
3	−.29	.33	.71	.83	.67
4	−.15	−.01	.20	.36	.49
5	−.26	.05	.40	.69	.84
6	−.56	−.42	−.07	−.23	.39
7	−.42	.41	.71	−.58	−.46
Average	−.31	.60	.45	.62	.65
Difference	.41	.83	.79	.62	.49
p	.94	.64	.70	.77	.88

G. AAA corporate bonds yield

Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1	−.54	−.03	.44	.66	.70
2	−.65	−.35	.16	.32	.38
3	.12	.69	.90	.69	.29
4	−.79	−.71	−.62	−.48	−.30
5	−.88	−.73	−.52	−.08	.19
6	−.82	−.87	−.68	−.48	−.29
7	−.72	−.10	.42	.62	.80
Average	−.61	−.30	.01	.17	.25
Difference	1.00	1.56	1.58	1.17	1.11
p	.55	.13	.25	.63	.67

(*continued*)

Table 2.7 (continued)

		Policy Variables			
H. Fiscal index					
Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1					
2	−.49	−.31	−.03	.12	.58
3	−.43	−.74	−.89	−.67	−.32
4	.73	.45	−.01	−.46	−.74
5	.40	.36	.28	.14	.04
6	−.10	−.20	−.35	−.67	−.63
7	.51	−.08	−.55	−.54	−.47
Average	.09	−.07	−.22	−.29	−.22
Difference	1.22	1.19	1.17	.81	1.32
p	.56	.61	.70	.92	.51
I. Nominal money, log of Ml					
Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1	.07	.44	.71	.76	.67
2	.59	.94	.92	.53	.02
3	.68	.73	.69	.40	−.08
4	−.46	−.38	−.31	−.17	−.05
5	.71	.88	.94	.80	.50
6	.08	.23	.53	.64	.74
7	.83	.87	.43	.11	−.16
Average	.35	.53	.56	.44	.23
Difference	1.29	1.32	1.25	.97	.90
p	.23	.14	.21	.65	.89
		Quantity Variables			
J. Logarithm of real consumption expenditures					
Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1	.22	.35	.32	−.02	−.46
2	.47	.78	.97	.72	.23
3	−.03	.61	.90	.84	.33
4	.69	.78	.88	.91	.90
5	.87	.96	.88	.59	.26
6	.69	.83	.96	.76	.60
7	.39	.86	.91	.40	−.03
Average	.47	.74	.83	.60	.26
Difference	.90	.61	.65	.93	1.36
p	.73	.69	.42	.54	.35
K. Logarithm of real residential investment expenditures					
Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_{+1}	ρ_{+2}
1	.34	.18	−.09	−.49	−.82
2	.77	.71	.55	−.00	−.50
3	.31	.78	.92	.65	.08
4	.02	−.01	−.11	−.29	−.47
5	.91	.88	.78	.43	−.01
6	.73	.86	.94	.73	.52
7	.72	.93	.68	.16	−.37
Average	.54	.62	.52	.17	−.22
Difference	.91	.94	1.05	1.21	1.34
p	.58	.38	.28	.22	.17

Table 2.7 (continued)

L. Logarithm of real nonresidential investment expenditures

Cycle	ρ_{-2}	ρ_{-1}	ρ_0	ρ_1	ρ_2
1	.30	.50	.63	.39	−.19
2	.02	.45	.86	.90	.75
3	−.65	−.23	.28	.81	.84
4	.75	.83	.89	.91	.87
5	.38	.68	.92	.97	.89
6	.39	.53	.77	.88	.89
7	−.58	.08	.64	.88	.84
Average	.09	.41	.71	.82	.70
Difference	1.40	1.06	.64	.58	1.08
p	.15	.41	.52	.53	.39

How do these sequences relate to Burns/Mitchell graphs? The relation is roughly the following: if the sequence is flat and close to zero, the variable has little cyclical behavior. If the sequence is flat and positive, the variable is procyclical, peaking at the cycle peak; if flat and negative, it is countercyclical, reaching its trough at the cycle peak.

If the sequence is not flat, the variable has cyclical behavior but reaches its peak, or its trough if countercyclical, before or after the cyclical peak. If, for example, ρ_{-1} is large and negative, this suggests that the variable is countercyclical, reaching its trough one quarter before the cyclical peak. As expected, the quantity variables are procyclical; there seems to be a tendency for nonresidential investment to lag GNP by one quarter and residential investment to lead GNP by one quarter. We find little average cyclical behavior of real wages. Relative fuel prices and long-term interest rates are countercyclical and lead GNP by at least two quarters. Relative nonfood/nonfuel materials and short-term rates appear to be procyclical. We now turn to the second question, which is one of the subjects of this paper. How different are the correlations, and are these differences surprising?

The first part of the answer is that *correlations are very different across cycles*. This is true both for variables with little cyclical behavior, such as the real wage, and for variables that vary cyclically, such as nominal rates. These differences suggest that business cycles are indeed not all alike. The second part of the answer may, however, also be surprising: it is that *under the small-shock hypothesis, such differences are not unusual*. For most correlations and most variables, the p values are not particularly small. Thus the tentative conclusion of this section is that, although business cycles are not very much alike, their differences are not inconsistent with the hypothesis of the accumulation of small shocks through an invariant propagation mechanism.

2.5 Conclusions

In sections 2.2 and 2.4 we specified and estimated a structural model that allowed us to directly investigate the properties of shocks and their role in economic fluctuations. From this analysis we conclude that fluctuations are due, in roughly equal proportions, to fiscal, money, demand, and supply shocks. We find substantial evidence against the small-shock hypothesis. What emerges, however, is not an economy characterized by large shocks and a gradual return to equilibrium, but rather an economy with a mixture of large and small shocks.

In section 2.4 we investigated the influence of shocks on economic fluctuations in an indirect way by examining stability of correlations between different economic variables across all of the postwar business cycles. Here we found that correlations were very unstable—that business cycles were not at all alike. This, however, is not inconsistent wtih the small-shock hypothesis and provides only mild support for the view that large specific events dominate the characteristics of individual cycles. These results cast doubt on the usefulness of using "the business cycle" as a reference frame in the analysis of economic time series.

Appendix 2.1

Table 2.A.1 Alternative Structural Estimates

$c_2 = 1.3$					
Fiscal	g =	−.34y − 1.3p			+ ϵ^g
Money supply	m =	1.20y + .22p			+ ϵ^m
Aggregate supply	y =	.45y			+ ϵ^s
Aggregate demand	y =	.09g − .10m − .40p			+ ϵ^d
Standard deviations	ϵ^g	ϵ^m	ϵ^s	ϵ^d	
	.041	.024	.011	.014	
$c_1 = 1.0$					
Fiscal	g =	−.34y − 1.0p			+ ϵ^g
Money supply	m =	1.52y + .14p			+ ϵ^m
Aggregate supply	y =	1.40p			+ ϵ^s
Aggregate demand	y =	.05g − .10m − .09p			+ ϵ^d
Standard deviations	ϵ^g	ϵ^m	ϵ^s	ϵ^d	
	.040	.029	.027	.010	

Table 2.A.2 Decomposition of Eight-Quarter Forecast Errors for GNP

Date	GNP	Eg	Es	Em	Ed
1950:1	−0.31	0.53	1.72	−0.17	−2.40
1950:2	1.09	0.29	1.12	0.11	−0.43
1950:3	1.99	−1.68	−0.30	0.16	3.81
1950:4	2.56	−0.76	−2.15	0.01	5.46
1951:1	2.44	0.41	−4.10	−0.27	6.41
1951:2	2.52	1.31	−3.59	−0.39	5.19
1951:3	3.65	2.07	−2.37	0.29	3.66
1951:4	3.05	2.83	−2.06	0.40	1.88
1952:1	1.33	1.39	−2.74	1.55	1.13
1952:2	4.47	4.55	−1.99	2.02	−0.12
1952:3	7.00	4.82	−0.62	2.97	−0.17
1952:4	8.85	5.16	−0.27	3.14	0.81
1953:1	9.98	4.65	1.51	2.70	1.12
1953:2	6.31	3.47	0.44	1.46	0.94
1953:3	2.81	2.85	−0.39	0.47	−0.12
1953:4	1.10	3.09	0.50	−0.85	−1.64
1954:1	−0.39	4.14	−0.56	−1.41	−2.56
1954:2	−2.66	3.27	−0.66	−1.64	−3.62
1954:3	−2.76	1.79	0.25	−1.53	−3.26
1954:4	−0.75	1.17	0.75	−0.70	−1.96
1955:1	0.44	0.03	0.54	0.00	−0.14
1955:2	0.83	−0.55	0.37	0.31	0.70
1955:3	2.08	−0.64	0.75	0.55	1.42
1955:4	1.12	−1.24	0.57	0.32	1.48
1956:1	0.64	−2.01	1.62	−0.21	1.24
1956:2	0.76	−1.60	1.44	−0.34	1.27
1956:3	−0.21	−1.26	0.47	−0.59	1.18
1956:4	0.78	−1.04	0.47	−0.70	2.04
1957:1	0.85	−1.37	0.24	−0.69	2.67
1957:2	0.74	−1.04	0.45	−0.80	2.13
1957:3	0.25	−1.55	0.04	−0.70	2.45
1957:4	−1.44	−1.13	−0.13	−0.86	0.69
1958:1	−4.20	−0.86	−0.54	−1.44	−1.35
1958:2	−4.48	−0.86	−0.55	−1.38	−1.69
1958:3	−2.85	−0.57	−0.25	−0.57	−1.46
1958:4	−1.07	0.30	−0.58	−0.11	−0.68
1959:1	−0.78	−0.14	−1.07	0.30	0.13
1959:2	0.09	0.08	−1.68	0.69	1.00
1959:3	−1.04	0.29	−1.85	0.66	−0.13
1959:4	−1.58	0.42	−1.92	1.46	−1.54
1960:1	−1.60	−0.51	−0.92	0.71	−0.89
1960:2	−3.47	−1.06	−0.78	−0.67	−0.96
1960:3	−5.34	−2.28	−1.06	−1.69	−0.30
1960:4	−6.69	−2.39	−1.14	−1.72	−1.44
1961:1	−5.33	−1.93	−0.20	−1.65	−1.55
1961:2	−3.84	−1.95	−0.05	−0.92	−0.93
1961:3	−4.10	−2.13	0.12	−0.88	−1.21
1961:4	−1.95	−1.84	0.22	0.23	−0.56
1962:1	−0.09	−0.17	−0.59	0.54	0.13

(continued)

Table 2.A.2 (continued)

Date	GNP	Eg	Es	Em	Ed
1962:2	−0.33	0.10	−0.99	0.38	0.17
1962:3	−1.15	−0.11	−0.66	−0.29	−0.10
1962:4	−2.39	0.08	−0.90	−1.02	−0.56
1963:1	−3.32	0.26	−1.36	−1.60	−0.62
1963:2	−2.71	0.06	−0.91	−1.42	−0.43
1963:3	−1.95	−0.03	−0.46	−1.20	−0.26
1963:4	−1.92	−0.03	−0.97	−0.76	−0.17
1964:1	−1.25	−0.70	−0.21	−0.18	−0.15
1964:2	−0.99	−0.38	0.01	−0.12	−0.50
1964:3	−0.76	−0.17	−0.15	−0.07	−0.37
1964:4	−1.01	−0.40	0.20	−0.05	−0.76
1965:1	0.34	−0.31	0.25	0.41	−0.01
1965:2	0.83	0.11	0.27	0.40	0.04
1965:3	1.16	0.22	0.34	0.05	0.55
1965:4	2.55	0.21	1.11	−0.28	1.51
1966:1	2.97	−0.09	0.85	−0.17	2.38
1966:2	2.06	−0.14	0.34	−0.16	2.02
1966:3	2.43	0.61	0.71	−0.08	1.19
1966:4	2.33	1.04	0.92	−0.71	1.08
1967:1	2.22	1.44	1.28	−1.36	0.85
1967:2	1.79	1.72	1.21	−1.88	0.74
1967:3	1.32	1.21	1.09	−2.22	1.24
1967:4	1.07	1.31	0.42	−1.77	1.10
1968:1	1.43	1.47	0.15	−1.09	0.90
1968:2	2.75	1.60	0.67	−0.50	0.98
1968:3	3.00	1.48	0.60	0.50	0.42
1968:4	2.57	1.13	0.20	0.97	0.27
1969:1	2.22	0.66	−0.10	1.51	0.14
1969:2	1.39	0.36	−0.35	1.98	−0.60
1969:3	0.44	−0.19	−0.44	1.82	−0.76
1969:4	−0.90	−0.79	−0.17	1.42	−1.35
1970:1	−1.76	−1.28	0.06	0.99	−1.52
1970:2	−2.69	−1.83	−0.05	0.63	−1.44
1970:3	−1.89	−1.09	0.19	0.28	−1.26
1970:4	−3.37	−1.32	0.14	−0.39	−1.79
1971:1	−1.04	−0.55	−0.01	0.04	−0.52
1971:2	−1.16	−0.22	−0.24	0.26	−0.95
1971:3	−0.94	−0.24	−0.15	1.38	−1.93
1971:4	−0.84	0.03	−0.02	2.07	−2.92
1972:1	0.08	−0.14	0.29	1.91	−1.98
1972:2	0.28	−0.65	0.86	1.84	−1.78
1972:3	0.57	−0.26	0.74	1.50	−1.40
1972:4	1.45	−0.30	1.53	1.05	−0.83
1973:1	3.28	−0.47	1.71	1.57	0.48
1973:2	1.98	−1.00	1.75	1.09	0.14
1973:3	2.00	−0.66	1.55	1.11	0.00
1973:4	2.19	−0.38	1.35	1.08	0.14
1974:1	0.96	0.00	1.03	0.16	−0.23

Table 2.A.2 (continued)

Date	GNP	Eg	Es	Em	Ed
1974:2	−0.46	−0.41	−0.18	0.44	−0.31
1974:3	−1.53	0.41	−1.05	−0.09	−0.81
1974:4	−4.78	−0.63	−2.35	−0.51	−1.30
1975:1	−6.75	0.13	−3.65	−0.30	−2.93
1975:2	−5.90	0.84	−2.88	0.00	−3.87
1975:3	−3.68	1.52	−2.63	0.60	−3.17
1975:4	−3.41	0.84	−2.71	0.88	−2.43
1976:1	−1.96	1.49	−2.14	−0.28	−1.02
1976:2	−1.68	0.80	−1.14	−0.78	−0.56
1976:3	−1.23	0.91	−0.51	−1.19	−0.44
1976:4	−0.92	0.65	0.33	−1.87	−0.04
1977:1	0.10	0.14	1.61	−2.01	0.36
1977:2	−2.03	−1.64	0.90	−1.75	0.46
1977:3	1.31	0.66	1.12	−1.21	0.74
1977:4	1.20	0.68	1.07	−0.81	0.27
1978:1	1.72	1.16	0.81	−0.49	0.23
1978:2	3.65	1.77	0.20	−0.23	1.90
1978:3	3.54	1.66	0.16	−0.11	1.83
1978:4	3.68	1.19	0.25	−0.29	2.53
1979:1	3.65	1.53	−0.01	−0.41	2.54
1979:2	2.47	0.73	0.00	−1.02	2.75
1979:3	2.55	0.17	0.04	−0.61	2.95
1979:4	2.10	0.26	0.52	−0.28	1.60
1980:1	1.83	0.29	0.10	−0.19	1.62
1980:2	−0.42	0.05	−0.45	0.42	−0.44
1980:3	−0.53	0.04	−0.53	−1.30	1.26
1980:4	0.25	−0.09	−0.66	−0.77	1.78
1981:1	2.05	0.27	−0.88	0.08	2.59
1981:2	1.00	0.36	−0.64	−1.05	2.32
1981:3	0.47	−0.21	−1.10	0.07	1.71
1981:4	−1.68	−0.03	−1.51	−0.76	0.61
1982:1	−3.30	−0.37	−1.04	−1.29	−0.58
1982:2	−2.69	−1.11	0.27	0.46	−2.30
1982:3	−4.26	−1.50	0.61	−0.69	−2.68
1982:4	−4.47	−1.41	1.14	−1.40	−2.80

Appendix 2.2
Construction of the Fiscal Index G

The index is derived and discussed in Blanchard (1985). Its empirical counterpart is derived and discussed in Blanchard (1983). This is a short summary.

The Theoretical Index

The index measures the effect of fiscal policy on aggregate demand at given interest rates. It is given by:

$$G_t \equiv \lambda(B_t - \int_t^\infty T_{t,s}\, e^{-(r+p)(s-t)}ds) + Z_t\,,$$

where Z_t,B_t,T_t are government spending, debt, and taxes; $x_{t,s}$ denotes the anticipation, as of t, of a variable x at time s.

The first term measures the effect of fiscal policy on consumption; λ is the propensity to consume out of wealth. B_t is part of wealth and increases consumption. The present value of taxes, however, decreases human wealth and consumption; taxes are discounted at a rate $(r + p)$, higher than the interest rate r. The second term captures the direct effect of government spending.

The index can be rewritten as:

$$G_t = (Z_t - \lambda \int_t^\infty Z_{t,s}e^{-(r+p)(t-s)}ds)$$
$$+ \lambda(B_t - \int_t^\infty (T_{t,s} - Z_{t,s})e^{-(r+p)(t-s)}ds).$$

This shows that fiscal policy affects aggregate demand through the deviation of spending from "normal" spending (first line), through the level of debt and the sequence of anticipated deficits, net of interest payments, $D_{t,s} \equiv (Z_{t,s} - T_{t,s})$.

The Empirical Counterpart

We assume that any time t, D and Z are anticipated to return at rate ξ to their full employment values D^*,Z^* respectively. More precisely:

$$dZ_{t,s}/ds = \xi(Z_t^* - Z_{t,s})$$
$$dD_{t,s}/ds = \xi(D_t^* - D_{t,s}).$$

The index becomes:

$$G_t = Z_t - \lambda\left(\frac{1}{r+p}Z_t^* + \frac{1}{r+p+\xi}(Z_t - Z_t^*)\right)$$
$$+ \lambda\left(B_t + \frac{1}{r+p}D_t^* + \frac{1}{r+p+\xi}(D_t - D_t^*)\right).$$

From the study of aggregate consumption by Hayashi (1982), we choose $\lambda = .08$, $p = .05$, $r = .03$. We choose $\xi = .30$ (all at annual rates). This gives:

$$\tilde{G}_t = .79(Z_t - Z_t^*) + .08B_t + .21D_t + .79D_t^*.$$

Let $\bar{Z}_t$ be the exponentially fitted trend for government spending. The index used in the paper is $G_t = \tilde{G}_t/\bar{Z}_t$. Time series for G_t and its components $(Z_t - Z_t^*)/\bar{Z}_t, B_t/\bar{Z}_t, D_t/\bar{Z}_t, D_t^*/\bar{Z}_t$ are given in table 2.A.3.

Construction of the Fiscal Feedback Rule

Let g, z, z^*, d, d*, t, and t^* be the unexpected components of G, $(Z/\bar{Z})$, $(Z^*/\bar{Z})$, $(D/\bar{Z})$, $(D^*/\bar{Z})$, $(T/\bar{Z})$, and $(T^*/\bar{Z})$. They satisfy, therefore:

$$g = .79(z - z^*) + .08b + .21d + .79d^*.$$

Using $d = z - t$, $d^* = z^* - t^*$ gives:

$$g = z - (.21t + .79t^*) + .08b.$$

Let y and p be, as in the text, the unexpected components of the logarithms of GNP and of the price level. Then

$$\frac{dg}{dy} = \frac{dz}{dy} - .21\frac{dt}{dy},$$

as by definition $\frac{dt^*}{dy} = 0$ and by construction, B being beginning of quarter debt, $\frac{db}{dy} = 0$:

$$\frac{dg}{dp} = \frac{dz}{dp} - .21\frac{dt}{dp} - .79\frac{dt^*}{dp} \approx \frac{dz}{dp} - \frac{dt}{dp},$$

since the effect of unexpected price movements on actual and full employment taxes is approximately the same.

Let σ_1, σ_2 be the elasticities of movements in government spending with respect to unexpected movements in the level of output and in the price level respectively. Let θ_1, θ_2 be similar elasticities for taxes. Then:

$$dg = (\sigma_1 - .21\theta_1)dy$$

$$dg = (\sigma_2 - \theta_2)dp.$$

We assume that, within a quarter, there is no discretionary response of g to either y or p. The response depends only on institutional arrangements. We therefore use the results of deLeeuw et al. (1980) and deLeeuw and Holloway (1982) to construct σ_1, σ_2, θ_1, and θ_2.

σ_1: From table 19 of deLeeuw et al. (1980), a one percentage point increase in the unemployment rate increases spending in the first quarter by 0.6% at an annual rate. From Okun's law it is reasonable to assume that a 1% innovation in output reduces unemployment by roughly 0.1 percentage point in the first quarter. Putting these together we have $\sigma_1 = -0.06$.

σ_2: G is composed of (1) purchases of goods and services, (2) wage payments to government employees, and (3) transfer payments. There is little or no effect of unexpected inflation on nominal purchases within a quarter. Although parts of (2) and (3) are indexed, indexing is not contemporaneous. Nominal payments for some transfer programs (Medicare, Medicaid) increase with inflation. A plausible range for σ_2 is -0.8 to -1.0. We choose -0.9 for the computations in the text.

θ_1: We considered four categories of taxes and income tax bases:(1) personal income tax; (2) corporate income tax; (3) indirect business taxes; (4) social security and other taxes.

We have

$$\theta_1 = \sum_{i=1}^{4} \frac{T_i}{T} \eta_{T_iY_i} \eta_{Y_iY}$$

$\frac{T_i}{T}$ is available in deLeeuw et al. (1980), table 6, for selected years.

η_{Y_iY} is available in ibid., table 8.

$\eta_{T_1Y_1}$ is available in ibid., table 10.

$\eta_{T_2Y_2}$ is available in ibid., 38, col. 1.

$\eta_{T_3Y_3}$ is available in ibid., table 15.

$\eta_{T_4Y_4}$ is available in ibid., table 18.

We calculated θ_1 using elasticities and tax proportions for 1959 and 1979. The results were very close and yielded $\theta_1 = 1.4$.

θ_2: We considered the same four categories of taxes. In the same way as before, we have

$$\theta_2 = \sum_{i=1}^{4} \frac{T_i}{T} \eta_{T_iY_i} \eta_{Y_iP}$$

$\frac{T_i}{T}$ is available in deLeeuw et al. (1980), table 6.

$\eta_{T_iY_i}$ are given in deLeeuw and Holloway (1982), table 8. (They are lower than the $\eta_{T_iY_i}$ reported above for the computations of θ_1.)

η_{Y_iP} are given in ibid., table 7.

We calculated θ_2 using elasticities and tax proportions for 1959, 1969, and 1979. The results were very close. A plausible range for θ_2 (de-

Table 2.A.3 Fiscal Index and Its Components

Date	G	$(Z\text{-}Z^*)/\bar{Z}$	$B/\bar{Z}$	$D/\bar{Z}$	$D^*/\bar{Z}$
1947:1	0.238	−0.003	7.788	−0.560	−0.533
1947:2	0.225	−0.003	7.521	−0.515	−0.527
1947:3	0.280	−0.003	7.216	−0.396	−0.450
1947:4	0.141	−0.003	6.877	−0.523	−0.550
1948:1	0.165	−0.003	6.654	−0.466	−0.506
1948:2	0.253	−0.003	6.472	−0.373	−0.397
1948:3	0.354	−0.003	6.257	−0.248	−0.275
1948:4	0.408	−0.002	6.219	−0.186	−0.219
1949:1	0.447	0.002	6.212	−0.119	−0.191
1949:2	0.501	0.012	6.201	−0.030	−0.153
1949:3	0.513	0.020	6.144	−0.005	−0.145
1949:4	0.486	0.024	6.099	−0.007	−0.177
1950:1	0.582	0.017	6.071	0.007	−0.050
1950:2	0.347	0.009	5.976	−0.285	−0.252
1950:3	0.218	0.002	5.762	−0.474	−0.332
1950:4	0.171	0.000	5.596	−0.477	−0.367
1951:1	0.156	−0.002	5.353	−0.478	−0.352
1951:2	0.318	−0.006	5.258	−0.265	−0.187
1951:3	0.459	−0.006	5.187	−0.111	−0.041
1951:4	0.466	−0.004	5.095	−0.056	−0.036
1952:1	0.424	−0.006	5.057	−0.093	−0.073
1952:2	0.490	−0.006	5.020	−0.014	−0.006
1952:3	0.551	−0.006	4.951	0.058	0.062
1952:4	0.505	−0.008	4.872	−0.015	0.034
1953:1	0.535	−0.009	4.834	0.000	0.074
1953:2	0.555	−0.009	4.808	0.033	0.094
1953:3	0.513	−0.009	4.768	0.022	0.049
1953:4	0.539	−0.002	4.757	0.129	0.048
1954:1	0.504	0.009	4.674	0.105	0.011
1954:2	0.431	0.012	4.626	0.035	−0.060
1954:3	0.409	0.014	4.605	0.009	−0.080
1954:4	0.361	0.010	4.539	−0.045	−0.114
1955:1	0.325	0.008	4.462	−0.104	−0.134
1955:2	0.276	0.003	4.394	−0.151	−0.170
1955:3	0.285	0.002	4.328	−0.148	−0.150
1955:4	0.250	0.002	4.246	−0.177	−0.175
1956:1	0.225	0.000	4.156	−0.175	−0.195
1956:2	0.225	0.002	4.067	−0.162	−0.188
1956:3	0.216	0.001	3.969	−0.151	−0.190
1956:4	0.194	0.001	3.884	−0.170	−0.202
1957:1	0.215	0.000	3.793	−0.139	−0.170
1957:2	0.224	0.000	3.730	−0.114	−0.158
1957:3	0.214	0.001	3.647	−0.114	−0.162
1957:4	0.236	0.008	3.621	−0.059	−0.152
1958:1	0.288	0.022	3.586	0.030	−0.119
1958:2	0.298	0.035	3.556	0.090	−0.130
1958:3	0.361	0.032	3.515	0.088	−0.042
1958:4	0.349	0.023	3.481	0.058	−0.037
1959:1	0.258	0.016	3.434	−0.033	−0.115

(*continued*)

Table 2.A.3 (continued)

Date	G	$(Z\text{-}Z^*)/\bar{Z}$	$B/\bar{Z}$	$D/\bar{Z}$	$D^*/\bar{Z}$
1959:2	0.208	0.010	3.391	−0.092	−0.150
1959:3	0.220	0.011	3.360	−0.056	−0.142
1959:4	0.211	0.013	3.317	−0.061	−0.148
1960:1	0.106	0.009	3.261	−0.171	−0.241
1960:2	0.132	0.010	3.222	−0.126	−0.216
1960:3	0.150	0.012	3.176	−0.091	−0.198
1960:4	0.160	0.019	3.142	−0.059	−0.197
1961:1	0.195	0.023	3.116	−0.021	−0.163
1961:2	0.212	0.025	3.072	−0.012	−0.141
1961:3	0.201	0.021	3.029	−0.025	−0.142
1961:4	0.204	0.016	3.013	−0.042	−0.127
1962:1	0.241	0.012	2.976	−0.007	−0.081
1962:2	0.224	0.011	2.954	−0.025	−0.094
1962:3	0.210	0.011	2.936	−0.037	−0.107
1962:4	0.205	0.011	2.902	−0.030	−0.110
1963:1	0.181	0.012	2.874	−0.050	−0.132
1963:2	0.150	0.011	2.857	−0.087	−0.159
1963:3	0.162	0.009	2.834	−0.081	−0.141
1963:4	0.172	0.009	2.794	−0.068	−0.126
1964:1	0.206	0.008	2.767	−0.045	−0.085
1964:2	0.240	0.007	2.740	−0.011	−0.047
1964:3	0.196	0.005	2.705	−0.049	−0.086
1964:4	0.175	0.004	2.680	−0.063	−0.104
1965:1	0.142	0.003	2.639	−0.107	−0.128
1965:2	0.149	0.002	2.608	−0.103	−0.115
1965:3	0.212	0.000	2.575	−0.044	−0.046
1965:4	0.227	−0.001	2.537	−0.044	−0.022
1966:1	0.202	−0.002	2.488	−0.074	−0.037
1966:2	0.184	−0.002	2.436	−0.080	−0.052
1966:3	0.213	−0.003	2.399	−0.046	−0.019
1966:4	0.227	−0.004	2.361	−0.028	−0.001
1967:1	0.263	−0.003	2.332	0.022	0.035
1967:2	0.263	−0.004	2.310	0.027	0.037
1967:3	0.264	−0.004	2.274	0.028	0.042
1967:4	0.259	−0.003	2.279	0.019	0.037
1968:1	0.235	−0.003	2.278	−0.005	0.013
1968:2	0.257	−0.005	2.277	0.005	0.040
1968:3	0.195	−0.005	2.220	−0.057	−0.015
1968:4	0.166	−0.006	2.210	−0.077	−0.044
1969:1	0.100	−0.006	2.181	−0.144	−0.106
1969:2	0.091	−0.005	2.143	−0.143	−0.113
1969:3	0.104	−0.005	2.050	−0.112	−0.093
1969:4	0.095	−0.005	2.041	−0.101	−0.106
1970:1	0.110	−0.005	2.031	−0.070	−0.094
1970:2	0.164	−0.003	2.002	−0.004	−0.041
1970:3	0.169	0.002	1.958	0.003	−0.036
1970:4	0.179	0.006	1.950	0.032	−0.035
1971:1	0.188	0.011	1.953	0.021	−0.025
1971:2	0.210	0.013	1.917	0.050	−0.003

Table 2.A.3 (continued)

Date	G	$(Z\text{-}Z^*)/\bar{Z}$	$B/\bar{Z}$	$D/\bar{Z}$	$D^*/\bar{Z}$
1971:3	0.206	0.013	1.910	0.048	−0.007
1971:4	0.201	0.014	1.938	0.040	−0.015
1972:1	0.166	0.013	1.944	−0.006	−0.047
1972:2	0.207	0.012	1.923	0.025	0.000
1972:3	0.164	0.009	1.885	−0.017	−0.035
1972:4	0.230	0.007	1.869	0.040	0.038
1973:1	0.180	0.005	1.886	−0.032	−0.008
1973:2	0.159	0.004	1.871	−0.042	−0.028
1973:3	0.127	0.002	1.817	−0.065	−0.054
1973:4	0.128	0.001	1.772	−0.060	−0.048
1974:1	0.109	0.000	1.751	−0.056	−0.070
1974:2	0.118	0.001	1.707	−0.034	−0.059
1974:3	0.086	0.002	1.647	−0.044	−0.089
1974:4	0.106	0.006	1.605	0.003	−0.075
1975:1	0.146	0.019	1.584	0.076	−0.054
1975:2	0.327	0.037	1.599	0.242	0.112
1975:3	0.226	0.038	1.625	0.132	0.007
1975:4	0.218	0.037	1.638	0.122	0.000
1976:1	0.200	0.036	1.673	0.088	−0.017
1976:2	0.176	0.033	1.706	0.062	−0.042
1976:3	0.182	0.030	1.722	0.068	−0.035
1976:4	0.191	0.027	1.714	0.077	−0.022
1977:1	0.153	0.026	1.722	0.026	−0.056
1977:2	0.170	0.022	1.716	0.034	−0.032
1977:3	0.200	0.019	1.685	0.058	0.005
1977:4	0.188	0.018	1.699	0.051	−0.008
1978:1	0.175	0.014	1.699	0.037	−0.016
1978:2	0.139	0.010	1.687	−0.016	−0.043
1978:3	0.124	0.010	1.658	−0.028	−0.055
1978:4	0.118	0.008	1.644	−0.039	−0.057
1979:1	0.089	0.007	1.624	−0.062	−0.084
1979:2	0.068	0.005	1.593	−0.070	−0.101
1979:3	0.091	0.005	1.559	−0.048	−0.074
1979:4	0.103	0.006	1.550	−0.030	−0.063
1980:1	0.106	0.006	1.540	−0.022	−0.060
1980:2	0.117	0.013	1.518	0.019	−0.062
1980:3	0.125	0.019	1.491	0.035	−0.058
1980:4	0.110	0.018	1.487	0.020	−0.071
1981:1	0.062	0.021	1.468	−0.039	−0.117
1981:2	0.063	0.027	1.480	−0.035	−0.124
1981:3	0.078	0.025	1.442	−0.019	−0.102
1981:4	0.099	0.020	1.434	0.024	−0.081
1982:1	0.099	0.028	1.446	0.041	−0.094
1982:2	0.099	0.035	1.458	0.043	−0.104
1982:3	0.146	0.046	1.449	0.094	−0.068
1982:4	0.204	0.051	1.502	0.157	−0.022

pending on which $\eta_{T_iY_i}$ are used) is 0.1 to 0.3. We choose 0.2 for computations in the text.

Our fiscal policy rule is therefore: $g = -.34y - 1.1p + \epsilon^g$.

Comment Robert J. Shiller

These are intriguing questions: Is the macroeconomy disturbed by very large shocks occasionally or by small shocks regularly? Is there a single source of shocks to the economy, or are there many? Is the pattern of behavior constant through time or changing? These questions are not stated in terms of an explicit model and seem to call for some sort of exploratory data analysis. The authors have done this in an imaginative and careful manner, showing thoughtful attention to methods. They took the trouble to do Monte Carlo work to get empirical distributions for statistics for which no distribution theory is available, to use modern robust regression techniques instead of the usual ordinary least squares, and even to create new formal techniques that capture some motivation of popular exploratory techniques.

They rightly perceive that to answer the questions posed here one really would like to have a model of the propagation mechanism for the shocks. Part of their work involves constructing such a model. The model, however, is not used throughout the paper, and some techniques used here are really in the nature of data description.

The authors refer to a "common framework of analysis" in modern macroeconomics that uses stochastic difference or differential equations to model the propagation of shocks. This framework is not enough by itself to suggest any way of studying the kurtosis of the extraneous shocks that strike the economy. We need to know more; we need to know the structure of the model. We can, of course, observe the residuals in an autoregression, as are shown in the authors' table 2.1. But what do these residuals mean? Suppose the true model is a continuous time stochastic differential equation of the form $dX_t/X_t = dW_t + .5dt$, where W_t is a unit Wiener process. Then $X_t = c^*\exp(W_t)$ is a lognormal variable whose kurtosis increases with t. Here the underlying shocks are all normal, but the propagation mechanism creates a variable X_t with an arbitrarily high kurtosis. (The kurtosis of a lognormal variable goes to infinity as the variance of the underlying normal variable is increased to infinity.)

Nor is there any way in the absence of a model to study whether the economy is dominated by a single shock or by many shocks. Even

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if we knew that the vector X_t is determined by a first-order vector autoregression model, we might have problems. Suppose all components of the vector error term were in fact zero except for the first; that is, the economy is driven by a single shock. That is, $X_t = BX_{t-1} + u_t$, where B is a matrix and $u_t = [u_{1t}, 0]$. We could discover that u_t has this special form by regressing X_t on X_{t-1} and looking at the variance matrix of residuals. However, if the special model holds for quarterly data but we had semiannual data for estimation, we would not observe this special structure for the error term. With data for every other quarter we would observe $X_t = B^2X_{t-2} + v_t$, where $v_t = u_t + Bu_{t-1}$. In general, none of the elements of v_t will have zero variance.

The model presented here is therefore of primary importance to the paper. The model is described not inappropriately as a "standard macroeconomic model," though not all the coefficient restrictions are "standard." The equations look something like standard textbook equations with lagged endogenous variables and error terms added. The policy variables are represented by fiscal and monetary reaction functions. The fiscal policy index g is unusual; it is a mixture of government expenditure, the national debt, and the deficit.

How are the equations identified? The identifying restrictions are almost those of the standard recursive model in which the matrix of coefficients is triangular and the variance matrix diagonal. Recall that in that system the diagonality of the variance matrix is necessary for identification of all equations in the model. Also, in the recursive system all equations can be estimated consistently by ordinary least squares. This system differs from the recursive system only in that the equation that in the recursive system would contain only one endogenous variable is an equation with three endogenous variables but with all of the coefficients assumed known a priori. This equation is the fiscal policy reaction function, the equation that determines g. The coefficients are specified before estimation by a clever use of some institutional data on government reactions. Now the model is identified, but the equations cannot be estimated by ordinary least squares. Since the matrix of coefficients of the endogenous variables is not triangular, we must instrument the regressions.

Even so, the assumption that the error terms are uncorrelated across equations is necessary for identification. I wonder if that is a reasonable assumption. Where did the assumption come from? One way of appreciating the arbitrariness of such an assumption is to note that if the model holds for monthly data, say, with error terms uncorrelated across equations, then the quarterly data will not generally have a representation with error terms uncorrelated across equations.

Since lack of correlation of residuals across equations was used to identify the model, it follows that if we doubt these restrictions we can take linear combinations of these equations as we please so long as these combinations do not violate zero restrictions on the coefficients. The authors are concerned that the coefficient of M in the aggregate demand equation has the wrong sign. One might, for example, add the money supply equation to this equation, thereby rectifying the wrong sign.

Another identifying assumption is that monetary policy does not depend on the contemporary fiscal policy variable. Why is this omitted? Does the Fed always ignore fiscal policy? How much trust do we wish to place in the high kurtosis of the monetary policy error term shown in table 2.4? Without the identifying assumptions above, the error term in the monetary policy equation could be any linear combination of the error terms in the other equations.

The assumptions used here for estimation might be compared with those of Hall in this volume (chap. 4). Hall felt that he could find no more than one truly exogenous variable for estimation of such a model, this variable being his military expenditure variable. The discussion of his paper questioned whether even this variable was exogenous. The assumed exogeneity here of residuals of equations lower in the hierarchy is certainly even more questionable than the assumption made by Hall.

The authors, inspired by the methods of Burns and Mitchell for describing business cycles, offer a formalization of their approach. As an exploratory technique, the Burns and Mitchell reference cycle approach has apparently had a wide appeal. It was a dominant theme in empirical macroeconomics for at least a decade after their book appeared in 1946, and the calculation of the reference cycle dates continues to be a widely publicized activity of the NBER. Like most statistical methods belonging in the realm of exploratory methods it is controversial, and the motivations for the techniques are only intuitive. Blanchard and Watson borrowed from the Burns and Mitchell approach and also modified it.

It is interesting to see a formalization of the reference cycle dating itself. It has sometimes been claimed that, while no simple formula is used to date the reference cycle, in fact a recession is declared whenever real GNP shows two consecutive quarters of decline. Blanchard and Watson have found that the quarter of a trough can be identified instead as any quarter in which real GNP declined from the previous quarter so that it is at least 0.5% below the previous peak and then increased for the each of the next three consecutive quarters.

Rather than present the Burns/Mitchell charts of cyclical patterns that were used to judge the business cycle pattern of series, the authors offer a summary measure. This measure is rather different from Burns and Mitchell's own "index of conformity," which relied on a sort of time deformation that in effect reduced all cycles to the same length and that depended only on the directions of change at various phases of the cycle and not on their magnitude. The authors instead present for each variable in each cycle five correlation coefficients: cross-correlations between the variable and real GNP contemporaneously and for two leads and lags. By fixing the leads and lags in terms of quarters rather than fractions of cycles, Blanchard and Watson's lead or lag may be a much higher proportion of the cycle for short cycles, such as that between 1960:2 and 1982:4, which was only ten quarters long. The authors' interpretation of the correlation coefficients relies on a sort of approximation whose validity is not independent of the length of the cycle. A procyclical variable will tend to show a correlation pattern that is not quite flat. There is probably a tendency for the peak of the specific cycle to occur, relative to the reference cycle, as indicated by these leads or lags, but that is not necessarily so. There is a fundamental difference between such correlation coefficients and methods involving identification of peaks and troughs. For example, all these correlations would be about zero for any series that was ninety degrees out of phase with the business cycle. These correlations would stay zero if the series were to switch sign in another business cycle, and so a dramatic change in the cyclical behavior would not be revealed by a change in correlations.

The correlations seem to serve well enough, however, to show to what extent the behaviors of the various series are directly "procyclical" or "countercyclical." What is immediately apparent in looking at the correlation coefficients is that there are not a lot of regular patterns to be seen. There is no series that shows a simple procyclical or countercyclical pattern in all seven cycles.

The authors characterize Burns and Mitchell (who studied cycles between 1854 and 1933) as having found substantially less variation in the cyclical pattern across cycles. That conclusion may be overstated. In looking at the Burns and Mitchell pictures one usually finds for any specific series at least one business cycle with an anomalous pattern. Only two of the Blanchard/Watson series appear to correspond approximately to series studied by Burns and Mitchell: these are the long-term and short-term interest rates. Plots very similar to those of Burns and Mitchell appear in the Zarnowitz/Moore paper, "Major Changes in Cyclical Behavior" in this volume (chap. 9). Comparing their tables 9.3 and 9.6 with their tables 9.4 and 9.7 suggests that conformity to the cycle has declined since World War II only for the short rate.

Both Blanchard and Watson and Zarnowitz and Moore find that short rates are procyclical, show good conformity to the cycle, and lag the reference cycle somewhat. Both papers find less conformity of long rates to the cycle. However, Blanchard and Watson find that the long rate is countercyclical and tends to lead the business cycle by at least two quarters. This last conclusion is not in Zarnowitz and Moore. One might note that it hardly seems likely that the short rate should be procyclical and the long rate countercyclical, since the two series are fairly positively correlated. This points up some of the rather important differences between the Zarnowitz/Moore and Blanchard/Watson approaches. The correlation patterns shown in Blanchard and Watson's table 2.7 are not totally dissimilar for long and short rates, but their identification of leads or lags or procyclical or countercyclical behavior depends on which correlation coefficient is biggest in absolute value. Of course, a procyclical variable that leads the cycle by x degrees can always be described as countercyclical and as leading the cycle by $180 - x$ degrees. With an average duration of postwar cycles of about four years, such calculations suggest that the long rate leads by over a year, which is beyond the range for which they computed correlation coefficients. (They could not have computed correlation coefficients out much further on all cycles, since one postwar cycle is only eleven quarters long.)

The summary statistics provided by Blanchard and Watson, the difference between the maximum and minimum correlation coefficient over the seven cycles, are generally above one for all series but the short rate. This documents the observation that for most variables there is at least one business cycle where the variable shows anomalous behavior.

The authors rightly felt that some sort of significance test for the statistic was desirable. They wanted to know whether differences of this magnitude should be "surprising." They then computed for each series an empirical distribution for this statistic for data generated according to the estimated fourth-order bivariate autoregression representation for the series and real GNP. Since the null hypothesis for this test allows a bivariate autoregression process for which the pattern of cycles may be either usually very similar across cycles or usually very different across cycles, it is not clear in what sense we could expect the power function for this test to divide important alternative hypotheses.

Comment Peter Temin

The paper by Blanchard and Watson is extremely interesting. The authors deserve our thanks for their efforts to take an informal con-

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troversy in the literature and endow it with enough shape to be testable. As any historian knows, there are pitfalls in that process. But the effort itself is necessary, even if it only provides a forum for others to disagree. By moving the discussion out of the realm of slogans into the arena of testable hypotheses, the authors have done a great service.

The question in the title turns out to have two (related) meanings. The obvious meaning, whether all business cycles look alike as they develop, is treated in the second part of the paper. A more subtle formulation, whether all business cycles have the same cause, is the concern of the first part. After a brief comment on the former topic, I will concentrate on the latter.

Postwar business cycles, according to Blanchard and Watson, are not all alike. But, they continue, little information is contained in their differences. These differences do not indicate that business cycles are caused by identifiable, discrete large shocks. It is just that the economy is not a tight enough system to force all fluctuations into a single fixed mold. This result then poses a further question for us to ponder—namely, whether the variance Blanchard and Watson found is truly random or the result of a regular process more complex than their model can capture.

Causation of business cycles is treated in an elegant way. The authors distinguish between proponents of so-called large-shock theories and small-shock theories of business cycles. Large shocks by their nature are identifiable and different one from another; they are the stuff of economic history. Small shocks, by contrast, are nameless and individually uninteresting. It is only their distribution over time that matters for the origin of cycles, which are then produced by the interaction of the economy's internal dynamics and the accumulation of small shocks. The shocks that we observe are themselves, the authors imply, the sum of many smaller shocks that we cannot even see in the data, much less describe or verify from other sources.

My comments will deal both with the authors' methods and with their conclusions. I will start with methodology and work toward substance.

The first point follows from Blanchard and Watson's interpretation of the imprecise distinction between large and small shocks. A discovery that the distribution of innovations has fatter tails than a normal distribution provides evidence of large shocks, in their parlance, but it may not capture the earlier discussion. Disturbances large and infrequent enough not to be captured by a normal distribution may still be too small and frequent to be noted by the proponents of large shocks. In fact, independent one-quarter shocks may be too limited in time to be counted as large. A different error structure and a different definition of large might well produce a different answer.

The factual basis of this investigation consists of precisely four quarterly time series. There are casual references to the rest of the world, but this is an exercise undertaken with strict rules. It is like classical geometry, in which the student is limited to what he or she can prove with straightedge and compass. Or like black-and-white photography in which the drama of color is eschewed in favor of a concentration on light and form. But since, unlike these two examples, there are no generally accepted guidelines to indicate which "stylized facts" should be included and which omitted, it is appropriate to ask if the use of four series seems attractive.

One alternative can be rejected immediately. The use of a large forecasting model would hopelessly complicate the issue without much anticipation of gain. There are so many errors in those models, with so many special properties, that a simple characterization of them as coming from a simple, stationary distribution would be impossible. Another alternative, though, appears more attractive. Why not five series, or six? The gains are not as impressive as those claimed by Bob Solow in the transition from one-sector to two-sector growth models, but they may not be negligible either. The authors provide us with one cautionary tale along these lines. They say that they cannot decompose prices into raw materials prices and other prices in order to examine more closely oil price shocks and related phenomena, because the errors relating to those shocks cannot be disentangled from other price shocks. The gain in information is only apparent, not real.

This argument does not seem, at least on its face, to be applicable to the omission of an interest rate, q, or other series reflecting the cost of capital. I am prepared to hear that there are good statistical reasons for omitting this kind of variable, but it gives me pause. True, the aggregate demand equation is derived from IS and LM curves, and its errors (innovations) are linear combinations of the errors in the underlying IS and LM curves. If errors in these two relationships are uncorrelated, little is lost. But if, as the authors suggest, there is a tendency for innovations to cluster in time, the aggregate demand innovations will miss offsetting innovations in the fiscal and monetary areas.

Blanchard and Watson, then, limit themselves to a simple aggregate demand/aggregate supply model. There are two other equations, but they do not provide additional detail about the functioning of the private economy. They are policy feedback rules. So if we compare this model with, say, Klein's simple models of the economy in the infancy of modern econometrics, we discover two differences. First, as already noted, it gives explicit attention to both fiscal and monetary innovations, even if information about them is limited. And second, it incorporates rules for government policy formation.

The second differences affects the identification of shocks no less than the first. Monetary and fiscal shocks are deviations from the assumed rules of behavior, not from constancy or some smooth path over time.

The policy rules are themselves of interest. The fiscal rule is calculated, not estimated. It reveals a countercyclical fiscal policy: automatic stabilizers. The fiscal policy variable was constructed to show various effects of government spending and debt. It contains a cyclical component, and the fiscal policy equation can be thought of as cleansing it. That is, the equation for g separates the normal cyclical movements of fiscal policy variables from "true shocks." This seems like an awkward process in which the cyclical components of fiscal policy are first built into g by construction and then removed again by calculation. It was indicated, apparently, to distinguish changes in discretionary fiscal policy from cyclical movements in the government budget while allowing both to influence subsequent events.

The monetary policy rule, by contrast, is estimated. It shows exactly the opposite pattern. Whereas fiscal policy was "normally" countercyclical, monetary policy was procyclical. It was, in other words, accomodating. This is in itself an important conclusion. It may be an accurate description of monetary policy in the immediate postwar years, but it hardly seems to describe the role of the Fed in recent years. The use of a single model for the entire period from 1947 through 1982 cannot uncover such a shift. It would be interesting to see what would happen if the sample was broken at some point in time.

This procedure would provide information about changes both in policy rules and in shocks. (Were the 1970s really worse than the 1960s?) If there was a structural shift midway through the sample, its absence would tend to magnify the apparent shocks, particularly at the ends of the period. An informal look at table 2.4 suggests that something like this may indeed be going on.

Examining table 2.4 moves us from methodology to substance. As Blanchard and Watson comment, the most striking feature of table 2.4 (which shows the structural disturbances) is its lack of intuitive appeal. Despite considerable ingenuity, the authors succeed in making only one positive identification between their calculated innovations and the economic history of the postwar years (in 1974–75). I do not count large disturbances of one sign followed immediately by equally large disturbances of the opposite sign or disturbances that can be assigned to a known cause only by Procrustean tailoring of the date. There are sixty large disturbances shown in table 2.4, and it would be surprising if chance alone did not place a few of them on the dates of historical events. There does not seem to be much correlation between the calculated disturbances and our historical memory.

This is due partly, no doubt, to the matter of definition and procedure mentioned earlier. But to the extent that it is not, this result forces us to choose between the formal history of Blanchard and Watson and the more informal history of Eckstein and Sinai. How should one choose?

To aid this choice, I compared the structural disturbances in table 2.4 with deviations of the four variables from simple time trends. The differences are striking. The large disturbances of table 2.4 do not look autocorrelated. And the distributions of the four disturbances do not appear very different. Compared with a time trend, however, the variables divide into two clear groups. M1 and the GNP deflator (*m* and *p*) have highly autocorrelated disturbances, with little variation from one quarter to the next. GNP and the fiscal index (*y* and *g*), by contrast, have far less autocorrelation and far more quarter-to-quarter variation.

M1 and the GNP deflator grew slower than trend until the late 1960s and faster thereafter. (If one were to break the sample into two, as suggested earlier, the turning point of the later 1960s would be the obvious break point.) Neither of the other variables seems to show a break in trend at that time, although the fiscal variable appears to have much larger (relative) variation than GNP.

This pattern can be discerned in Blanchard and Watson's table 2.3, which decomposes forecast errors of their model. First, it is worth noting that the policy innovations seem to have little effect on the rest of the economy. Fiscal shocks affect the fiscal shock and nothing else in the next quarter. Even after a year, they have almost no effect on the other variables. And after five years, the effect of past fiscal shocks on current fiscal shocks is over twice as large as its effect on any of the other variables. Monetary shocks have exactly the same pattern. After five years, there is some effect of monetary shocks on prices, but the effect is only about half as strong as the effect on money itself.

Second, there is a sharp separation between the effect of supply and demand shocks. Supply shocks affect prices, and demand shocks affect output. This is true in the next quarter and remains largely true after a year. (Demand shocks have noticeable effects on prices by then, although supply shocks still dominate.) Only after five years do prices and income appear to be affected by a mixture of influences.

In the short run, therefore, Blanchard and Watson's economy has a roughly horizontal aggregate supply curve and a roughly vertical aggregate demand curve. As the authors note, this conclusion is strongly affected by their derivation from the fiscal equation. They present alternative structural estimates of their model in appendix 2.1, based on different calculated values of the price responsiveness of the fiscal stimulus. They allow the coefficient of *p* in the fiscal equation to vary from 1.0 to 1.3. This makes the price coefficient in the supply equation vary from 0.45 to 1.40—a threefold variation. It follows that the particular short-run characterization of the economy given by Blanchard and Watson may not be the only reasonable description. This in turn implies that the pattern of variances shown in table 2.3 may not be the only one generated by reasonable constructions of the economy.

In the long run this model appears to give more conventional results, showing the joint effect of all the shocks cited. A note of caution may be injected, however, in the use of a very simple model like this one to project the effect of errors over fifty years. The last section of table 2.3 may only be telling us that the relations estimated in table 2.2 are too simple to use in projecting an economy over five years. The authors use this kind of model to look at the short-run errors; there is no reason to think it is the best way to approach problems of longer-run forecasting.

Blanchard and Watson therefore seem to be presenting us with an economy that, at least in the short run, decomposes into several rather independent subeconomies. This surely is different from the economy described by Eckstein and Sinai. It also may well depend on the particular derivation used, as suggested by the alternatives furnished in appendix 2.1. The principal issue they raise, therefore, may not be whether shocks to the economy are generated by a normal distribution, but rather whether the short-run responsiveness of the economy has the simple structure they have attributed to it.

Discussion Summary

The discussion began with a response by Sims to Shiller's comments regarding identification and exogeneity. Sims stated that the identification of any stochastic model requires that restrictions be placed on the model's error process. Usually investigators make assumptions about the exogeneity of certain variables entering the model. Blanchard and Watson's approach, however, is to regard nothing as totally exogenous. This forces them to turn to exclusion restrictions and explicit assumptions on the error terms, in particular a diagonal covariance matrix for the contemporaneous shocks, to achieve identification. Shiller's point that the use of the wrong sampling period can induce contemporaneously correlated errors applies equally to incorrect identifying restrictions. Such a criticism does not mean that identification should not be attempted; it means only that identifying restrictions should be used wisely, as Blanchard and Watson have done. Finally Sims drew attention to the sophisticated extension of VAR methodology used by this paper. A vector autoregression was first used to generate "reduced form" residuals. Robust simultaneous equation estimation techniques were then used to transform the reduced-form shocks into shocks arising from a structural model.

Although McCallum felt that the large-shock versus small-shock view of impulses was valuable, he questioned the authors' statements on the associated policy implications. McCallum said he could see no reason why the presence of small shocks necessarily supported policy rules or why large shocks necessarily led to a need for discretionary policy. In reply, Blanchard referred McCallum to the debate between Robert Lucas and Robert Solow in *Rational Expectations and Economic Policy* (Fischer 1980, 249–64). Blanchard felt that the prevailing view of the profession, as espoused by Solow, seemed to be that large shocks were unique events whose source could be readily identified and counterbalanced with specific policies. The sources of small perturbations were harder to isolate, and this argued for built-in automatic stabilizers to offset their effects. Finally, several participants remarked that there might be difficulties in differentiating between true small shocks and a large shock that came in as a succession of small shocks because of the choice of the sampling interval. Blanchard conceded that this was a potential problem, but held that the eight-quarter forecast decompositions of GNP presented in table 2.A.2 did not seem to indicate that this was the case here.

Additional Contribution
Are Business Cycles Symmetrical?

J. Bradford DeLong and Lawrence H. Summers

1. Introduction

The dating of peaks and troughs and the concomitant emphasis on the different qualitative mechanisms involved in cyclical expansions and contractions have been major features of the NBER program on business cycle research. Asymmetry between expansions and contractions has long been a focus of such research. Thus Wesley Mitchell wrote (1927), "the most violent declines exceed the most considerable advances . . . . Business contractions appear to be a briefer and more violent process than business expansions." Keynes wrote in the *General Theory* (1936) that "the substitution of a downward for an upward tendency often takes place suddenly and violently . . . no such sharp

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turning point occurs when an upward is substituted for a downward tendency." Indeed Neftci (1984) states that "the claim that major economic time series are asymmetric over different time phases of the business cycle arises in almost all major works on business cycles."

In many respects the techniques of modern statistics and econometrics surely supersede earlier methods of cyclical analysis. They make possible the application of techniques of statistical estimation and inference. They remove the need for judgment in data description. And they provide a rigorous basis for nonjudgmental forecasting. Yet statistical models of the sort used in economics—whether built in the structural spirit of the Cowles Commission or in the modern time series tradition—are entirely unable to capture cyclical asymmetries. If, as Keynes, Mitchell, and others belived, cyclical asymmetries are of fundamental importance, then standard statistical techniques are seriously deficient. Something like traditional business cycle analysis may then be necessary to provide an adequate empirical basis for theorizing about cyclical behavior.

Hence the question of the magnitude of cyclical asymmetries seems to be of substantial methodological importance. Yet with the exception of the work of Neftci (1984), it appears to have attracted relatively little attention. This paper examines the extent of cyclical asymmetries using American data for the prewar and postwar periods and data on five other major OECD nations for the postwar period. We find no evidence of asymmetry in the behavior of GNP or industrial production. For the United States only, we find evidence of some asymmetry in the behavior of unemployment. We conclude that asymmetry is probably not a phenomenon of first-order importance in understanding business cycles. It appears that there is not much basis for preferring some version of traditional cyclical techniques of analysis and forecasting to more statistical methods.

Section 2 of this note describes our methods and presents the results of our analysis of GNP and industrial production. Section 3 follows Neftci (1984) in considering unemployment. We note some methodological problems we have with his analysis and then show that his conclusions about the behavior of unemployment appear to be invalid outside the United States. Section 4 provides some brief conclusions.

2. Asymmetries in Output?

The essence of the claims of Keynes and Mitchell quoted in the previous section was that economic downturns are brief and severe relative to trend, whereas upturns are longer and more gradual. This hypothesis has a clear implication: there should be significant skewness in a frequency distribution of periodic growth rates of output. That is, the distribution should have significantly fewer than half its observations below the mean; and the average deviation from the mean of the observations below the mean should be significantly more than the average deviation of the observations above the mean. The median output growth rate should exceed the mean by a significant amount. Figure C2.1 depicts the predicted frequency distribution of output growth under the null hypothesis of symmetry and under the alternative hypothesis of Keynes and Mitchell.

Our procedure is simple: it is to calculate the coefficient of skewness of the distribution of output growth rates for a variety of output measures and time intervals. The coefficient of skewness is defined as the ratio of the third centered moment to the cube of the standard deviation. For a symmetric distribution, the coefficient of skewness is zero, and the mean equals the median.

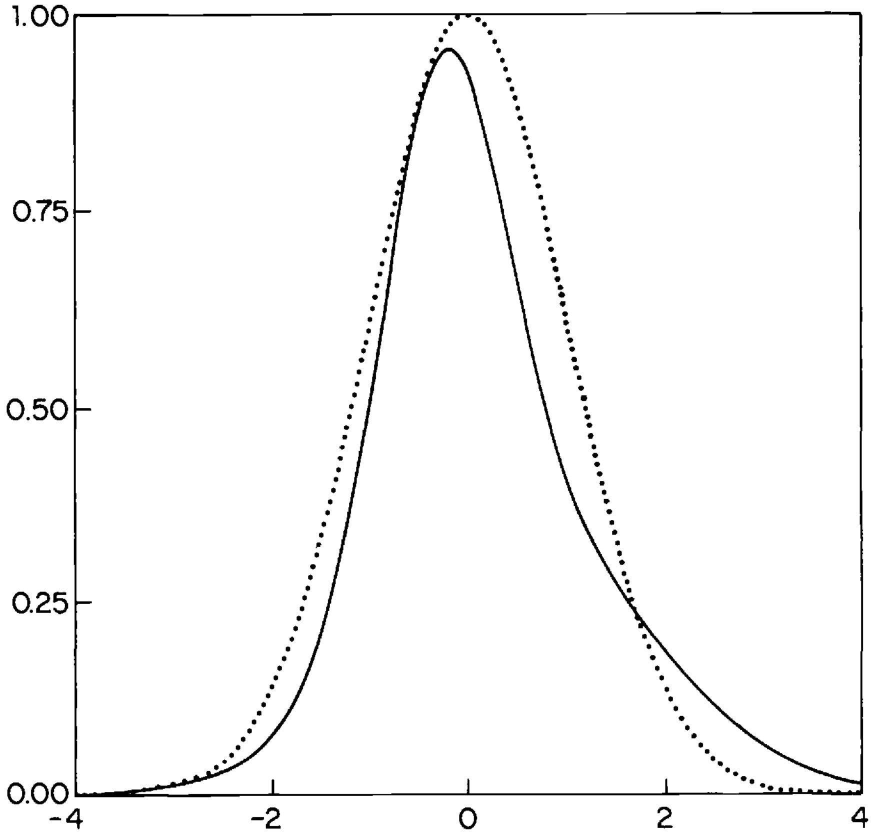

Fig. C2.1 Standardized (zero mean and unit variance) distribution with zero and unit skewness, respectively.

Evaluation of the statistical significance of any measured deviations from symmetry clearly requires an estimate of the sampling variability in our estimates of skewness. Standard statistical works such as Kendall and Stuart (1969) note that under the null hypothesis of zero skewness, the estimated skewness of a set of n independent random normal observations is normally distributed with a standard error of $(6/n)^{1/2}$. Unfortunately, the observations on growth rates considered here are highly serially correlated, and so this formula is inapplicable. We therefore used the following Monte Carlo procedure for each series and sample period considered. First, a third-order autoregression process was estimated for the time series of growth rates. It was then used to generate three-hundred artificial series for the sample period under the assumption that the shocks to the autoregression process were normally distributed. The standard deviation of the estimated skewness under the null hypothesis was then used calculated as the standard deviation of the skewnesses of the artificially generated series.[1]

Table C2.1 presents some evidence on skewness in quarterly and annual growth rates of United States GNP and industrial production

Table C2.1 Skewness of United States GNP and Industrial Production Growth Rates

		Annual Data		Quarterly Data	
Variable	Period	Skewness	Standard Error	Skewness	Standard Error
GNP	1891–1915	− .47	.73	.55	.29
GNP	1923–40	− .70	1.12	.04	.42
GnP	1949–83	−1.37	.74	−.33	.29
IP	1949–83	− .55	.68	−.58	.40

Source: Data from Gordon 1982 and from the 1984 *Business Conditions Digest.*

1. We verified that the estimated skewnesses were approximately normally distributed. Coefficients of kurtosis were less than 10% away from their value of three under the null hypothesis. Note that our test of asymmetry is appropriate if output is stationary either when detrended or when differenced. Our standard errors are calculated under the second assumption, which is weakly supported by Nelson and Plosser 1982. Because they include periods in their analysis like the Great Depression and World War II, during which no one would expect the underlying rate of growth of the economy to stay constant, it is hard to interpret how their warnings against the practice of detrending apply to analyses that deal only with periods for which one has good reason to suspect that the underlying growth of potential output has been approximately constant.

For the United States industrial production index, estimated skewnesses for subperiods of the post–World War II period are highly variable—more variable than the stochastic errors calculated under the assumption of an AR(3) generating process would suggest. Apparently, modeling the generating process as an AR(3) does not capture all the serial dependence in the series and leads to estimated standard errors that are presumably too low. Therefore the standard errors reported in this paper are probably below their actual values.

for various sample periods. We use industrial production as well as GNP because the latter contains a greater number of imputed series, and because cyclicality is most apparent in the manufacturing sector of the economy. Because using quarterly data is complicated by the need for seasonal adjustment and by high-frequency movements that might render existing skewness undectable, both annual and quarterly data are examined.

Very little evidence of significant asymmetries emerges. Before World War II, quarterly GNP growth rates exhibit *positive* skewness, the opposite of that implied by the hypotheses of Keynes and Mitchell. The failure of the steep 1929–33 decline to dominate the interwar period is somewhat surprising. We expected significant skewness to be most apparent around the Great Depression. Similar conclusions are obtained with annual GNP data and with data on annual industrial production for the prewar period. Asymmetries do not appear to be substantial enough to be important. The difference between the median and mean growth rates reaches a maximum of 0.3% using quarterly data on industrial production for the postwar period. This difference is only 2% of the interquartile range of the distribution of quarterly growth rates: it is a very small number.

There is a little bit of evidence in favor of skewness in postwar data. All the estimated skewnesses are negative, as predicted by Keynes and Mitchell. In the case of annual GNP data, the estimated skewness approaches statistical significance. However, no equivalent result is found with either quarterly GNP or annual industrial production data. Hence we are inclined to discount its significance. It is of course possible that with longer time series significant asymmetries would emerge—the estimate of skewness would become sharper. But as figure C2.2 reveals, the observed skewness does not appear to be substantively important. The naked eye cannot easily judge the direction of asymmetry.

As a further check, table C2.2 reports estimated skewnesses of quarterly GNP and industrial production for other major OECD countries for the postwar period. Skewness is noticeably negative for only two of the five countries—Canada and Japan—using either industrial production or GNP data. There is no significant evidence of asymmetry for any country. The only natural grouping suggested by the data is a possible division into the United States, Canada, and Japan on the one hand and the United Kingdom, France, and West Germany on the other. But this possible difference between "non-European" and "European" business cycles is not strongly enough present in the data to give us any confidence that it is anything more than the workings of chance.

How has the picture of recessions as short violent interruptions of the process of economic growth emerged? Part of the answer lies in

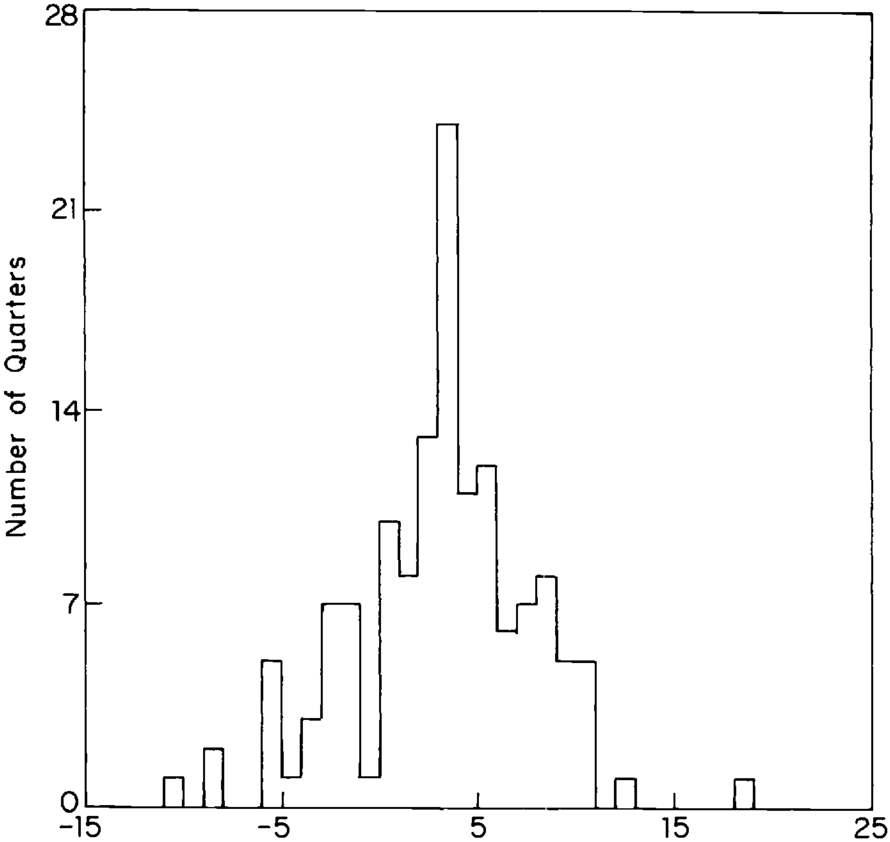

Fig. C2.2 Histogram of quarterly GNP growth rates, 1949–82.

Table C2.2 **Skewness of Quarterly Changes in GNP and Industrial Production, 1950–79**

	Industrial Production		GNP	
Country	Skewness	Standard Error	Skewness	Standard Error
United States	−.61	.42	−.33	.29
Japan	−.66	.40	−.43	.29
Canada	−.52	.39	−.42	.30
West Germany	−.01	.34	−.11	.26
United Kingdom	.13	.35	.61	.27
France	.27	.33	−.03	.24

Source: Data from the OECD *Historical Statistics* and from the 1984 *Business Conditions Digest.*

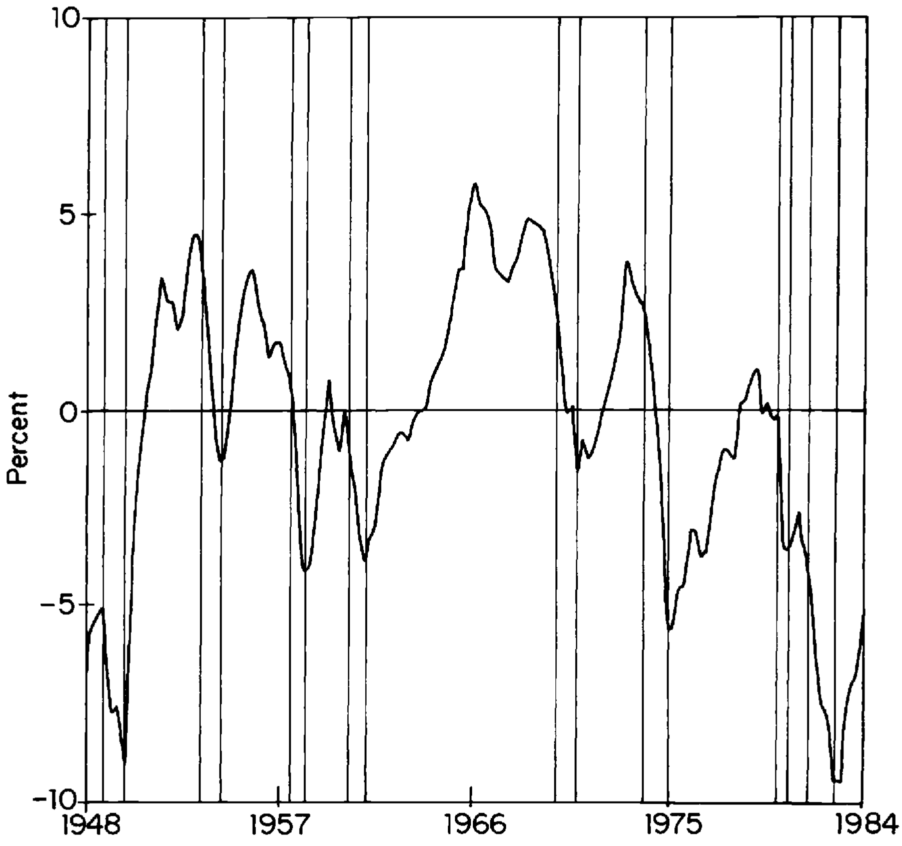

Fig. C2.3 Deviation of United States GNP from its natural rate. NBER reference cycle recessions shaded.

the way economic data are frequently analyzed. Figures C2.3 and C2.4 depict the NBER reference cycles, contractions are definitely shorter than expansions, confirming the judgments of Keynes and Mitchell. But this is a statistical artifact. The superposition of the business cycle upon a trend of economic growth implies that only the most severe portions of the declines relative to trend will appear as absolute declines and thus as reference cycle contractions. Even a symmetric business cycle superimposed on a rising trend would generate reference cycles for which the recessions would be short and severe relative to trend, even though the growth cycles—the cycles in detrended indexes—would be symmetric. As this argument suggests, there is little difference between the lengths of growth cycle expansions and contractions. The difference in length between expansions and contractions for the nine

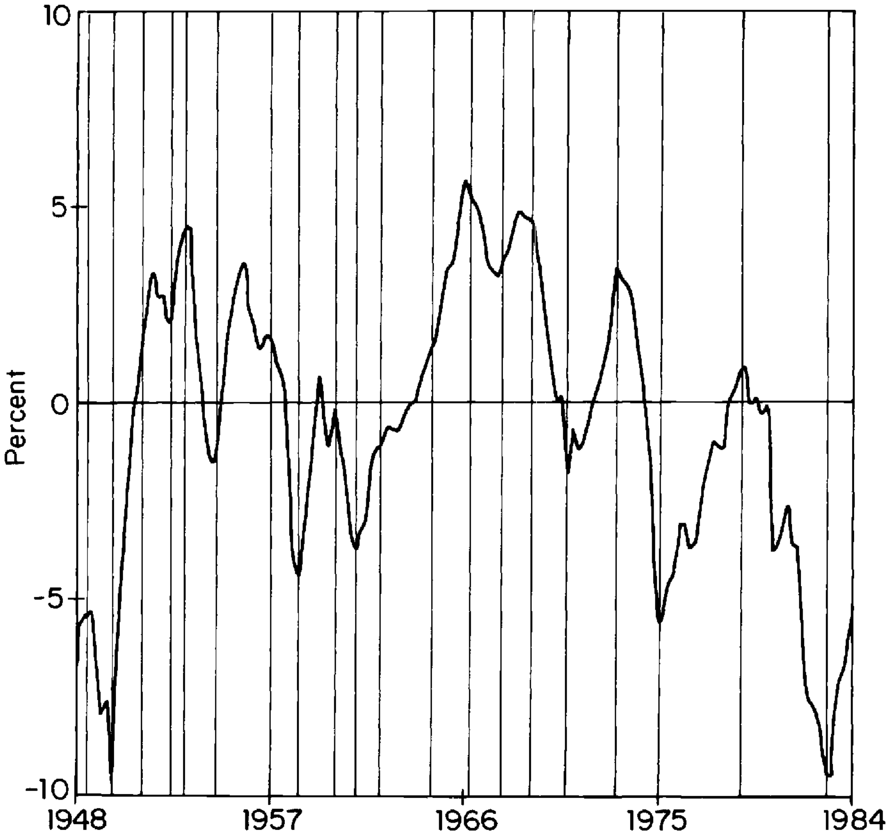

Fig. C2.4 Deviation of United States GNP from its natural rate. NBER growth cycle recessions shaded.

growth cycles averaged 0.9 quarters; the standard deviation of this estimate of the average is 1.4 quarters.[2] By contrast, the average length of the seven reference cycle expansions was 11.4 quarters longer than the length of the subsequent contractions.[3]

We conclude from this investigation that once one takes proper account of trend growth—using either our skewness-based approach or

2. We are assuming that each postwar business cycle is an independent draw from a population characterized partly by the difference in length between the expansion and the recession phase. Cycle dates are taken from Moore and Zarnowitz's "The Development and Role of the National Bureau of Economic Research's Business Cycle Chronologies" (appendix A to this Volume). Note that, as Moore and Zarnowitz report, it was not always the case that expansions were as a rule longer than contractions.

3. With a standard error of the mean of 3.3 quarters. Excluding the highly anomalous 1961–70 reference cycle, the mean difference is 8.1 quarters, and its standard error is 1.8 quarters.

the traditional NBER cycle dating approach—little evidence remains of cyclical asymmetry in the behavior of output. The impression to the contrary that we used to hold seems to result from a failure to take account either imrpessionistically or quantitatively of the effects of long-run economic growth. Few extant theories suggest that business cycles should depend on the rate of underlying growth of either productivity or population.[4] The next section considers whether similar conclusions are obtained using data on unemployment.

3. Asymmetries in Unemployment?

Our conclusions so far contradict those of Neftci (1984), who examines the behavior of the unemployment rate and finds evidence against the null of symmetry at the .80 level. Neftci's statistical procedure seems inappropriate to us: eliminating the quantitative information in the data by reducing it to a series of ones (unemployment increasing) and zeros (unemployment decreasing) cannot lead to a test of maximum power.

Table C2.3 presents estimates of the skewness in detrended unemployment rates for the United States and other major OECD countries for the postwar period. We examine only the postwar data because earlier unemployment estimates are in general not derived independently from output data. For the United States, we confirm Neftci's conclusion. Indeed, we are able to reject the null hypothesis of symmetry at the .95 level. Annual data suggest as much skewness as quarterly data, but the skewness in annual data is not statistically significant.

None of the other OECD countries, however, have statistically significant skewnesses in their detrended unemployment rates.[5] This suggests that skewness in the United States is either a statistical accident or a result of a peculiarity in the United States labor market. Asymmetry in changes in unemployment rates is not a strong general feature of business cycles.

We have briefly attempted to examine the reasons for asymmetry in American unemployment rates. Skewness does not arise from the behavior of labor force participation: labor force participation rates exhibit no noticeable skewness, and skewness is present in detrended

4. But see Schumpeter 1939 for arguments that the cyclical variance of output is itself positively related to the rate of long-run growth.

5. Detrending European unemployment rates is not easy: there appears to have been an enormous rise in structural unemployment rates all over Europe in the past ten years. The results reported used a second-degreee polynomial to detrend the data. The results were effectively unchanged when a third- or a fourth-degree polynomial was used or when a piecewise linear trend with a breakpoint in 1973 was used. If the rise in unemployment is attributed entirely to cyclical factors—if the skewness of raw changes is calculated—then changes in European unemployment rates since 1970 appear strongly skewed.

Table C2.3 **Skewness of Quarterly Changes in Unemployment Rates, 1950–79**

Country	Skewness	Standard Error
United States	1.03	.30
Japan	.40	.28
Canada	.55	.29
West Germany	−.13	.27
United Kingdom	.27	.30
France	.14	.33

Source: Data from the 1984 *Business Conditions Digest*.

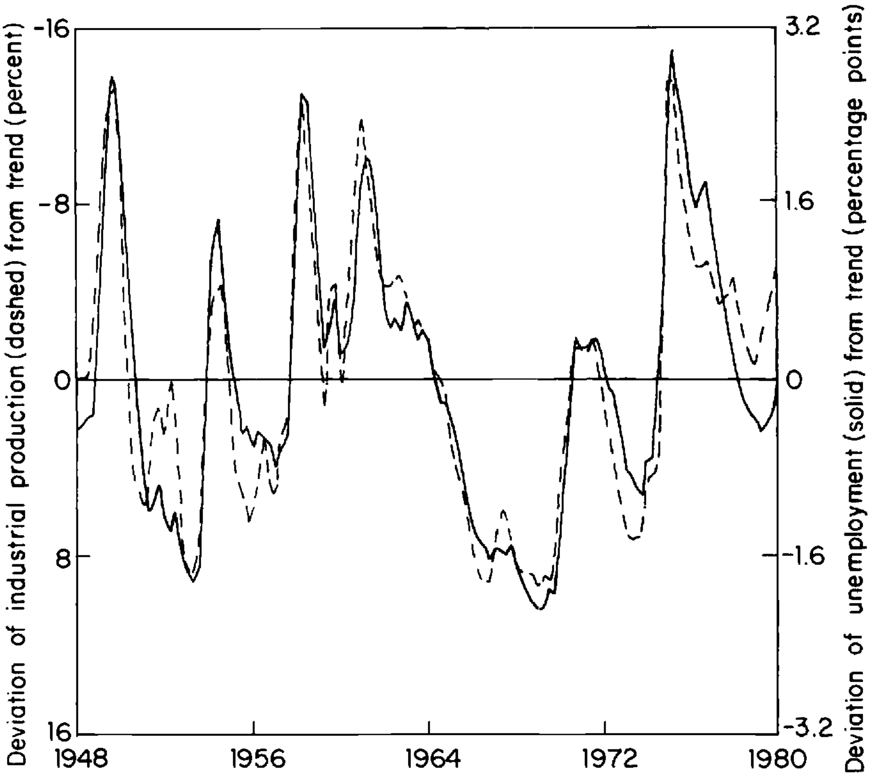

Fig. C2.5 United States industrial production and unemployment, 1948–80.

unemployment numbers as strongly as in detrended unemployment rates. Moreover, quarterly changes in employment over the period 1949–82 exhibit a skewness coefficient of −1.90, significant at the .95 level. Skewness in employment and unemployment but not in GNP clearly indicates a breakdown in Okun's law. In figure C2.5, inverted deviations of industrial production from trend are plotted alongside the detrended unemployment rate. At business cycle peaks—unemployment troughs—the unemployment rate lags behind output measures. Output measures start to decline relative to trend before unemployment starts to rise. There is a period of time, after the growth cycle peak and before the reference cycle peak, during which output is falling relative to trend and employment is still rising relative to trend. This discrepancy in timing appears only near business cycle peaks. At business cycle troughs, the unemployment rate peaks within one quarter of the trough of output measures.

The significant coefficient of skewness found in the United States unemployment rate is apparently another manifestation of the "end of expansion" productivity effect documented in Gordon (1979). According to Gordon, normal equations for raw labor productivity go awry in the quarters after output reaches its maximum relative to trend. The magnitude of this effect can be seen in Gordon's figure 1 (reprinted as fig. C2.6). Output has begun to fall relative to trend; employment is still rising relative to trend; and so raw labor productivity naturally declines sharply. Firms are able to expand their work forces rapidly after business cycle troughs in order to keep pace with rising aggregate demand. Why don't they contract their work forces relative to trend after growth cycle peaks? We suspect that there is an explanation related to the burgeoning literature on labor hoarding (see Medoff and Fay 1983 or Fair 1984, for example), but it is beyond our competence to suggest here what the explanation might be.

4. Conclusion

Our investigation into the possible asymmetry of the business cycle has, in our estimation, failed to turn up significant evidence that the econometric model building approach to business cycles is misguided. We could not find the skewness coefficients we thought we would find; and we therefore conclude that it is reasonable in a first approximation, to model business cycles as symmetric oscillations about a rising trend. GNP growth rates and industrial production growth rates do not provide significant evidence of asymmetry. We therefore think that the main advantage of the econometric model building approach—the body of statistical theory behind it—makes it the methodology of choice for analyzing macroeconomic fluctuations.

Our results call into question at least one possible justification for using reference cycles in studying macroeconomic fluctuations. An alternative justification for the reference cycle approach stresses the commonality of the patterns of comovements in variables across different business cycles. Blanchard and Watson's paper challenges this proposition. Studies of macroeconomic fluctuations using the reference

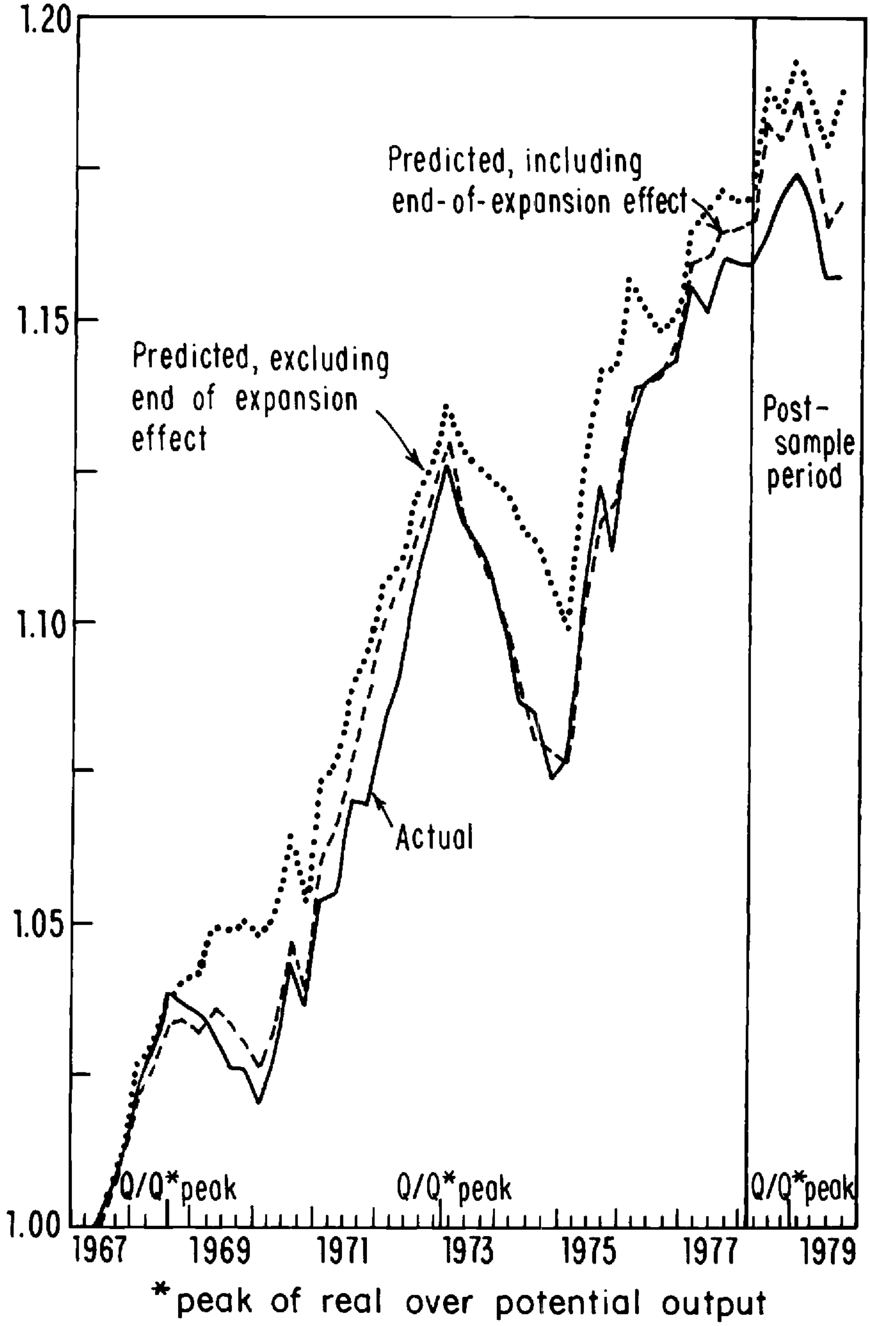

Fig. C2.6 Output per hour in the nonfarm business sector, actual and predicted from alternative equations, 1969:2 to 1979:3. Reprinted from Gordon 1979.

cycle approach are the foundation of empirical macroeconomics. But given the availability of modern statistical methods, there appears to be no scientific basis for the use of reference cycles in either macroeconomic analysis or forecasting. As yet, no phenomenon or regularity has been adduced that can be studied using the reference cycle approach but is inconsistent with the assumptions of standard time series methods. Until such a demonstration is provided, there is little justification for the continued use of reference cycles in studying or forecasting macroeconomic fluctuations.

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