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Productivity Shocks and Real Business Cycles

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Productivity Shocks and Real Business Cycles[1*]

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Abstract

Productivity shocks play a central role in real business cycles as an exogenous impulse to macroeconomic activity. However, measured Solow/Prescott residuals do not behave as an exogenous impulse. Rather, econometric evidence provided in this paper indicates that (1) money, interest rates, and government spending Granger-cause these impulses; and (2) a substantial component of the variance of these impulses (between one quarter and one half) is attributable to variations in aggregate demand. These results are robust to a number of econometric issues, including measurement errors, specification of the production function, and certain forms of omitted real variables.

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1. Introduction

Productivity shocks play a central role in Real Business Cycle theories as an impulse to macroeconomic activity (as in Kydland and Prescott (1982), Hansen (1985), Altug (1985), and King, Plosser, and Rebelo (1988), for example). In characterizing the business cycle properties of these models, and then comparing them with the cyclical properties of the data, these researchers assume that productivity shocks are exogenous and uninfluenced by other economic factors. And yet no evidence currently exists to support this standard Real Business Cycle assumption.

Many critics of Real Business Cycle (RBC) theories question the exogeneity of procyclical productivity shocks; indeed, many theories predict these shocks to be endogenous. For example, Summers (1986) argues that empirical measures of the change in total factor productivity are contaminated by labor hoarding phenomena; consequently, aggregate demand impulses can give rise to a procyclical productivity measure. Mankiw (1989) argues that the large growth in total factor productivity from 1939-1944 is interpreted most plausibly as a demand-driven response to the military buildup of World War II. Hall (1988) finds evidence in annual data that cost-based measures of Solow residuals covary with exogenous instruments: he attributes this endogeneity to noncompetitive forces. Murphy, Shleifer, and Vishny (1989) survey competitive theories with external increasing returns; these theories predict that changes in total factor productivity are endogenous and demand-driven. Caballero and Lyons (1990) find evidence in annual data of external increasing returns in manufacturing. According to these criticisms, measures of productivity shocks which are based upon changes in total factor productivity will not be strictly exogenous.

This paper investigates several quarterly measures of the impulse to an aggregate productivity shock and asks if these measured Solow residuals can survive simple exogeneity tests. The evidence is inconsistent with the hypothesis that the impulse to an aggregate productivity shock is exogenous; consequently, the productivity shock is not exogenous. Initially, in Section 2, the analysis employs Prescott's (1986) measure of the impulse to aggregate productivity. Money, nominal interest rates, and government spending consistently provide significant predictive power for this impulse. These results are economically significant: about one-quarter of the variance of the productivity impulse can be attributed to aggregate demand shocks.[1] The analysis of Sections 3-5 demonstrates that these conclusions are robust to a number of econometric issues. Section 3 considers the possibility of random measurement error in the productivity data: in this case, about one-half of the variance of the productivity impulse can be attributed to aggregate demand shocks. Section 4 considers the possibility of specification errors in the production function; twelve measures of the productivity impulse are considered and the exogeneity test results are unchanged. Section 5 considers the possibility that these results are due to omitted real shocks, along the lines considered by King and Plosser (1984) and Litterman and Weiss (1985). However, the finding that money and nominal interest rates provide predictive power a year in advance of the productivity impulse realization makes this an unlikely explanation. For each possibility, the evidence favors the conclusion that measured aggregate productivity impulses do not behave as a strictly exogenous stochastic process.

These findings indicate that the role of productivity shocks in generating economic fluctuations has been overstated in the RBC literature. Further research aimed at identifying and understanding "productivity shocks" may be an important element in the debate between RBC theorists and their critics.

2. Are Productivity Shocks Exogenous?

Prescott (1986) measures the impulse to the aggregate productivity shock as the change in total factor productivity. Assuming an aggregate Cobb-Douglas production function,

$$Y_t = z_t N_t^{\theta} K_t^{1-\theta} \qquad [1]$$

the productivity shock z_t can be measured using data on output (Y), labor hours (N), and the capital stock (K) for a given labor share parameter θ. Assuming that z_t contains a unit root in logarithms leads to:

$$z_t = z_{t-1} \exp(\mu + \epsilon_t) \qquad [2]$$

$$\epsilon_t = \beta(L)\,\epsilon_{t-1} + w_t$$

where ϵ_t is a stationary random variable, $\beta(L)$ is a polynomial in the lag operator L, and w_t is a mean zero, serially uncorrelated random variable. In Prescott's study, ϵ_t is the measure of technological change. The Real Business Cycle literature has not taken a firm stand on the stochastic process for ϵ_t. Prescott (1986), Altug (1985), Christiano-Eichenbaum (1991), and Braun (1989) assume that ϵ_t is white noise; while Christiano (1988), King-Plosser-Rebelo (1988), and Eichenbaum-Singleton (1986) allow objects like ϵ_t to be serially correlated.[2]

A critical assumption that these papers share is that z_t is an exogenous random variable. These models assume that changes in monetary and fiscal policy variables do not alter the distribution of z_t; consequently, real models like these can usefully "provide a ... well-defined benchmark for evaluating the importance of other factors (e.g., monetary disturbances) in actual business-cycle episodes [Long-Plosser(1983, p.68)]." Alternatively, if z_t is endogenously-determined, as Summers (1986) and the models of Murphy-Shleifer-Vishny (1989) imply, then the omission of fiscal and monetary variables distorts the benchmark assessment. In the context of specification [2], the exogeneity of z_t requires that ϵ_t be exogenous. Thus, the RBC literature relies upon the exogeneity of ϵ_t, but it may be either white noise or a serially correlated random variable.

Using [1] and [2], ϵ_t can be measured as follows:

$$\epsilon_t = \Delta \log Y_t - \theta \Delta \log N_t - (1-\theta) \Delta \log K_t - \mu \qquad [3]$$

and ϵ will hereafter be referred to as the productivity impulse.[3] To measure ϵ, Prescott (1986) uses GNP data, an efficiency labor hours series as computed by Hansen (1984), and a capital stock measure which includes the stock of residential housing but excludes the stock of durable consumption goods. For calibration purposes, Prescott states that a value of θ=.75 is appropriate. This particular choice requires elaboration. In the theoretical model, Prescott uses the value θ=.64 since this is the average of labor's share in output during the postwar period when output is defined to include the services of durable consumption goods. His empirical analysis, however, uses GNP as the measure of output, and GNP does not include the services of durable consumption goods. Since GNP understates the theoretical measure of output, but labor's compensation is unaffected, labor's share rises to .75 for the postwar period. This reasoning underlies the value of θ=.75 and Prescott's measure of the productivity impulse ϵ.

Given a measure of the aggregate productivity impulse ϵ, a standard exogeneity assumption of RBC models becomes a refutable assumption; furthermore, standard exogeneity testing remains valid even if measures of other real shocks are not available. For example, consider a class of RBC models in which there are two real, driving variables, z_t and τ_t. Suppose that τ_t follows

$$\log \tau_t = \rho \log \tau_{t-1} + \nu_t \qquad |\rho|<1$$

where ν_t is a mean zero, random variable. The innovations w_t and ν_t are assumed to constitute a vector white noise process, and w_t and ν_t may be contemporaneously correlated. According to specification [2], past values of

ν should not help predict ϵ_t beyond the own past history of ϵ. Consequently, the productivity impulse ϵ_t is unpredictable based upon the past values of real variables, nominal variables, or the omitted real shock ν: in this context, the exogeneity of ϵ can be refuted without measuring ν. The generalization to more than two driving variables and alternative linear representations for τ_t should be clear. The critical assumption in [2] is the omission of lagged shocks other than ϵ (namely, ν_{t-s}, $s \geq 1$): all of the previously cited RBC papers share this assumption.

One way to investigate the exogeneity issue is to conduct a standard, multivariate time series analysis of ϵ and other potential explanatory variables. The following specification is investigated:[4]

$$\epsilon_t = \beta(L)\ \epsilon_{t-1} + \alpha(L)\ x_{t-1} + w_t \qquad [4]$$

where $\beta(L)$, and $\alpha(L)$ are polynomials in the Lag operator L. According to specification [2], x should not provide predictive power for ϵ. A finding that $\alpha(L) \neq 0$ in [4] is sufficient to refute the assumption that ϵ is strictly exogenous (for example, see Geweke (1984)).[5]

The list of variables included in the vector x is: the M1 measure of money (M1), 90-day Treasury Bill rates (TBILL), the Consumer Price index (CPI), real government expenditures (GOVT), and Crude Oil prices (OIL). These variables were selected since, in an RBC model, productivity shocks may reflect the influence of any omitted variables: all of these variables are typically omitted. The data is quarterly and seasonally adjusted. Four lags of all variables are included in the autoregression [4]. The interest rate variable is measured as the change in Treasury Bill rates; money, government expenditures, the consumer price index, and the crude oil price index are measured as growth rates (that is, log first-differences).[6] The two sample periods studied are 1957:II-1983:II and 1957:II-1978:IV. The 1983:II sample period is dictated largely by the availability of Prescott's series for ϵ

which begins in 1954:IV. The 1978:IV sample period was chosen to gauge the sensitivity of the results to an alternative sample period which did not include the "Volcker experiment" years, 1979-1982.

Table 1 reports that M1, TBILL, CPI, and GOVT individually Granger-cause ϵ over the 1983:II sample period.[7] [8] The R^2 for this regression is .47, so the statistical significance of these results is also quantitatively significant. For both periods, government spending, money and inflation are always significant at levels below the 2% level. Oil prices are not significant at conventional levels. This suggests that identifying productivity shocks with past oil price increases may be misleading.[9] The significance of interest rates in the 1983:II period does not hold for the shorter 1978:IV period. McCallum (1983) has argued in a similar context that <u>both</u> M1 and TBILL may reflect monetary policy in an equation such as this. Therefore, a specification which includes both TBILL and M1 may not be appreciably better than one with simply TBILL (or simply M1). To investigate this possibility, notice that M1 and TBILL are jointly significant at less than the 1% level in both periods. Further, when only M1 (and not TBILL) or only TBILL (and not M1) are included in the x-vector, these variables are significant (at the 2.5% level). Thus, money and nominal interest rates jointly provide significant explanatory power for ϵ. The results in Table 1 provide evidence against the hypothesis that this measure of the productivity impulse ϵ is exogenous; consequently, the productivity shock z is not exogenous.

The quantitative significance of these nonexogeneity results can be investigated by a decomposition of variance analysis. For a VAR containing ϵ, M1, TBILL, OIL and GOVT, Table 2 reports the percentage of the 16-quarter ahead forecast error variance of ϵ attributable to these variables. Since the own ϵ-innovations account for 70.8% and 68.5% of the variance in ϵ in the

1983:II and 1978:IV samples, the M1, TBILL, OIL and GOVT innovations jointly account for 29.2% and 31.5% of the variance in ϵ. The lower bound of the 95% confidence interval is 16.6%,[10] so the nonexogeneity of ϵ is quantitatively significant. Taken singly, the lower bounds of the intervals for M1, TBILL, OIL, and GOVT are near zero; some uncertainty remains about exactly which innovations are quantitatively significant. However, following McCallum (1983) in interpreting monetary policy as M1 and TBILL innovations jointly, monetary policy is quantitatively significant for the full sample period.[11 12]

To conclude this section, evidence has been presented to show that Prescott's measure of productivity shocks is not exogenous. Changes in aggregate demand, reflected in M1, TBILL, and GOVT, influence ϵ in a statistically as well as economically significant way. These results alone, however, are insufficient to refute the exogeneity hypothesis. In principle, these results could represent erroneous rejections if certain econometric and theoretical objections are quantitatively important. Sections 3, 4, and 5 tackle the issues of measurement error bias, specification error bias, and a special form of omitted shock bias. In fact, the essential conclusions of this section are unchanged by these considerations.

3. Measurement Error Analysis

The failure of ϵ to pass simple exogeneity tests in Section 2 could be due to measurement errors in the data. If ϵ is measured with error, then the Ordinary Least Squares estimator of $\beta(L)$ in [4] is not consistent, the estimated standard errors are not consistent, and the previous test results are uninterpretable. To assess the influence of measurement error on the exogeneity tests, consider the following statistical model of the true productivity impulse (now referred to as ϵ^*), the other variables (x), and two error-ridden measures of the productivity impulse (ϵ_1 and ϵ_2):

$$\epsilon_t^* = A_{11}(L)\ \epsilon_{t-1}^* + A_{12}(L)\ x_{t-1} + w_t, \qquad H_0: A_{12}(L)=0 \tag{5}$$

$$x_t = A_{21}(L)\ \epsilon_{t-1}^* + A_{22}(L)\ x_{t-1} + \omega_t \tag{6}$$

$$\epsilon_{1t} = \epsilon_t^* + B_1(L)\ v_{1t} \tag{7}$$

$$\epsilon_{2t} = \epsilon_t^* + B_2(L)\ v_{2t} \tag{8}$$

where $A_{ij}(L)$ and $B_i(L)$ are polynomials in the lag operator L, and w_t and ω_t are the innovations to ϵ_t^* and x_t. Economic agents observe the true productivity impulse ϵ^*, but the econometrician can only observe ϵ_1 and ϵ_2. The random variables v_1 and v_2 are mean zero, serially independent measurement errors generated by the data reporting agencies. Since this is a model of random measurement errors, each of the errors v_1 and v_2 is assumed to be independent of ϵ^*. When the two productivity measures ϵ_1 and ϵ_2 are constructed with data reported by independent agencies, the errors v_1 and v_2 are assumed to be mutually independent as well. Models of classical measurement error similar to this one have been investigated recently by Sargent (1989), Prescott (1986), and Christiano-Eichenbaum (1991).

To complete the measurement error model, the relationship between x, ϵ_1, and ϵ_2 must be clarified. I assume that the test variables x are measured without error: x, v_1, and v_2 are jointly independent at all leads and lags. Allowing for measurement errors in x, as well as ϵ_1 and ϵ_2, would treat all data series symmetrically, an analysis with much merit. Unfortunately, insufficient data on x is available to implement the instrumental variables estimator described below. To make some progress on the issue of measurement errors, therefore, I follow Prescott (1986) and Christiano-Eichenbaum (1991) in treating the data series asymmetrically.

Testing the exogeneity hypothesis in this context requires consistent estimation of $A_{12}(L)$ and its covariance matrix estimator; the latter requires consistent estimation of $A_{11}(L)$ as well. If either ϵ_1 or ϵ_2 is used in place of the unobserved ϵ^*, and OLS is applied to equation [5], the $A_{11}(L)$ estimator will not be consistent. Using ϵ_2 as an instrument for ϵ_1 in equation [5], however, results in consistent estimation and a valid exogeneity test can be conducted. This estimation procedure is semiparametric in the sense that estimates of $B_i(L)$ are not necessary; consequently, misspecification of the order of $B_i(L)$ is not an issue.

A decomposition of variance analysis of the VAR system [5] and [6] is possible if a consistent estimator of Ω, the covariance matrix for the innovation vector, is available. In fact, for each innovation w_t and ω_t, two error-ridden observations are available given estimates of $A_{ij}(L)$ and the two error-ridden series ϵ_{1t} and ϵ_{2t}. Since the measurement errors in ϵ_{1t} and ϵ_{2t} are orthogonal, the error-ridden residual series will also be orthogonal. Construction of a consistent covariance estimator is straightforward given these residual series.[13]

Implementing this econometric procedure requires two measures of ϵ^* whose measurement errors are arguably independent. Prescott assumes that the measurement errors in the growth rates of GNP and the capital stock measure are negligible. He focuses on measurement errors in the labor input, where two independent series are available for total labor hours: Gary Hansen's efficiency hours (constructed from the Household Survey data), and total nonagricultural hours from the Survey of Business Establishments. The data for these series are collected by two separate government agencies, so the measurement errors are arguably independent. I also consider measurement errors in output by employing the Federal Reserve's series for Industrial Production as a proxy for GNP. If the one-sector theoretical economy exhibits balanced growth, then the data's actual sectoral outputs should aggregate to the one-sector aggregate output series. Thus, the growth rates of GNP and IP should be measuring the same theoretical growth rate in output: to the extent that these growth rates differ, this is interpreted as being due to (serially correlated) measurement errors. Finally, the tables below do not report results which allow for measurement errors in the capital stock variable: I am unable to find an independent measure of the capital stock which is highly correlated with the primary measure used in this study.[14]

Table 3 presents the Instrumental Variable (IV) exogeneity test results. The results are presented for two cases: (1) assuming that only the growth rate of hours is measured with error (Hours only);[15] and (2) assuming that only the growth rates of hours and output are measured with error (Hours/Output). For the Hours only case, ϵ^* continues to fail the exogeneity test, but the patterns of failure differ. CPI and GOVT Granger-cause ϵ^* in both periods; TBILL does in only the 1983:II period; and M1 does in only the 1978:IV period. However, M1 and TBILL jointly Granger-cause ϵ^* in both periods; and when only TBILL (and not M1) or only M1 (and not TBILL) are included in the system, these variables are significant in both periods. Interpreting both M1 and TBILL as instruments of monetary policy sustains the conclusion that monetary policy has influenced the evolution of the productivity impulse ϵ^*.

For the case of Hours/Output, the evidence of predictability in ϵ^* is weaker. M1, TBILL and CPI are jointly significant in the 1983:II period, but not in the 1978:IV period. This lack of stability across sample periods could be due to a change in monetary policy over the period 1979-82. GOVT Granger-causes ϵ^* in both periods. For this case, there is some evidence against the exogeneity of ϵ^*, but the Granger-causality evidence is substantially weaker than in Table 1.

Given IV estimates of $\hat{A}_{ij}(L)$ and $\hat{\Omega}$, Table 4 reports decomposition of variance results for ϵ^*, the true productivity impulse, in a VAR which includes M1, TBILL, OIL, and GOVT. For each case in both periods, the percentage of variance in ϵ^* which is attributable to own innovations is smaller than in Table 2. Apparently, in Table 2 the measurement error in ϵ is being attributed more to the productivity impulse innovations than the other innovations. The confidence intervals tend to be wider when measurement error is accommodated. Nevertheless, aggregate demand variables and oil prices contribute between 34-60% of the variance of ϵ^*; the lower bounds on the 95% confidence interval are between 10-43%. The nonexogeneity evidence here is stronger than in Table 2.

Based upon the evidence presented in Tables 3 and 4, the failure of measured productivity impulses to pass simple exogeneity tests is not likely to be due to the presence of classical measurement errors in the productivity data.

4. Specification Error Analysis

Another potential criticism of the exogeneity tests is the particular measure of the aggregate productivity impulse ϵ. In principle, the results in Section 2 might be specific to: (1) the choice of labor input data; (2) the value of the constant labor share parameter θ; (3) the functional form for the aggregate technology; or (4) the assumption of a constant rate of capacity utilization. This section briefly discusses the results of a sensitivity analysis. The principal finding is that the results of Section 2 are robust: the strict exogeneity of ϵ is refuted for the 12 measures considered.

First, Prescott's measure of ϵ uses Hansen's (1984) efficiency hours series as the measure of labor hours. In principle, the predictability of ϵ could be an artifact of this constructed series. Two alternative aggregate labor hours series, however, are available: the Household Survey measure and the Survey of Business Establishments. Accordingly, alternative measures of ϵ have been computed using the Household and Establishment Survey hours data to address this possibility.

Second, under the assumption of an aggregate Cobb-Douglas production function, measuring ϵ requires an estimate of labor's share in output (θ). The previous measure assumes that θ=.75, just as Prescott did. For each of the three labor measures, however, θ can be estimated directly from the aggregate Cobb-Douglas production function. Since theory predicts that labor hours will respond to productivity shocks, consistent estimation requires the use of an instrumental variables estimator. If the true impulse is serially uncorrelated, however, a valid set of instruments includes lagged values of labor hours, capital, and output. Given consistent estimates of θ, appropriate measures of ϵ can be constructed.

A third problem may be the assumption of an aggregate Cobb-Douglas production function. This criticism can be addressed by computing a standard Solow measure of total factor productivity, which uses time-varying factor weights. This measure is consistent with any constant-returns-to-scale (CRS) aggregate technology if markets are competitive. Since Real Business Cycle theories typically assume a competitive environment, the Solow residual is an appropriate measure of the productivity impulse for any CRS technology. As Hall (1988) has noted, however, in noncompetitive environments this measure of productivity impulses will not be exogenous. In this case, an exogeneity test failure would be consistent with Hall's findings.[16]

Finally, using the entire aggregate capital stock as a measure of the capital input to production implicitly assumes that capacity utilization is constant over the business cycle. Relaxing this assumption is difficult since existing measures of capacity utilization are inappropriate for computing a utilized capital series (see Shapiro (1989) for example). I follow Prescott (1986) in allowing for variable capital utilization through the variations in labor input. Specifically, utilized capital services in production is $u_t k_t$,

u_t is the utilization rate, and $u_t = n_t^{\alpha}$. Prescott used a value of α=0.40; selecting a variety of α values left the test results qualitatively unchanged.

The Granger-causality and variance decomposition results are similar to the results of Section 2, and so are not reported here to conserve space. A four-variable VAR containing ϵ, M1, TBILL, and GOVT was estimated. In each of the 12 specifications,[17] either M1, TBILL, or both Granger-causes ϵ at very low significance levels (less than 2.5%); GOVT Granger-causes ϵ in each of the 12 cases also at low significance levels. The predictability of the productivity impulse ϵ is a remarkably robust result.

The variance decomposition results mimic the robustness of the Granger-causality test results. Innovations in M1, TBILL, and GOVT account for between 26-33% of the variance in the 16-quarter ahead forecast error of ϵ. The lower bounds of the 95% confidence intervals are between 12-21%. Thus, the quantitative significance of these variables is also robust across the alternative measures of ϵ.

5. Signalling and the Omitted Real Shock Hypothesis

The predictability of ϵ can be interpreted plausibly in one of two ways: either (1) changes in money, interest rates, and government spending lead to changes in measured productivity ϵ, or (2) changes in these variables reflect changes in other real shocks which lead to changes in ϵ. The latter interpretation, the omitted real shock hypothesis, is that the empirical findings above are spurious, and a more complete specification of the real shocks in the economy would overturn the results. As I discussed in Section 2, specification [2] rules out many omitted shock hypotheses; however, the RBC literature has featured one important alternative which has not been ruled out so far. King and Plosser (1984) consider an RBC model in which endogenous money can respond to real shocks before output can respond. Specifically, some productivity shocks which occur in period t+1 are revealed in period t; endogenous money and other financial variables respond to this information in period t. Similarly, Litterman and Weiss (1985) describe an economy where economic agents have more information about future aggregate supply shocks than does the econometrician; since financial and monetary variables convey information about these unobserved shocks, nominal variables Granger-cause real variables. After controlling for the unobserved shocks, however, Litterman-Weiss find that real variables are block exogenous with respect to nominal variables. Thus, the apparent importance of nominal variables in the Litterman-Weiss economy is spurious. These examples suggest that the importance of nominal variables for predicting productivity shocks may simply reflect the influence of omitted real shocks, even in the context of specification [2].

To see this in a simple context, suppose that the productivity shock z_t follows the stochastic process:

$$\log z_t = \log z_{t-1} + \mu + \epsilon_{1t} + \epsilon_{2,t-1} \qquad [9]$$

where ϵ_{1t} and $\epsilon_{2,t-1}$ are assumed to be mean zero, serially uncorrelated, stationary random variables and $E[\epsilon_{1t}\epsilon_{2,t-1}] \neq 0$ is permitted. The impulse ϵ_{1t} is revealed in period t, whereas ϵ_{2t-1} is revealed in period t-1; both impulses, however, are realized in period t. This specification is in the spirit of King-Plosser (1984): economic agents can anticipate some productivity shocks prior to their realization, while others are completely unanticipated. Define $\epsilon_t = \epsilon_{1t} + \epsilon_{2,t-1}$ and note that ϵ_t is the measured productivity impulse from equation [3].

In a monetary economy with this aggregate technology, inside money, outside money, stock prices, and nominal interest rates can respond in period t to an impulse (ϵ_{2t}) which is signalled in period t but not realized until period t+1. In this sense, a finding that time t nominal variables

Granger-cause ϵ_{t+1} could be spurious; that is, ϵ could fail Granger-causality tests but be strictly exogenous.

In the context of [9], ϵ_{t+1} should not be correlated with money and interest rates which are sufficiently distant in time: in this example, the growth rate of money and nominal interest rates in period t-1 should be uncorrelated with ϵ_{t+1}.[18] More generally, some impulses may be revealed p periods in advance of their realization, but information which becomes available in period t-p should be uncorrelated with ϵ_{t+1}. For a given choice of p, specification [4] can be appropriately altered to control for the possible signalling factors:[19]

$$\epsilon_t = \beta(L)\ \epsilon_{t-1} + \alpha(L)\ x_{t-p-1} + w_t \qquad [4']$$

Thus, the exogeneity hypothesis now implies that $\alpha(L)=0$ in [4'].

No a priori information is available to suggest one, unique value for p. Litterman-Weiss (1985) and King-Plosser (1984) each select a model which would set p equal to one period. Since the sample interval for this study is quarterly, and the King-Plosser model could easily refer to yearly decisions, Table 5 reports signalling test results for p= 1, 2, 3, and 4 quarters.

In Table 5, the vector of explanatory variables includes M1, TBILL, and GOVT. First, government spending is not significant at any reasonable level for any choice of p≥1. Second, TBILL provides explanatory power as early as four quarters ahead (p=3), and M1 provides explanatory power at seven quarters ahead (p=6, unreported). Jointly, M1 and TBILL are always significant (up to p=6, unreported). Third, when ϵ is computed using θ=.75 and either the Establishment or Household Survey hours, the corresponding results for Table 5 are not appreciably different (again, unreported).[20]

If the signalling hypothesis is the correct explanation for the explanatory power of money and interest rates, then productivity impulses must be anticipated 7 quarters ahead: this feature is at variance with every RBC model which has been studied to date. Consequently, the evidence favors an interpretation in which the nominal variables influence ϵ in a fundamental way, not an omitted variable channel such as specification [9].[21]

6. Conclusions

The results above demonstrate that productivity shocks as measured by Solow/Prescott methods do not behave as strictly exogenous stochastic processes. Money, nominal interest rates, and government spending individually and jointly Granger-cause various measures of the impulses to these shocks. These results are not due to Classical measurement errors. The hypothesis that this result is due to omitted real factors has been investigated, and no evidence has been found to support the hypothesis. Furthermore, the influence of money, interest rates, and government spending is economically significant: their innovations account for between one-quarter and one-half of the forecast error variance in ϵ at the 16-quarter forecast horizon. The lower one-quarter value is computed under an RBC orthogonalization of the innovations in the absence of measurement errors; the upper one-half value, after accounting for measurement errors.

As a whole, these results cast a shadow over the current generation of RBC models which assume strictly exogenous productivity shocks and exclude any interesting role for aggregate demand shocks or other supply shocks. At a minimum, these results imply that the RBC literature to date has overstated the importance of productivity shocks for economic fluctuations. Two theories which may be consistent with the evidence presented here are the labor hoarding model of Burnside, Eichenbaum, and Rebelo (1990) and the productive externality model of Baxter and King (1990). According to both models, conventionally measured Solow/Prescott residuals are not exogenous. In these models prices are perfectly flexible, so the empirical finding that money and interest rates Granger-cause productivity shocks would presumably be explained as reverse causation as in King and Plosser (1984). Alternatively, if prices were assumed to be sticky in these types of economies, these Granger-causality findings would be explained as direct causality. To discriminate among these various theories as well as further assess the role of productivity shocks, researchers should investigate economic structures which jointly predict the stylized facts of business cycles and endogenous Solow residuals.

Data Appendix

Many of the data series used in this study are directly available from the CITIBASE data base (their CITIBASE labels are in []): CPI, the consumer price index less shelter [PUXHS]; GOVT, real (federal) government expenditures [GGE82]; OIL, the producer price index for crude oil [PW561]; GNP, real gross national product [GNP82]; IP, Industrial Production [IP]; Labor hours data: Establishment survey [LPMHU], Household Survey [LHOURS]; and the Capital Stock [KRH72, KN72]. The Efficiency hours data is from Hansen (1984). The M1 (money) and TBILL (90-day Treasury Bill rates) data are the same as in Eichenbaum-Singleton (1986).

Table 1: The Predictability of Prescott's Productivity Impulse[a]

$$\epsilon_t = \beta(L)\,\epsilon_{t-1} + \alpha(L)\,x_{t-1} + w_t \qquad [4]$$

		Marginal Significance Levels for Testing H_0: $\alpha(L)=0$	
X-vector[b]		1957:II - 1983:II	1957:II - 1978:IV
a.	M1	.0033	.0172
	TBILL	.0183	.1628
	CPI	.0003	.0193
	GOVT	.0005	.0019
	OIL	.8895	.1455
	M1, TBILL	.0000	.0001
	M1, TBILL, CPI	.0000	.0001
b.	M1 alone*	.0003+	.0002
c.	TBILL alone*	.0048	.0209+

[a]Four lagged values of ϵ and X are used in the autoregression. The marginal significance levels can be interpreted in the following manner: for M1 in the period 1957:II-1978:IV, the marginal level .0172 indicates that the Null Hypothesis of $\alpha(L)=0$ (with respect to the M1 components of X) would be rejected at significance levels of 1.72% and higher.

[b]The vector autoregression includes M1, TBILL, CPI, GOVT, and OIL as components of the X-vector. The line "M1, TBILL" reports marginal significance levels for testing the joint hypotheses that the M1 and TBILL coefficients are a block zero vector. Similarly for "M1, TBILL, CPI."

* Other elements in the X-vector are: GOVT, OIL, and CPI.
+ OIL is significant at the 5% significance level.

Table 2: Decomposition of Variance Results[a]

Percentage of Variance in Prescott's Productivity Impulse ϵ
Explained by Innovations in Vector Autoregression [4]:
Point Estimates and 95% Confidence Intervals

Components of X-vector	1957:II - 1983:II	1957:II - 1978:IV
ϵ	70.8	68.5
	(58.2, 83.4)	(55.0, 82.1)
M1	8.2	6.5
	(2.5, 14.0)	(0.9, 12.1)
TBILL	7.7	9.0
	(0.4, 15.1)	(0.0, 18.4)
OIL	2.4	4.2
	(0.0, 5.6)	(1.1, 7.2)
GOVT	10.8	11.8
	(0.0, 21.9)	(0.0, 25.4)
M1, TBILL[b]	15.9	15.5
	(6.5, 25.3)	(3.9, 27.0)
OIL, GOVT[b]	13.2	16.0
	(1.9, 24.5)	(2.9, 29.1)

[a]The order of orthogonalization is in the order of the variables listed. The forecast horizon is 16 quarters.

[b]The line "M1, TBILL" reports the percentage of variance jointly explained by M1 and TBILL innovations. The point estimate is the simple sum of the individual M1 and TBILL percentages; however, the 95% confidence interval requires more extensive calculations (see footnote #11 in the text). Similarly for "OIL, GOVT."

Table 3: The Predictability of Prescott's Productivity Impulse[a] in the Presence of Classical Measurement Errors

$$\epsilon^*_t = A_{11}(L)\,\epsilon^*_{t-1} + A_{12}(L)\,x_{t-1} + w_t \qquad [5]$$

	Marginal Significance Levels for Testing H_o: $A_{12}(L) = 0$			
	1957:II - 1983:II		1957:II - 1978:IV	
X-vector[b]	Hours Only[c]	Hours/Output[c]	Hours Only	Hours/Output
a. M1	.0699	.7455	.0092	.8240
TBILL	.0004	.2286	.2533	.7554
CPI	.0000	.0338	.0005	.3136
GOVT	.0369	.0145	.0327	.0137
OIL	.7428	.7518	.0780	.0405
M1, TBILL	.0000	.1210	.0000	.5305
M1, TBILL, CPI	.0000	.0004	.0000	.3160
b. M1 alone*	.0191	.5404	.0056	.4493
c. TBILL alone*	.0015	.0840	.0458	.4137

[a]Four lagged values of ϵ and X are used in [5], and 8 lags are used in computing the Newey-West heteroskedasticity-autocorrelation consistent covariance matrix estimator.

[b]See footnote b in Table 1.

[c]"Hours Only": IV estimation assumes that only the Hours series contains measurement error; "Hours/Output": IV estimation assumes that the Hours and Output series contain measurement error.

*Other elements in the X-vector are: GOVT, OIL, CPI.

Table 4: Decomposition of Variance Results in the Presence of[a] Classical Measurement Errors

Percentage of Variance in Prescott's Productivity Impulse
Explained by Innovations in the Vector Autoregression [5]:
Point Estimates and 95% Confidence Intervals

Components of[b] X-vector	1957:II - 1983:II		1957:II - 1978:IV	
	Hours Only[c]	Hours/Output[c]	Hours Only	Hours/Output
ϵ^*	47.5	66.0	39.8	50.8
	(30.1, 65.0)	(42.8, 89.3)	(23.5, 56.1)	(22.4, 79.2)
M1	16.1	9.5	13.8	8.0
	(8.0, 24.2)	(0.0, 20.8)	(3.5, 24.0)	(0.0, 23.0)
TBILL	13.7	5.9	15.1	7.1
	(1.2, 26.2)	(0.0, 12.0)	(0.0, 31.2)	(0.0, 21.7)
OIL	4.8	3.2	8.4	4.8
	(0.0, 10.7)	(0.0, 9.7)	(0.0, 17.2)	(0.0, 11.3)
GOVT	17.9	15.5	22.9	29.2
	(0.0, 41.3)	(0.0, 36.7)	(0.0, 49.5)	(0.0, 58.5)
M1, TBILL[d]	29.8	15.4	28.9	15.1
	(14.9, 44.7)	(3.4, 27.4)	(9.5, 48.3)	(0.0, 38.7)
OIL, GOVT[d]	22.7	18.7	31.3	34.0
	(0.0, 46.7)	(0.0, 39.6)	(7.2, 55.4)	(6.7, 61.3)

[a]The order of orthogonalization is in the order of the variables listed. The forecast horizon is 16 quarters.

[b,c]See the corresponding footnotes in Table 3.

[d]See footnote b in Table 2.

Table 5: Testing the Signalling Hypothesis[a]

$$\epsilon_t = \beta(L)\,\epsilon_{t-1} + \alpha(L)\,x_{t-p-1} + w_t \qquad [4']$$

X-vector[b]	Marginal Significance Levels for Testing H_0: $\alpha(L)=0$			
	p = 1	p = 2	p = 3	p = 4
M1	.1536	.0311	.0294	.0013
TBILL	.0278	.0008	.0008	.1497
GOVT	.8907	.6924	.7703	.9462
M1, TBILL[c]	.0000	.0000	.0000	.0000

[a]The productivity impulse ϵ is Prescott's measure, the sample period is 1957:II - 1983:II, and four lags are used in the estimation.

[b]The elements of the X-vector are M1, TBILL, and GOVT.

[c]The Null hypothesis is that the block of coefficients associated with M1 and TBILL are jointly zero.

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Footnotes

[1]The empirical approach here differs from Hall (1988) and Caballero-Lyons (1990) in three ways: (1) the instruments and identifying restrictions are different; (2) this paper uses quarterly rather than annual data; and (3) Hall-Caballero-Lyons focus exclusively on contemporaneous correlations, whereas this paper does not.

[2]In a trend-stationary economy, the logarithm (or level) of z_t is often assumed to be an exogenous, AR(1) process as in Hansen (1985), Hansen-Sargent (1988), King-Plosser-Rebelo (1988), and McCallum (1989).

[3]Referring to ϵ as the productivity "impulse" is an abuse of standard terminology if ϵ is serially correlated. Nevertheless, since z_t is the productivity <u>shock</u>, I will refer repeatedly to ϵ as the "impulse," irrespective of its serial correlation properties.

[4]Specifications of [4] which set $\beta(L)=0$ a priori have also been investigated, and the conclusions drawn are similar.

[5]Weaker forms of exogeneity do not seem appropriate here. Weak exogeneity and predeterminedness are econometric conditions which determine efficient estimation techniques (Engle, Hendry, and Richard (1983)); these conditions, however, admit specifications for ϵ_t which violate the spirit of RBC models.

[6] Alternative stationary-inducing transformations of the data have been investigated. In particular, the basic conclusions of this paper are unchanged for trend-stationary and Hodrick-Prescott transformations of the data (including the productivity variable z_t).

[7] All of the test results reported in this paper have been generated using conditional heteroskedasticity-consistent covariance estimators as suggested by White (1980) and Hansen (1982).

[8] In simple autoregressions with only a univariate x-variable, the exogeneity hypothesis fails often. For example, the following variables Granger-cause ϵ in these autoregressions: the monetary base (in the 1983:II period only), M1, TBILL, the Federal Funds rate, CPI, GOVT, and OIL. The Trade deficit did not Granger-cause ϵ.

[9] This evidence in no way rules out the possibility that oil price changes influence ϵ contemporaneously.

[10] Confidence intervals were computed by the normal approximation method described in Runkle (1987); the covariance matrix estimator is conditional heteroskedasticity-consistent.

[11] Confidence intervals around the statistic $Q = g_1(\beta) + g_2(\beta)$ are computed in the obvious way, using the fact that $\mathrm{Var}(Q) = \mathrm{Var}[g_1(\beta)] + \mathrm{Var}[g_2(\beta)] + 2\mathrm{Cov}[g_1(\beta), g_2(\beta)]$.

[12] This conclusion regarding M1 and TBILL continues to hold if the order of orthogonalization is ϵ, OIL, GOVT, M1, and TBILL.

[13]For example, let w_{1t} and w_{2t} be the two constructed residuals of [5] using ϵ_{1t} and ϵ_{2t}, respectively. Then an estimator for the variance of w_t is the sample covariance between w_{1t} and w_{2t}.

[14]As an instrument for the capital stock, Costello (1989) uses electricity <u>consumption</u>, but that data is available only annually. As a quarterly instrument, I have tried the <u>production</u> of electricity by utility companies. The correlation between this instrument and the primary capital variable is .37 (in growth rates). When this instrument is employed, the exogeneity hypothesis fails more often than for the case which uses Hours and Output only.

[15]Prescott and Christiano-Eichenbaum assume that only the logarithm of labor hours is measured with error: their assumptions imply that $B_i(L)=B_0^i-B_0^iL$ and $A_{11}(L)=0$. Instead, I assume that the growth rate of labor hours is measured with error, and allow the measurement error process to be arbitrarily serially correlated. Also, $A_{11}(L) \neq 0$ is permitted.

[16]Under the assumption that the technology is accurately specified, issues of market power play no explicit role in the nonexogeneity of measured productivity shocks. For example, in a noncompetitive economy where aggregate production takes place according to [1] and [2], if ϵ_t is correctly measured according to [3], it will survive exogeneity tests even in the presence of market power.

[17]Twelve measures arise due to the three labor hours series and the four cases: (1) θ=.75; (2) θ estimated by IV; (3) time-varying Solow weights θ_t; and (4) variable capacity utilization with θ=.75.

[18]This restriction applies regardless of the propagation mechanisms in the economy. For example, suppose that the propagation mechanisms lead to m_{t-1} and R_{t-1} being correlated with y_{t+1}, k_{t+1}, and n_{t+1}. If the technology is accurately specified and the factors are accurately measured, then $\Delta \log z_{t+1} = \mu + \epsilon_{t+1}$. By specification [9], ϵ_{t+1} is uncorrelated with m_{t-1} and R_{t-1}.

[19]As in [4], serial correlation in ϵ_t can be accommodated. Suppose that the aggregate productivity shock process is given by:

$$\log z_t = \log z_{t-1} + \mu + \epsilon_t$$
$$\epsilon_t = \beta(L)\, \epsilon_{t-1} + u_t$$
$$u_t = u_{1t} + u_{2,t-1} + \ldots + u_{p+1,t-p}$$

where the $\{u_{it}\}$ are mean-zero and serially uncorrelated, but the $\{u_{it}\}$ may be contemporaneously correlated in the period in which they are realized (that is, $E[u_{1t}u_{2,t-1}]\neq 0$, $E[u_{1t}u_{3,t-2}]\neq 0$, etc.). The exogeneity tests based upon [4'] are valid for this more general specification of the omitted real shock hypothesis. Also, setting $\beta(L)=0$ a priori leads to essentially the same test results as reported in Table 5.

[20]Allowing for measurement errors as in Section 3 does not alter these conclusions; in fact, the Granger-causality evidence against exogeneity is stronger than in Section 3 for both sample periods. Accounting for alternative detrending procedures also does not change the qualitative results.

[21]Since productivity shocks contain predictable components, these results are consistent with the existence of numerous sources of economic fluctuations. If nominal variables influence z_t, and z_t drives the economy, then nominal variables should influence output. Boschen-Mills (1988), however, find no significant influence of nominal variables on output. Quantifying this output effect, however, is a challenge for future research. Presumably, this will require a structural model which tightly restricts the specifications and lag lengths assumed here and in Boschen-Mills.