Are Technology Improvements Contractionary?

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Yes. We construct a measure of aggregate technology change, controlling for aggregation effects, varying utilization of capital and labor, nonconstant returns, and imperfect competition. On impact, when technology improves, input use and nonresidential investment fall sharply. Output changes little. With a lag of several years, inputs and investment return to normal and output rises strongly. The standard one-sector real-business-cycle model is not consistent with this evidence. The evidence is consistent, however, with simple sticky-price models, which predict the results we find: when technology improves, inputs and investment generally fall in the short run, and output itself may also fall. (*JEL* E22, E32, O33)

When technology improves, does employment of capital and labor rise in the short run? Although standard frictionless real-business-cycle (RBC) models generally predict that they do, other macroeconomic models predict the opposite. For example, sticky-price and imperfect-information models often predict that technology improvements cause employment to fall in the short run, when prices are fixed, but rise in the long run, when prices change. These models also imply that technology improvements could, by reducing short-run investment demand, cause a short-run decline in output as well as inputs. Hence, correlations among technology, inputs, investment, and output shed light on the merits of different business-cycle models.

Measuring these correlations requires an appropriate measure of aggregate technology. We construct such a series by controlling for nontechnological effects in aggregate total factor productivity (TFP): varying utilization of capital and labor, nonconstant returns and imperfect competition, and aggregation effects.[1] "Purified" technology varies about half as much as TFP. Technology shocks appear permanent and do not appear serially correlated.

We find that technology improvements reduce total hours worked within the year, but increase hours with a lag of up to two years. Output changes little on impact, but increases strongly thereafter. Nonresidential investment falls sharply on impact before rising above its steady-state level. Household spending (espe-

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[1] Unless we specifically state otherwise, we use "technology" and "TFP" to refer to growth in these variables. We define TFP to be the standard Solow residual: output growth minus revenue-share-weighted input growth, which need not measure technology. We note that Robert M. Solow's (1957) original article suggests modifications and extensions (for example, for factor utilization and monopoly power) necessary for his residual to properly measure technology at business cycle frequencies; he also notes the issue of aggregation. Basu and Fernald (2001) discuss additional references on technology and TFP.

cially durable consumption and residential investment) rises on impact and rises further with a lag. Thus, after a year or two, the response to our estimated technology series more or less matches the predictions of the standard, frictionless RBC model. But the short-run effects do not.

Correcting for unobserved input utilization (labor effort and capital's workweek) is central to understanding the relationship between procyclical TFP and countercyclical purified technology. Utilization is a form of primary input. Our estimates imply that when technology improves, unobserved utilization, as well as observed inputs, fall sharply on impact. Both then recover with a lag. In other words, when technology improves, utilization falls—so TFP initially rises less than technology does.

Of course, if technology shocks were the only impulse—and if, as we estimate, these shocks were negatively correlated with the cycle—then even before controlling for utilization, we would still be likely to observe a negative correlation between uncorrected TFP and the business cycle. Demand shocks can explain why, instead, uncorrected TFP is procyclical. When demand increases, output and inputs—including unobserved utilization—increase as well. We find that nontechnology shocks are important enough at cyclical frequencies for changes in utilization to make uncorrected TFP procyclical.

To identify technology, we use tools from Basu and Fernald (1997) and Basu and Kimball (1997), who in turn build on Solow (1957) and Robert E. Hall (1990). Basu and Fernald stress the role of sectoral heterogeneity and aggregation. They argue that for economically plausible reasons—for example, differences across industries in the degrees of market power—the marginal product of an input may differ across uses. The aggregate Solow residual (growth in aggregate TFP) then depends on which sectors change input use the most over the business cycle. Basu and Kimball stress the role of variable capital and labor utilization. Their basic insight is that a cost-minimizing firm operates on all margins simultaneously, both observed and unobserved. Hence, changes in observed inputs can proxy for unobserved utilization changes. For example, if labor is particularly valuable, firms will tend to work existing employees both longer (observed hours per worker rise) and harder (unobserved effort rises).

Together, these two papers imply that one can construct an index of aggregate technology change by "purifying" sectoral Solow residuals and then aggregating across sectors. Thus, our fundamental identification comes from estimating sectoral production functions.

Jordi Galí (1999a) independently proposes a quite different method to investigate similar issues. Following Olivier J. Blanchard and Danny Quah (1989) and Matthew D. Shapiro and Mark W. Watson (1988), Galí identifies technology shocks using long-run restrictions in a structural vector autoregression (SVAR). Galí assumes that only technology shocks affect labor productivity in the long run. He examines aggregate data on output and hours worked for a number of countries and, like us, finds that technology shocks reduce input use on impact.

A growing literature questions or defends Galí's (1999a) specification.[2] Neville Francis and Valerie A. Ramey (2005) extend Galí's identification scheme and subject it to a range of economic and statistical tests; they conclude that "the original technology-driven real business cycle hypothesis does appear to be dead." Lawrence J. Christiano et al. (2003), however, argue for using per capita hours in log-levels rather than in growth rates. With this subtle change in specification, they conclude that technology improvements *raise* hours worked on impact.[3] Thus, although the SVAR evidence mostly suggests that technology improvements reduce hours, the evidence from this approach is not yet conclusive.

Our alternative augmented-growth-accounting approach relies on completely different assumptions for identification, with at least four advantages relative to the SVAR literature. First, our results do not depend on a theoretically derived long-run identifying restriction that might not hold. For example, increasing returns, permanent sectoral shifts, capital

[2] See Galí and Pau Rabanal (2005) for a recent summary.

[3] Fernald (2005) argues that the specification of Christiano et al. (2004) also implies that technology improvements reduce hours worked, once one controls for the early 1970s productivity slowdown and mid-1990s acceleration in productivity. (Christiano et al., 2005, and related work by David Altig et al., 2005, make several important methodological contributions to the literature that we view as more substantive than their specific empirical results.) We discuss several related points in Section IV.

taxes, and some models of endogenous growth would all imply that nontechnology shocks can change long-run labor productivity, thus invalidating the identifying assumption.[4] Our production-function approach allows these deviations. Second, we can identify transitory as well as permanent technology shocks; the SVAR approach, in contrast, at best identifies only permanent, unit-root technology shocks. Third, even if the long-run restriction holds, it produces well-identified shocks and reliable inferences only with potentially restrictive, atheoretical auxiliary assumptions (see, for example, Jon Faust and Eric M. Leeper, 1997).[5] Our production-function approach, by contrast, does not rely on these same identification conditions. Fourth, we can easily look at the effect of technology shocks on a large number of variables; results from an identified VAR might look very different as more variables are added. Nevertheless, we view the identified VAR and augmented-growth-accounting approaches as complements, with distinct identification schemes and strengths.

Two additional approaches also suggest that technology improvements reduce input use. First, estimated structural dynamic general equilibrium (DGE) models (for example, Frank Smets and Raf Wouters, 2003, for the euro area and Andrew Levin et al., 2006, for the United States) often find that technology improvements reduce input use on impact. Second, John Shea (1999) measures technology as innovations to R&D spending and patent activity and finds that with a lag of several years *process* innovations increase TFP and simultaneously lower labor input.[6]

Despite differing data, countries, and methods, the bottom line is that the state-of-the-art versions of four very different approaches yield similar results. We thus view the contractionary effect of technology improvements as a robust stylized fact that models need to explain.

What do these results imply for modeling business cycles? They are clearly inconsistent with standard parameterizations of frictionless RBC models, including Robert G. King and Sergio T. Rebelo's (1999) attempt to "resuscitate" these models. The negative effect of a technology improvement on nonresidential investment is particularly hard to reconcile with flexible-price RBC models (including the models suggested by Francis and Ramey, 2003), given our finding that technology appears to be a random walk. Our findings are consistent, however, with the predictions of DGE models with sticky prices. Consider the quantity-theory case where output is proportional to real balances. In the short run, if the supply of money is fixed and prices cannot adjust, then real balances and hence output are also fixed. Now suppose technology improves. Firms now need fewer inputs to produce this unchanged output, so they lay off workers and desire less capital, which could reduce investment.[7] Over time, however, prices adjust, the underlying RBC dynamics take over, and output rises. Relaxing the quantity-theory assumption allows for richer dynamics for output (which could even decline) and its components, but doesn't change the basic message. Models where price rigidity arises endogenously from imperfect common knowledge, as in Michael Woodford (2003a), make similar predictions (see Takuji Kawamoto, 2004).

Of course, in a sticky-price model, technology improvements will be contractionary only if the monetary authority does not offset their short-run effects through expansionary monetary policy. After all, standard sticky-price models predict that a technology improvement that increases full-employment output creates a short-run disinflation, which gives the monetary authority room to lower interest rates. In Section V, we argue that technology improvements are still likely to be contractionary, reflecting

[4] Gadi Barlevy (2004) models cyclical fluctuations as having a substantial effect on long-run growth. In the SVAR context, Harald Uhlig (2004) discusses capital taxes and time-varying attitudes toward workplace leisure; Pierre-Daniel Sarte (1997) suggests that hysteresis in labor quality might lead demand shocks to have permanent productivity effects.

[5] Thomas F. Cooley and Mark Dwyer (1998) and Christopher J. Erceg et al. (2005) estimate SVARs on data from calibrated DGE models and suggest that these concerns could, in some cases, be important. Erceg et al. do conclude that SVAR results can be informative.

[6] Galí (1999b) draws out and discusses this implication of Shea's findings.

[7] James Tobin (1955) makes this point in a model with exogenously fixed nominal wages.

the fact that central banks observe technology shocks only with a long lag.[8]

Clearly, our results are not a "test" of sticky-price or sticky-information models of business cycles, even though the results are consistent with that interpretation. We favor this interpretation in part because one wants a model that appropriately captures the economy's response to both monetary and real shocks, and models with endogenous or exogenous price rigidity can generate large monetary nonneutralities. Other possible explanations include a flexible-price world with autocorrelated technology shocks, low capital-labor substitutability, or substantial real frictions such as habit persistence in consumption and investment adjustment costs; sectoral shifts, if reallocations are correlated with technology growth; the need to learn about new technologies; and "cleansing effects" of recessions, in which recessions lead firms to reorganize or, within an industry, eliminate low-productivity firms. We discuss a range of alternative explanations in Section V.

The paper has the following structure. Section I reviews our method for identifying sectoral and aggregate technology change. Section II discusses data and econometric method. Section III presents our main empirical results. Section IV discusses robustness. Section V presents alternative interpretations of our results, including our preferred sticky-price interpretation. Section VI concludes.

I. Estimating Aggregate Technology, Controlling for Utilization

We identify aggregate technology by estimating (instrumented) a Hall-style regression equation with a proxy for utilization in each disaggregated industry. We then define aggregate technology change as an appropriately weighted sum of the resulting residuals. Section IA discusses our augmented Solow-Hall approach and aggregation; Section IB discusses how we control for utilization.

A. *Industry and Aggregate Technology*

We assume each industry has a production function for gross output:

$$Y_i = F^i(A_i K_i, E_i H_i N_i, M_i, Z_i). \tag{1}$$

The industry produces gross output, Y_i, using the capital stock K_i, employees N_i, and intermediate inputs of energy and materials M_i. We assume that the capital stock and number of employees are quasi-fixed, so their levels cannot be changed costlessly. But industries may vary the intensity with which they use these quasi-fixed inputs: H_i is hours worked per employee; E_i is the effort of each worker; and A_i is the capital utilization rate (that is, capital's workweek). Total labor input, L_i, is the product $E_i H_i N_i$. The production function F^i is (locally) homogeneous of arbitrary degree γ_i in total inputs; γ_i exceeding one implies increasing returns to scale, reflecting overhead costs, decreasing marginal cost, or both; Z_i indexes technology.

Following Hall (1990), we assume cost minimization and relate output growth to the growth rate of inputs. The standard first-order conditions give us the necessary output elasticities, i.e., the weights on growth of each input.[9] Let dx_i be observed input growth, and du_i be unobserved growth in utilization. (For any variable J, we define dj as its logarithmic growth rate $\ln(J_t/J_{t-1})$.) This yields:

$$dy_i = \gamma_i(dx_i + du_i) + dz_i, \tag{2}$$

where

$$dx_i = s_{Ki} dk_i + s_{Li}(dn_i + dh_i) + s_{Mi} dm_i, \tag{3}$$

$$du_i = s_{Ki} da_i + s_{Li} de_i,$$

and s_{Ji} is the ratio of payments to input J in total cost. Section IB explores ways to measure du_i. With constant returns, perfect competition, and no utilization changes, technology change dz_i equals the standard Solow residual (TFP): $dz_i = dy_i - dx_i$.

[8] This assumption is quite natural in sticky-information models, where it amounts to assuming that the central bank and the public have the same information set. See Kawamoto (2004).

[9] Basu and Fernald (2001) provide detailed derivations and discussion of the equations that follow. Note that with increasing returns, firms must charge a markup of price over marginal cost to cover their total costs. Hence, the resulting estimating equation controls for imperfect competition as well as increasing returns.

We define "purified" technology change as a weighted sum of industry technology change:

$$dz = \sum_i \left(\frac{w_i}{1 - s_{Mi}}\right) dz_i, \tag{4}$$

where w_i equals $(P_iY_i - P_{Mi}M_i)/\Sigma_i\ (P_iY_i - P_{Mi}M_i) \equiv P_i^V V_i/P^V V$, the industry's share of aggregate nominal value added. Conceptually, dividing through by $1 - s_M$ converts gross-output technology shocks to a value-added basis (desirable because of the national accounts identity that aggregate final expenditure equals aggregate value added). These shocks are then weighted by the industry's share of aggregate value added.[10]

We define changes in aggregate utilization as the contribution to final output of changes in industry-level utilization. This, in turn, is a weighted average of industry-level utilization changes:

$$du = \sum_i \left(\frac{w_i}{1 - s_{Mi}}\right) \gamma_i du_i. \tag{5}$$

From equation (2), $\gamma_i du_i$ enters in a manner parallel to dz_i, so that (5) parallels (4).

Implementing this approach requires disaggregated estimates of returns to scale and of variations in utilization. We observe all other variables necessary to calculate aggregate technology and utilization.

B. *Measuring Industry-Level Capital and Labor Utilization*

Utilization growth, du_i, is a weighted sum of growth in capital utilization, A_i, and labor effort, E_i. Since cost-minimizing firms operate on all margins simultaneously, changes in observed inputs can potentially proxy for unobserved utilization changes. We derive such a relationship from the cost-minimization conditions of the representative firm within each industry, following Basu and Kimball (1997). The model below provides microfoundations for a simple proxy: changes in hours-per-worker are proportional to unobserved changes in both labor effort and capital utilization. We assume only that firms minimize cost and are price-takers in factor markets; we do not require any assumptions about firms' pricing and output behavior in the goods market. In addition, we do not assume that we observe the firm's internal shadow prices of capital, labor, and output at high frequencies.

We model firms as facing adjustment costs to investment and hiring, so that capital (number of machines and buildings), K, and employment (number of workers), N, are both quasi-fixed. One needs quasi-fixity for a meaningful model of variable factor utilization. Higher utilization must raise firms' costs, or they would always utilize factors fully. Given these costs, if firms could costlessly change the rate of investment or hiring, they would always keep utilization at its long-run cost-minimizing level and vary inputs by hiring/firing workers and capital. Thus, only if it is costly to adjust capital and labor is it sensible to pay the costs of varying utilization.[11]

We assume that firms can freely vary A, E, and H without adjustment cost. We assume the major cost of increasing capital utilization, A, is that firms pay a shift premium (a higher wage) to compensate employees for working at night or other undesirable times. We take A to be a continuous variable for simplicity, although discrete variations in capital's workday (the number of shifts) are an important mechanism for varying utilization.[12] When firms increase labor utilization, E, they must compensate workers for the increased disutility of effort with a higher wage. High-frequency fluctuations in this wage might be unobserved if an implicit contract governs wage payments in a long-term relationship.

[10] This weighting scheme follows Evsey Domar (1961). In previous work, we defined aggregate technology with $(1 - \gamma_i s_{Mi})$ in the denominator. This definition is convex in γ_i. Indeed, as $\gamma s_M \to 1$, $[1/(1 - \gamma s_M)] \to \infty$. This means that positive and negative estimation error does not cancel out. Domar-weighted residuals are thus more robust to mismeasurement.

[11] As Trygve Haavelmo (1960) observes, one-sector models can treat variable utilization without *internal* adjustment costs, since the representative firm's input demand affects the economy-wide real wage and interest rate. But internal adjustment costs are required to model why industries vary utilization in response to idiosyncratic changes in technology or demand.

[12] J. Joseph Beaulieu and Joseph Mattey (1998) and Shapiro (1996), for example, apply the variable-shifts model to manufacturing.

An industry's representative firm minimizes the present value of expected costs:

$$(6)\quad \min_{A,E,H,M,I,D} E_t \sum_{\tau=t}^{\infty}\left[\prod_{j=t}^{\tau-1}(1+r_j)^{-1}\right]$$
$$\times [W_\tau G(H_\tau, E_\tau)V(A_\tau)N_\tau + P_{M,\tau}M_\tau$$
$$+ W_\tau N_\tau \Psi(D_\tau/N_\tau) + P_{I,\tau}K_\tau J_\tau(I_\tau/K_\tau)]$$

subject to

$$(7)\quad \bar{Y}_\tau = F(A_\tau K_\tau, E_\tau H_\tau N_\tau, M_\tau, Z_\tau);$$

$$(8)\quad K_{\tau+1} = I_\tau + (1-\delta)K_\tau;$$

$$(9)\quad N_{\tau+1} = N_\tau + D_\tau.$$

In (6), we use the convention that the product of discount factors is equal to 1 when $\tau = t$. In each period, the firm's costs in (6) are total payments for labor and material, and the costs associated with undertaking gross investment I and hiring (net of separations) D. $WG(H, E)V(A)$ is total compensation owed per worker (which, if it takes the form of an implicit contract, may not be observed period by period). W is the base wage; the function G specifies how the hourly wage depends on effort, E, and the length of the workday, H; and $V(A)$ is the shift premium. P_M is the price of materials. $WN\Psi(D/N)$ is the total cost of changing the number of employees; $P_I KJ(I/K)$ is the total cost of investment; and δ is the rate of depreciation. We assume that Ψ and J are convex.[13] We omit time and industry subscripts except where needed for clarity.

There are six intra-temporal first-order conditions and Euler equations for each of the two state variables K and N. The multiplier on constraint (7), λ, represents marginal cost. F_j, $j = 1, 2, 3$ denotes derivatives of F with respect to argument j, and literal subscripts denote derivatives of the labor cost function G. To conserve space, we analyze only the optimization conditions that affect our derivations, which are those for A, H, and E:

$$(10)\quad A:\quad \lambda F_1 K = WNG(H, E)V'(A);$$

$$(11)\quad H:\quad \lambda F_2 EN = WNG_H(H, E)V(A);$$

$$(12)\quad E:\quad \lambda F_2 HN = WNG_E(H, E)V(A).$$

Note that uncertainty does not affect our derivations, which rely only on *intra*-temporal optimization equations. Uncertainty affects the evolution of the state variables (as the Euler equations would show) but not the minimization of variable cost at a point in time, *conditional* on the levels of the state variables.

Equations (11) and (12) can be combined into an equation implicitly relating E and H:

$$(13)\quad \frac{HG_H(H, E)}{G(H, E)} = \frac{EG_E(H, E)}{G(H, E)}.$$

The elasticities of labor costs with respect to H and E must be equal because, in terms of benefits, elasticities of effective labor input with respect to H and E are equal. Given the assumptions on G, (13) implies a unique, upward-sloping E-H expansion path: $E = E(H)$, $E'(H) > 0$. That is, we can express unobserved intensity of labor utilization E as a function of observed hours per worker H. We define $\zeta \equiv H^*E'(H^*)/E(H^*)$ as the elasticity of effort with respect to hours, evaluated at the steady state. Log-linearizing, we find:

$$(14)\quad de = \zeta dh.$$

To find a proxy for capital utilization, we combine (10) and (11). Rearranging, we find:

$$(15)\quad \frac{F_1 AK/F}{F_2 EHN/F} = \left[\frac{G(H, E)}{HG_H(H, E)}\right]\left[\frac{AV'(A)}{V(A)}\right].$$

The left-hand side is a ratio of output elasticities. As in Hall (1990), cost minimization implies that they are proportional to factor cost shares, which we denote by α_K and α_L. Define

[13] We make necessary technical assumptions on G in the spirit of convexity and normality. The conditions on G are easiest to state in terms of the function Φ defined by $\ln G(H, E) = \Phi(\ln H, \ln E)$. Convex Φ guarantees a global optimum; assuming $\Phi_{11} > \Phi_{12}$ and $\Phi_{22} > \Phi_{12}$ ensures that there is only one possible optimal value of E for each value of H. We make some normalizations relative to normal or "detrended steady state" levels. Let $J(\delta) = \delta$, $J'(\delta) = 1$, and $\Psi(0) = 0$. We also assume that the marginal employment adjustment cost is zero at a constant level of employment: $\Psi'(0) = 0$.

$g(H)$ as the elasticity of labor cost with respect to hours: $g(H) = HG_H(H, E(H))/G(H, E(H))$. Define $v(A)$ as the elasticity of labor cost with respect to capital's workweek (equally, the ratio of the marginal to the average shift premium): $v(A) = AV'(A)/V(A)$. We can then write equation (15) as:

$$(16) \qquad v(A) = (\alpha_K/\alpha_L)g(H).$$

The function $g(H)$ is positive and increasing by the assumptions we have made on $G(H, E)$; let η denote the (steady-state) elasticity of g with respect to H. The function $v(A)$ is positive if the shift premium is positive. We assume that the shift premium increases rapidly enough with A to make the elasticity increasing in A. Let ω be this elasticity of v. We also assume that α_K/α_L is constant, which requires that F be a generalized Cobb-Douglas function in K and L.[14] The log-linearization of (16) is simply

$$(17) \qquad da = (\eta/\omega)dh.$$

Equations (17) and (14) say that the change in hours per worker proxies for changes in *both* unobservable labor effort and the unmeasured workweek of capital. Hours per worker proxies for capital utilization as well as labor effort because shift premia create a link between capital hours and labor compensation. The shift premium is most worth paying when the marginal hourly cost of labor is high relative to its average cost, which occurs when hours per worker are also high.

Putting everything together, we have a simple estimating equation that controls for variable utilization:

$$(18) \quad dy = \gamma dx + \gamma\left(\zeta s_L + \frac{\eta}{\omega} s_K\right)dh + dz$$

$$= \gamma dx + \beta dh + dz.$$

We will not need to identify all of the parameters in the coefficient multiplying dh, so we denote that composite coefficient by β. This specification controls for *both* labor and capital utilization.[15] We measure industry technology change as the residuals dz.

After estimating equation (18) on disaggregated data, which controls for nonconstant returns, imperfect competition, and utilization, we aggregate as in (4) to get a measure of "purified" aggregate technology.

II. Data and Method

A. *Data*

We seek to measure true aggregate technology change, dz, by estimating disaggregated technology change and then aggregating up to the private nonfarm, nonmining U.S. economy. We use industry data from Dale Jorgenson and collaborators from 1949 to 1996. The data comprise 29 industries (including 21 manufacturing industries at roughly the two-digit level) that cover the entire nonfarm, nonmining private economy. These sectoral accounts include industry gross output and inputs of capital, labor, energy, and materials. The Appendix describes the data and our calculations in more detail.

Given the potential correlation between input growth and technology shocks in equation (18), we use instruments uncorrelated with technology change. We use updated versions of two of the Hall-Ramey instruments: oil prices and growth in real government defense spending. For oil, we use increases in the U.S. refiner acquisition price. Our third instrument updates Burnside's (1996) quarterly Federal Reserve "monetary shocks" from an identified VAR. For all three instruments, we use the sum of the year $t - 1$ quarterly shocks as the instrument for year t.[16]

[14] Thus, we assume $Y = Z\Gamma((AK)^{\alpha_K}(EHN)^{\alpha_L}, M)$, where Γ is a monotonically increasing function.

[15] As in Basu and Kimball (1997), allowing capital utilization to affect depreciation would add two more terms. We cannot reject that these terms are zero; in any case, including them has little effect on results reported below. An alternative approach assumes, more restrictively, fixed proportions between an observed and unobserved input. For example, Craig Burnside et al. (1995, 1996) follow Dale Jorgenson and Zvi Griliches (1967) and Alfred W. Flux (1913) and suggest that electricity use might proxy for true capital services. This might be reasonable for some manufacturing industries, but it ignores labor effort and is probably more appropriate for heavy equipment than structures.

[16] The qualitative features of the results in Section III appear robust to different combinations and lags of the instruments. An on-line Appendix (available from the *AER* Web site at http://www.e-aer.org/data/dec06/20040577_app.pdf) discusses several econometric issues, including the small-sample properties of instrumental variables.

TABLE 1—PARAMETER ESTIMATES

Durable manufacturing		Nondurable manufacturing		Nonmanufacturing	
A. Returns-to-scale (γ_i) estimates					
Lumber (24)	0.51 (0.08)	Food (20)	0.84 (0.20)	Construction (15–17)	1.00 (0.07)
Furniture (25)	0.92 (0.05)	Tobacco (21)	0.90 (0.27)	Transportation (40–47)	1.19 (0.10)
Stone, clay, & glass (32)	1.08 (0.04)	Textiles (22)	0.64 (0.11)	Communication (48)	1.32 (0.21)
Primary metal (33)	0.96 (0.05)	Apparel (23)	0.70 (0.08)	Electric utilities (491)	1.82 (0.21)
Fabricated metal (34)	1.16 (0.06)	Paper (26)	1.02 (0.10)	Gas utilities (492)	0.94 (0.06)
Nonelectrical machinery (35)	1.16 (0.09)	Printing & publishing (27)	0.87 (0.19)	Trade (50–59)	1.01 (0.21)
Electrical machinery (36)	1.11 (0.09)	Chemicals (28)	1.83 (0.16)	FIRE (60–66)	0.65 (0.22)
Motor vehicles (371)	1.07 (0.05)	Petroleum products (29)	0.91 (0.19)	Services (70–89)	1.32 (0.25)
Other transport (372–79)	1.01 (0.03)	Rubber & plastics (30)	0.91 (0.09)		
Instruments (38)	0.95 (0.11)	Leather (31)	0.11 (0.19)		
Miscellaneous manufacturing (39)	1.17 (0.17)				
Column average	1.01		0.87		1.16
Median	**1.07**		**0.89**		**1.10**
B. Coefficient on hours per worker					
Durables Manufacturing	1.34 (0.22)	Nondurables Manufacturing	2.13 (0.38)	Nonmanufacturing	0.64 (0.34)

Notes: Heteroskedasticity- and autocorrelation-robust standard errors in parentheses. Coefficients from regression of output growth on input growth and hours-per-worker growth. (Constant terms and, for nonmanufacturing, post-1972 dummy, not shown.) Hours-per-worker coefficient is constrained to be equal within groups (durables, nondurables, and nonmanufacturing). Instruments are oil price increases; growth in real defense spending; and VAR monetary innovations. FIRE is finance, insurance, and real estate.

B. *Estimating Technology Change*

We estimate industry-level technology change from the 29 regression residuals from (18), estimated as a system of equations. To conserve parameters, we restrict the utilization coefficient within three groups: durables manufacturing (11 industries, listed in Table 1); nondurables manufacturing (10); and nonmanufacturing (8). Wald and quasi-likelihood ratio tests do not reject these constraints. (Without the constraints, the variance of estimated technology rises but qualitative and, indeed, quantitative results change little.) Thus, for the industries within each group, we estimate

$$(19) \qquad dy_i = c_i + \gamma_i dx_i + \beta dh_i + dz_i .$$

This parsimonious equation controls for both capital and labor utilization. Note that hours-per-worker growth dh essentially enters twice, since it is also in observed input growth dx. We allow returns-to-scale γ_i to differ by industries within a group. (Hypothesis tests overwhelmingly reject constraints on the γ_i). Aggregate "purified" technology change is the weighted sum of the industry residuals plus constant terms.

Given the mid-sample productivity slowdown, we used Donald Andrews's (1993) approach to test for a break in the industry constants. We imposed the restriction that any break is common to all industries within each group. Only in nonmanufacturing do we reject the null of no break; for TFP growth, the maximum F statistic for a break corresponds to 1973 (just exceeding the value for 1967). We imposed a 1973 break in what follows; our main results appear little affected by adding this break or by changing the break date.

TABLE 2—MEANS AND STANDARD DEVIATIONS OF PRODUCTIVITY AND TECHNOLOGY
(*Annual percent change*)

		Private economy	Durable manuf.	Nondurable manuf.	Nonmanuf.
Solow residual	Mean	0.79	1.75	2.07	0.34
	Std deviation	2.04	3.59	4.18	1.90
"Purified" technology	Mean	0.35	1.54	1.43	−0.12
	Std deviation	1.50	4.58	4.60	1.90

Notes: Sample period is 1949–1996. "Purified" technology is the Domar-weighted sum of industry residuals (including constant terms) from the regression results shown in Table 1, which control for imperfect competition, nonconstant returns, and unobserved utilization. As described in the text, industry Domar weights are $w_i/(1 - s_{Mi})$, where w_i is the value-added weight and s_{Mi} is the share of intermediate inputs in output.

In addition, results are robust to using (unconstrained) industry-by-industry estimation, either by 2SLS or LIML. Parameter estimates are less precise and more variable with individual than group estimation, but median estimates are similar to the median GMM estimates. Estimating individual equations raises the variance of estimated aggregate technology but does not change our main conclusions.

III. Results

A. *Estimates and Summary Statistics*

Our main focus is the aggregate effects of technology shocks, estimated as an appropriately weighted average of industry regression residuals. Table 1 summarizes the underlying industry parameter estimates from equation (19). For durable manufacturing, the median returns-to-scale estimate is 1.07; for nondurable manufacturing, 0.89; for nonmanufacturing, 1.10. For all 29 industries shown, the median estimate is 1.00. (Omitting hours-per-worker growth raises the overall median estimate of returns to scale to 1.12). After correcting for variable utilization, there is thus little overall evidence of increasing returns, although there is wide variation across industries.[17] Throwing out "outlier" industries (lumber, textiles, chemicals, leather, electric utilities, FIRE, and services) has little effect on results below.

The coefficient on hours-per-worker, in the bottom panel, is strongly statistically significant in durables and nondurables manufacturing. The coefficient is significant at the 10-percent level in nonmanufacturing.

Table 2 summarizes means and standard deviations for TFP (the Solow residual) and "purified" technology. TFP does not adjust for utilization or nonconstant returns. Purified technology controls for utilization and nonconstant returns, aggregated as in equation (5).

For the entire private nonmining economy, the standard deviation of technology, 1.5 percent per year, compares with the 2.0-percent standard deviation of TFP; indeed, the variance is only 55 percent as high. For both durable and nondurable manufacturing, the standard deviation of purified technology is, perhaps surprisingly, higher than for TFP. The reduction in variance in column 1 comes primarily from reducing the (substantial) positive covariance across industries, consistent with the notion that business cycle factors—common demand shocks—lead to positively correlated changes in utilization and TFP across industries.

Some simple plots summarize the comovement in our data. Figure 1 plots business-cycle data for the private economy: growth in TFP, output (aggregate value added), and hours (all series are demeaned). These series comove positively, quite strongly so in the case of TFP and output.

Figure 2 plots our purified technology series against these three variables plus estimated aggregate utilization and nonresidential investment. The top panel plots TFP and technology. Technology fluctuates much less than TFP, consistent with varying input utilization and other nontechnological effects raising TFP's volatility. Some periods also show a phase shift: TFP lags technology. The second panel plots aggregate output growth and technology. There is no

[17] Basu and Fernald (2001) discuss the apparent decreasing returns in nondurables manufacturing.

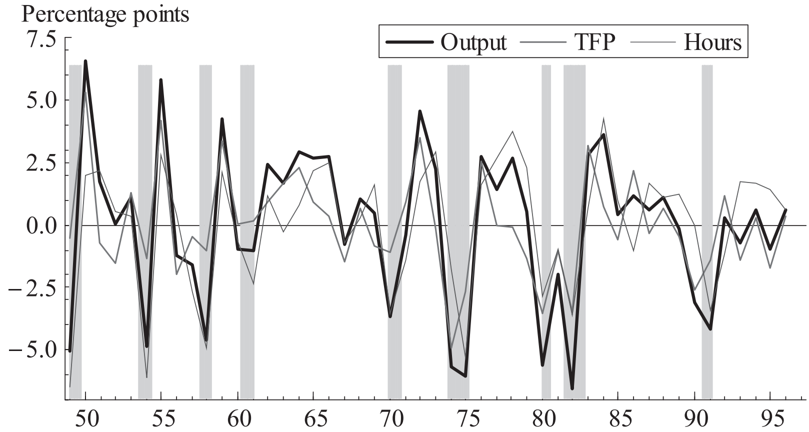

FIGURE 1. TFP, OUTPUT, AND HOURS
(*Annual percent change*)

Notes: All series are de-meaned. Sample period is 1949–1996. All series cover the nonfarm, nonmining private business economy. Growth in aggregate output is measured as real value added. TFP is measured as output growth minus the share-weighted average of growth in primary inputs of capital and labor. Shaded regions show NBER recession dates.

clear contemporaneous comovement between the two series. Particularly in the first half of the sample, the series has the same phase shift as TFP: output comoves with technology, lagged one to two years.

The third panel shows one central result: contemporaneously, hours worked covaries negatively with technology shocks; the correlation is −0.48. These two series clearly comove negatively over the entire sample period, although the negative correlation appears more pronounced in the 1950s and 1960s than later. Following a technology improvement, hours rise with a lag. The fourth panel shows that estimated factor utilization—which, like hours, is a form of input—also covaries negatively with technology. The utilization pattern explains much of the phase shift in the previous charts. That is, when technology improves, utilization falls, which in turn reduces measured TFP relative to technology. Utilization generally rises strongly a year or so after a technology improvement, raising TFP.

The bottom panel shows a second central result: nonresidential investment often falls when technology improves. Conversely, when technology falls (growth below its mean), investment often rises. (The largest investment swings, though, are most likely unrelated to purified technology.)

As expected, the utilization correction explains most of the reduction in procyclicality.[18] If we simply subtract estimated utilization growth from TFP, the resulting series has a correlation of −0.3 with hours growth; various procyclical reallocations then account for the further reduction to a correlation of −0.48.[19]

B. *Dynamic Responses to Technology Improvement*

We summarize dynamics with regressions and with impulse responses from small bivariate (near) VARs. To begin, the level of purified technology (i.e., cumulated growth rates) appears to have a unit root. With an augmented

[18] Corrections to all three groups—manufacturing durables, manufacturing nondurables, and nonmanufacturing—contribute to the negative correlation, although adjustments to manufacturing appear most important. For example, if we simply use TFP in nonmanufacturing rather than estimated technology, the correlation with aggregate hours is −0.33.

[19] As Basu and Fernald (1997) discuss, one reallocation effect comes from the difference in returns to scale between durable and nondurable manufacturing. Durables industries tend to have higher estimated returns to scale (see Table 1) as well as much more cyclical input usage. Hence, during a boom, resources are disproportionately allocated to industries where they have a higher marginal product. This generates a procyclical reallocation effect on measured TFP.

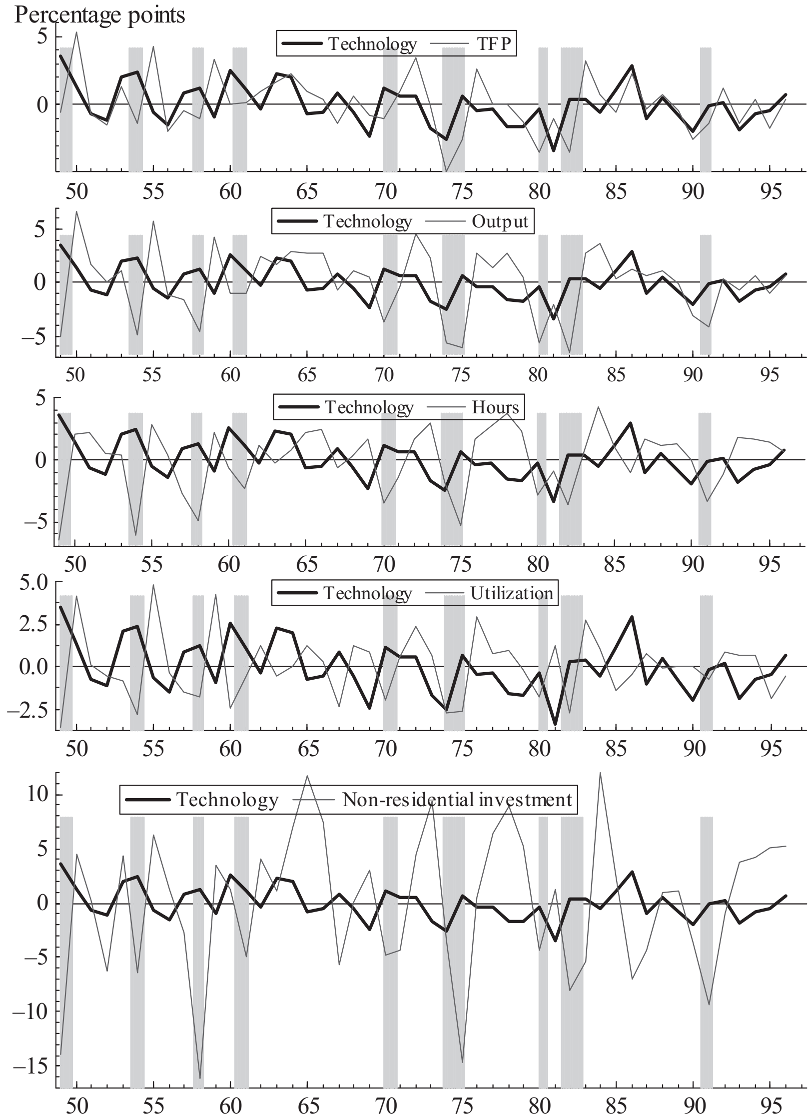

FIGURE 2. TECHNOLOGY, TFP, OUTPUT, HOURS, UTILIZATION, AND NONRESIDENTIAL INVESTMENT
(*Annual percent change*)

Notes: All series are demeaned. Sample period is 1949–1996. All series cover the nonfarm, nonmining private business economy. Technology is the utilization-corrected aggregate residual. For description of series, see text and/or notes to Figure 1 and Table 2. Shaded regions show NBER recession dates.

Dickey-Fuller test, we cannot reject the null of a unit root (p-value of 0.8) in the level. By contrast, with a KPSS test, we *can* reject the null of stationarity (with or without a trend); the p-value is less than 0.01. In addition, technology *growth* shows little evidence of autocorrelation. The point estimates from an autoregression show slight negative autocorrelation with the second lag and positive correlation at the third lag, but the economic and statistical significance appears

TABLE 3—REGRESSIONS ON CURRENT AND LAGGED TECHNOLOGY

Dependent variable (growth rate, unless otherwise indicated)	Regressor dz	dz(−1)	dz(−2)	dz(−3)	dz(−4)	R^2	DW stat.
(1) Output	0.00 (0.21)	1.17 (0.34)	0.52 (0.20)	−0.08 (0.21)	−0.48 (0.20)	0.43	2.39
(2) Hours	−0.60 (0.14)	0.55 (0.27)	0.51 (0.12)	−0.06 (0.16)	−0.41 (0.19)	0.45	1.70
(3) Input	−0.44 (0.09)	0.40 (0.17)	0.43 (0.07)	0.08 (0.12)	−0.21 (0.12)	0.48	1.45
(4) Utilization	−0.40 (0.13)	0.68 (0.15)	0.06 (0.16)	−0.26 (0.12)	−0.23 (0.09)	0.53	2.81
(5) Employment	−0.52 (0.11)	0.36 (0.25)	0.48 (0.09)	0.11 (0.15)	−0.34 (0.18)	0.42	1.56
(6) TFP (Solow residual)	0.44 (0.15)	0.76 (0.20)	0.09 (0.17)	−0.16 (0.13)	−0.27 (0.11)	0.46	2.94
(7) Nonresidential fixed investment	−1.07 (0.36)	1.04 (0.79)	1.63 (0.43)	−0.20 (0.42)	−0.81 (0.61)	0.35	1.43
(8) Residential investment and consumption of durables	1.25 (0.50)	2.83 (0.89)	−0.07 (0.67)	−1.70 (0.47)	−1.36 (0.58)	0.44	2.20
(9) Consumption of nondurables and services	0.10 (0.11)	0.41 (0.11)	0.24 (0.10)	0.03 (0.08)	−0.14 (0.09)	0.40	1.79
(10) Δ Inventories/GDP (*not in growth rates*)	−0.14 (0.03)	0.11 (0.05)	0.14 (0.04)	0.04 (0.05)	−0.02 (0.04)	0.42	1.60
(11) Net exports/GDP (*not in growth rates*)	0.01 (0.13)	−0.03 (0.13)	0.07 (0.10)	0.19 (0.10)	0.20 (0.11)	0.16	0.29

Notes: Each row shows a separate OLS regression of the variable shown (in growth rates, unless otherwise indicated) on current and lagged values of purified technology growth, *dz*, plus a constant term (not shown). Heteroskedasticity- and autocorrelation-robust standard errors in parentheses (calculated with TSP's GMM command with NMA = 3). All regressions are estimated from 1953 to 1996.

small. Thus, in what follows, we assume technology change is a random walk.

Table 3 shows results from regressing a wide range of variables on four lags of technology shocks. (Since purified technology is close to white noise, using more or fewer lags has little effect on coefficients shown.)[20] Purified technology is a generated regressor, so correct standard errors must account for the estimation error involved in estimating technology from the underlying data and the "first step" parameter estimates. As is typical with generated regressors, the correction depends on the true coefficient on technology as well as the first-step estimation error; but if the true coefficient is zero, then the usual standard errors are correct. The standard errors in Table 3 assume the null that the true coefficient is zero.[21]

[20] We interpret our technology shocks as fundamental shocks to the vector moving-average representation of each series. Assuming orthogonality with other fundamental shocks (an assumption not imposed for identification), the coefficients are consistent. We report (Newey-West) heteroskedasticity and autocorrelation-robust standard errors. For most variables, minimizing the Akaike or Schwartz Bayesian Information Criteria suggests two lags, at most. The regressions show more lags for completeness, since adding them has little impact on the dynamics at zero to two lags.

[21] More subtly, however, we want to test the *sign* of the impact-effect coefficient. In particular, we must reject not only the null hypothesis that the coefficient is zero but also that it is positive. In principle, sufficiently large "first-step" estimation error could cause us to reject a true coefficient of zero but *not* reject that the true coefficient is some positive number. The on-line Appendix derives a simple test statistic that allows us to reject this possibility. Hence, if we can reject the null hypothesis of zero, we can also reject the null hypothesis that the true coefficient has the opposite sign from the one reported.

In Table 3, the first row shows that in response to a technology shock, output growth changes little on impact but rises strongly with a lag of one and two years. Output growth is flat in year three, but below normal in year four, possibly reflecting a reversal of transient business cycle effects.

The second row summarizes one of the two key points of this paper: When technology improves, total hours worked fall very sharply on impact. The decline is statistically significant. In the year after the technology improvement, hours recover sharply. The increase in hours continues into the second year.

Total observed inputs (cost-share-weighted growth in capital and labor), row 3, and utilization, row 4, show a similar pattern. Note that utilization recovers more quickly but less persistently. In particular, after the initial decline, utilization rises sharply with a one-year lag but is flat with two lags, even as hours continue to rise. Economically, this pattern makes sense. The initial response of labor input during a recovery reflects increased intensity (existing employees work longer and harder). As the recovery continues, however, rising labor input hours reflects primarily new hiring rather than increased intensity. Thus, one would expect utilization to peak before total hours worked or employment. Indeed, line 5 shows that employment recovers more weakly with one lag than does total hours worked. With two lags, however, as utilization levels off, total hours worked continue to rise because of the increase in employment.

The results for utilization explain the phase-shift in Figure 2. On impact, when technology rises, utilization falls. Measured TFP depends (in part) on technology plus the change in utilization; the technology improvement raises TFP, but the fall in utilization reduces it. Hence, on impact TFP rises less than the full increase in technology. With a one-year lag, utilization increases, which in turn raises TFP.

In sum, the estimates imply that, on impact, both observed inputs and utilization fall. These declines about offset the increase in technology, leaving output little changed. With a lag of a year, observed inputs, utilization, and output recover strongly. With a lag of two years, observed output and inputs (notably the number of employees) continue to increase whereas utilization is flat.

The bottom five rows show selected expenditure categories from the national accounts. Row 7 shows the second key point of this paper: on impact, nonresidential investment falls very sharply; with a lag of one and two years, nonresidential investment rises sharply. Thus, the response of nonresidential investment looks qualitatively similar to the response of total hours worked.

In contrast, residential investment plus consumer durables purchases *rises* strongly on impact, then rises further with a lag. The different response of business and household investment is not surprising. Nonresidential investment is driven by the need for capital in production, whereas the forces driving residential investment and purchases of consumer durables are more closely connected to the forces driving consumption generally. Consumption of nondurables and services rises slightly but not significantly on impact and then rises further (and significantly) with one and two lags. Note that we are largely identifying one-time permanent shocks to the level of technology. Thus, our shocks raise permanent income (though not expected future growth in permanent income). We therefore expect that consumption should rise in response, although habit formation or consumption-labor complementarity, combined with the effects of long-run interest rates, could explain the initial muted response.

The final two rows show the response of inventories and net exports; in both cases, we scale the level by GDP. The inventory/GDP ratio falls significantly; net exports/GDP rises, but insignificantly. These are interesting because, when technology improves, firms could potentially use these margins to smooth production, even if they don't plan to sell more output today.

Figure 3 plots impulse responses to a 1-percent technology improvement for the quantity variables discussed above. Although we could simply plot cumulative responses from the regressions in Table 3, we instead use a complementary approach of estimating bivariate VARs. The impulse responses provide a simple and parsimonious method of showing dynamic correlations. In particular, we estimate (via seemingly unrelated regressions) a near-VAR. The first equation involves regressing dz on a constant term; i.e., we impose that dz is white noise, a restriction consistent with the data. The second equation, for any variable J, regresses dj

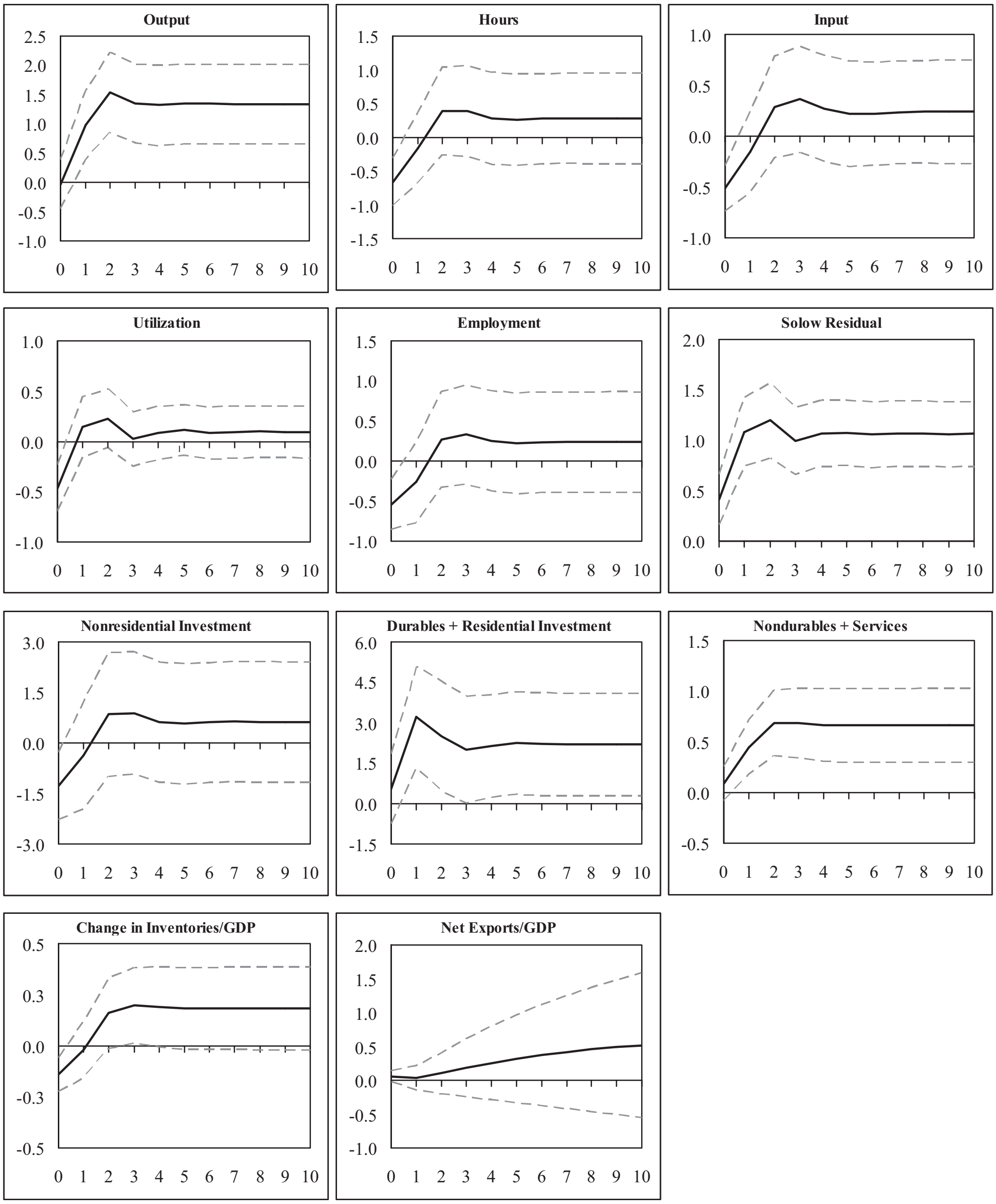

FIGURE 3. IMPULSE RESPONSES TO TECHNOLOGY IMPROVEMENT: QUANTITIES

Notes: Impulse responses to a 1-percent improvement in "purified" technology, estimated from bivariate VARs with two lags, but where purified technology is taken to be exogenous. All entries are percentage points; horizontal scale represents years after the technology shock. Dotted lines show 95-percent confidence intervals, computed using RATS Monte Carlo method. Sample period is 1951–1996.

on two lags of itself and dz. We derive impulse responses (from the moving average (MA) representation) in the standard way from the estimated equations. Relative to Table 3, the VAR approach conserves degrees of freedom by estimating impulse responses from a parsimonious autoregression. Note that we do not use the VAR to *identify* shocks, since we assume that we have already identified exogenous technology shocks.

The impact effect and short-term responses in Figure 3 are generally similar to the regression results. At longer horizons, the impulse responses suggest that output rises about 1.5 times as much as technology; hours, employment, and total inputs rise a bit (but not significantly) relative to pre-shock levels; utilization returns close to its pre-shock level; measured TFP rises almost one for one with technology; and the level of household spending rises. The standard error on nonresidential investment is too large to make any definite statement about its long-run behavior.

C. *Dynamics of Prices and Interest Rates*

Figure 4 shows VAR impulse responses of a range of price and interest rate series. (The price regressions corresponding to Table 3 yield qualitatively similar results.) The top row shows deflators for nonfarm business and several economically sensible aggregates: the combination of (residential and nonresidential) investment and consumer durables; and consumption of nondurables and services.[22] Focusing on total nonfarm business, the price level falls about half as much as the technology improvement on impact; prices continue to fall with one lag and, slightly, with a second lag. The cumulative decline is about 1 percent.

The qualitative results for prices of the two expenditure aggregates are similar. Hence, in the middle panel, when we look at the relative price of investment (including durables) to consumption (nondurables and services), we find very little. (The point estimate suggests that the relative price of investment rises insignificantly.) A growing literature focuses on "investment-specific" technical change (for example, Jeremy Greenwood et al., 1997, and Jonas Fisher, 2006). Since we use chain-linked data, our technology series is a weighted average of consumption- and investment-sector technology change. That we don't find a change in the relative price of investment suggests that shocks to technical change, on average, are largely neutral. (This does not preclude a difference in the mean rate of technical change for investment goods as opposed to consumption goods.)

The remaining responses on the second row show that the nominal federal funds rate and the nominal three-month T-Bill rate both decline noticeably and remain below normal for an extended period. The third row shows that the real interest rate appears to decline, but modestly. (Interestingly, the decline is sharper for the federal funds rate than for the three-month Treasury rate.)

Finally, we include real and nominal values of the exchange rate and wage. The exchange rate depreciates sharply when technology improves. (We note, however, that the sharp appreciation of 1980–1985 and depreciation of 1985–1988 dominate the data. Adding separate dummies for those two periods markedly reduces both the magnitude and statistical significance of the coefficients.) The nominal wage stays flat; with a fall in the price level, the measured real wage increases. We hesitate to overinterpret the increase in the real wage, however, since observed wages might not be allocative period by period.

IV. Robustness Checks

We now address robustness. We report a range of VAR specifications and Granger-causality tests; put purified technology into a long-run structural VAR; and look at the industry technology shocks themselves. The on-line Appendix discusses econometric issues of input measurement error and small-sample properties of instrumental variables. Our basic finding that input use and investment covary negatively with technology is robust.

A. *Alternative VAR Specifications and Granger Causality*

Reported results are affected little if, instead of taking our technology series as white noise, we allow the series to be autoregressive and/or allow shocks to variable J to affect technology with a lag (e.g., if we use the standard ordering identification in a VAR). Figure 5 illustrates this robustness with six different estimates of the hours response and four different estimates of the nonresidential investment response. The

[22] We use inflation rates, wage growth, and interest rate levels in the VAR, along with decadal dummies for the 1970s and 1980s. We plot cumulative effects on price, wage, and interest rate levels.

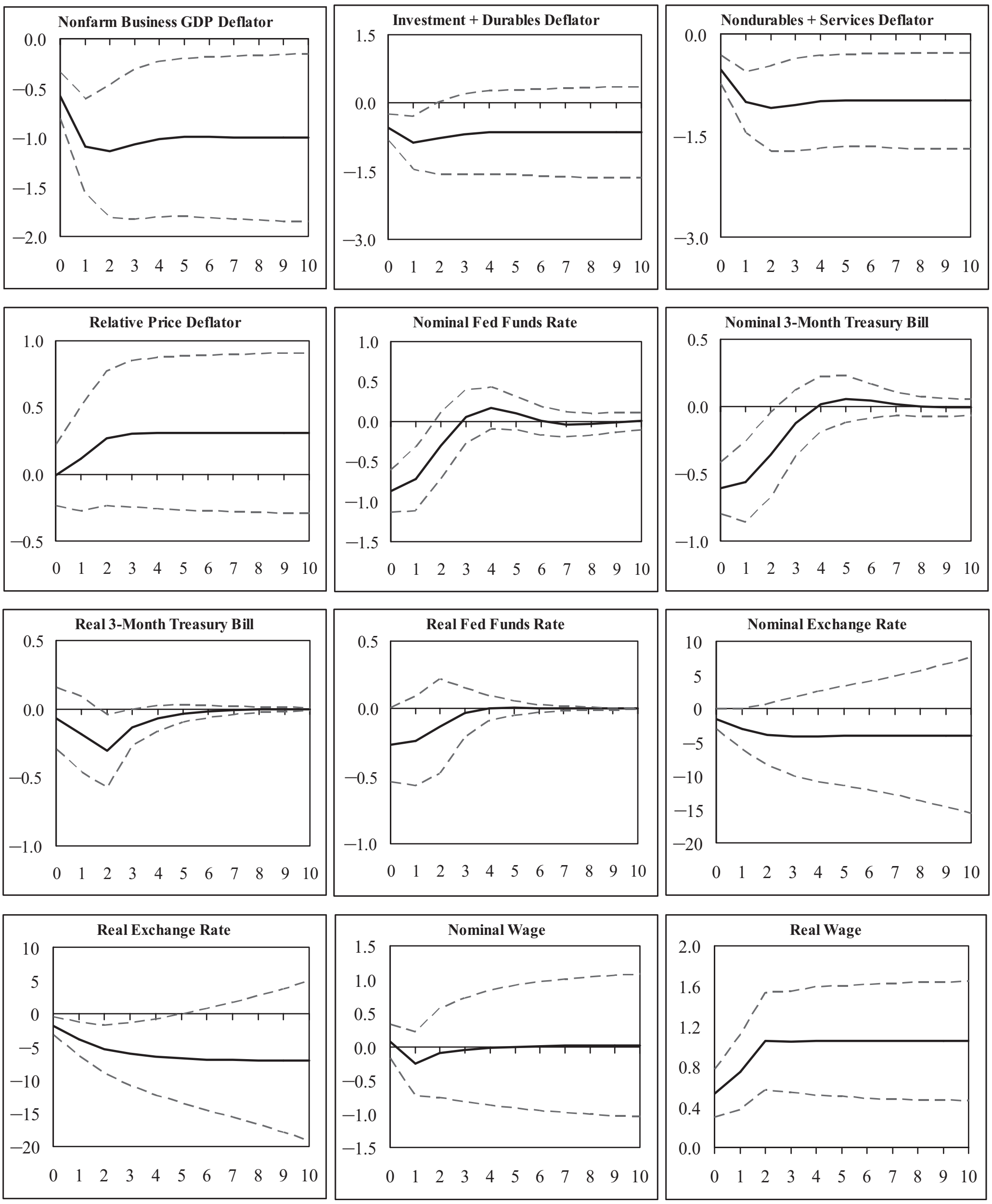

FIGURE 4. IMPULSE RESPONSES TO TECHNOLOGY IMPROVEMENT: PRICES AND INTEREST RATES

Notes: Impulse responses to 1-percent improvement in "purified" technology, estimated from bivariate VARs with two lags, where purified technology is taken to be exogenous. All entries are percentage points; horizontal scale represents years after the technology shock. VARs for prices, interest rates, and wages include decadal dummy variables for the 1970s and 1980s. Nominal and real trade-weighted exchange rates are Federal Reserve Board (broad) indices; an increase represents an appreciation. Investment includes residential and nonresidential investment; relative price deflator is ratio of deflator for investment (residential and nonresidential) and consumer durables to the price deflator for consumer nondurables and services. Dotted lines show 95-percent confidence intervals, computed using RATS Monte Carlo method. Sample period is 1951–1996 except for the fed funds rate (1957–1996) and real/nominal exchange rate (1976–1996).

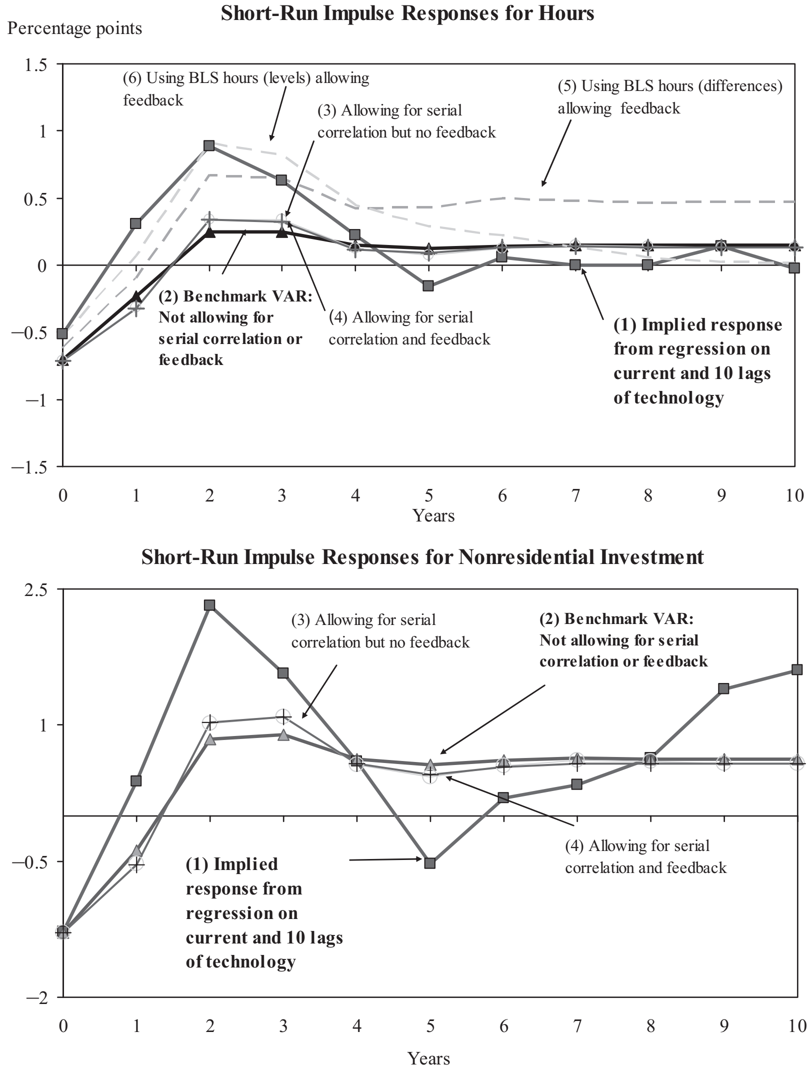

FIGURE 5. ALTERNATIVE ESTIMATES OF THE HOURS AND INVESTMENT RESPONSES TO A TECHNOLOGY IMPROVEMENT

Notes: Each line represents the impulse response from a separate estimation. For all specifications shown, the impact effect (year 0) is statistically significantly negative. (1) is cumulated response from regressions on current and 10 lags of technology; sample period is 1959–1996. (2)–(6) are from bivariate VARs with two lags, estimated 1951–1996. (2) does not allow serial correlation or feedback in the equation for purified technology; (3) allows serial correlation; (4)–(6) allow serial correlation and feedback. In top panel, (1)–(4) use aggregate hours growth from Jorgenson dataset; (5) and (6) use growth and log-level of BLS nonfarm business hours per capita (age 16 and older).

thick line with boxes shows the implied response from direct regressions on growth in current and 10 lags of technology. (This approach uses a lot of degrees of freedom, so the sample period runs only from 1959 to 1996. The shorter sample period is the main reason why the direct regression response lies above the other responses at short horizons.) The thick line with triangles shows our benchmark VAR response, where we assume that purified technology is an exogenous white-noise process. The two thin lines (almost indistinguishable in the figures) show results (a) allowing for serial correlation of technology in the VAR, i.e., adding lags of technology growth to the technology equation, and (b) allowing for serial correlation *and* (lagged) feedback of shocks to hours or investment on technology (i.e., putting lagged growth in hours or investment into the technology equation). In the top panel, the final two dashed lines (lines 5 and 6) use Bureau of Labor Statistics (BLS) nonfarm business hours worked per capita (age 16+) rather than Jorgenson's hours growth, since the SVAR literature focuses on BLS data (and focuses on some apparent differences when hours per capita enter in levels or differences). Those specifications allow for serial correlation and feedback. (Results using total BLS private business hours per capita are similar to the nonfarm responses.) It is clear that, in this setting, the distinction between levels and differences is inconsequential.

The bottom line is that the impact effect is very similar in all cases. The graphs uniformly show that hours and nonresidential investment fall on impact and bounce back robustly with one and two lags. The initial declines are statistically significant in all cases.

This robustness is not surprising, since lags of technology have little explanatory power for current technology. In addition, the variables we examine in this paper (plus various measures of government spending) do not appear to Granger-cause technology, so we cannot reject the exogeneity assumption.

Christiano et al. (2004) suggest that the *level* of hours per capita Granger-causes the technology series from an earlier version of this paper. However, neither in levels nor in growth rates are Jorgenson's or the BLS nonfarm business hours series even remotely significant; for example, the p-value on two lags of the (log) level of BLS nonfarm business hours per capita (age 16+) is 0.35. Christiano et al. (2004) use private business hours rather than nonfarm business hours; the p-value of 0.11 is still insignificant, although it's much closer.

Christiano et al. (2004) might argue that farm hours Granger-cause our technology series,[23] but Fernald (2005) points out that even this relatively high level of significance reflects the productivity slowdown. Both average technology growth and the level of total business hours per capita were higher before 1973 than after. Indeed, private business hours per capita appear to Granger-cause the productivity slowdown (using a series that is 1 before 1973 and 0 afterward): estimated 1951–1996 with two lags, the p-value is 0.02. Nonfarm business hours per capita do not show the same pattern. Hence, when we estimate the same Granger-causality test with purified technology that *excludes* the industry constant terms and the trend break, the p-value for BLS private business hours rises to 0.39. This example points out a limitation of Granger-causality tests in this context. Quite clearly, the Granger-causality evidence in Christiano et al. (2004) reflects a low-frequency correlation, not high-frequency "measurement error" in purified technology.

Nevertheless, our procedure doesn't *require* strict exogeneity of technology (so that dz_t is independent of other shocks at time τ, where τ need not equal t). Our identification does require that our instruments not be correlated contemporaneously with true technology. But suppose, for example, that an expansionary money (interest rate) shock leads firms to cut back on R&D, which reduces future technology growth; dz_t then depends on past monetary shocks, which would Granger-cause technology. Nevertheless, it seems likely that the lags are longer than a year, so that our identification assumption still holds. That said, we find no evidence that any of the other variables we examine Granger-causes technology.[24]

B. *Long-Run Restrictions*

A large literature measures technology innovations using VARs with the long-run

[23] Hours worked by farmers are poorly estimated relative to the number of employees, which is a reason to prefer nonfarm measures. But with two lags, the log of the number of agriculture employees (from the household survey) indeed Granger-causes purified technology with a p-value of under 0.02. Farm employees proxy nicely for the productivity slowdown, since they fall by more than half from 1949 to 1971, but remain fairly level thereafter.

[24] Paul Beaudry and Franck Portier (2004) present a model where current behavior reflects (imperfectly) anticipated future changes in technology. Hence, current variables could in principle Granger-cause even completely exogenous future technology.

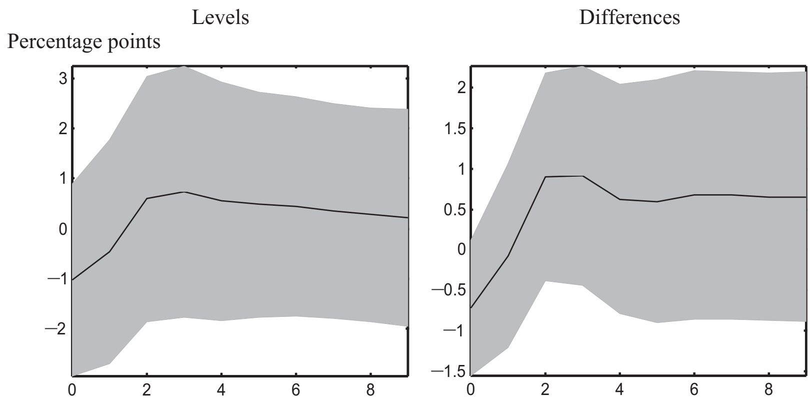

FIGURE 6. ESTIMATES FROM VARs WITH LONG-RUN RESTRICTIONS: HOURS RESPONSE TO A TECHNOLOGY IMPROVEMENT

Notes: Responses identified from the assumption that only "true" innovations to technology permanently affect the level of purified technology. Response shows percentage deviation of the level of hours; horizontal scale represents years after the technology shock. The "level" specification uses the log-level of hours worked from Jorgenson dataset (private nonfarm, nonmining business) divided by the population age 16 and older. The difference specification uses the growth rate of hours worked per capita; 95-percent confidence interval shown.

identifying restriction that only technology shocks affect labor productivity in the long run. Christiano et al. (2004) suggest replacing labor productivity with our "purified" technology series, hoping the long-run restriction might clean out any remaining high-frequency cyclical measurement error.[25] As in that literature, we focus on the response of hours to technology, even though (as we discuss below) the response of business investment to technology may be even more decisive for the key theoretical issues. We consider only bivariate specifications here.

Suppose *PR* is the log of the productivity measure to which one applies the long-run restriction (labor productivity or the level of purified technology); *HR* is some function of the log of hours worked, for example, either the log-level or the growth rate of hours per capita. Assuming two shocks (a technology shock, ε_t^Z, and a nontechnology shock, ε_t^N), then the long-run restriction is that $C_{12}(1) = 0$ in the system's MA form:[26]

$$\begin{bmatrix} \Delta PR_t \\ HR_t \end{bmatrix} = \begin{bmatrix} C_{11}(L) & C_{12}(L) \\ C_{21}(L) & C_{22}(L) \end{bmatrix} \begin{bmatrix} \varepsilon_t^Z \\ \varepsilon_t^N \end{bmatrix}.$$

Following Christiano et al. (2004), we estimate bivariate VARs with two lags, defining *PR* as purified technology. We identify "true" long-run technology shocks as the estimated shock that affects the long-run level of technology. For *HR*, we use Jorgenson's hours series per capita (16+), either in log-levels or in log-differences. Figure 6 shows the impulse responses from these two specifications. The responses look qualitatively very similar to the short-run specifications discussed earlier, although with wider confidence intervals. In particular, technology improvements reduce hours worked; hours then recover with a lag.

The resulting technology series has a correlation of 0.82 (level specification) or 0.97 (dif-

[25] Christiano et al. (2004) cite "countercyclical markups" which, in our setup, presumably means "countercyclical returns to scale." This effect would not, however, lead to cyclical measurement error. Suppose the true (time-varying) value is γ_t but we estimate a constant $\bar{\gamma}$; then the estimated error term contains $(\gamma_t - \bar{\gamma})dx$. Countercyclical γ_t implies that this extra term is always negative, so the main effect is on the constant term rather than the cyclicality of the residual. Note also that Shapiro and Watson (1988) argue against using TFP growth, since it is naturally defined in first differences, as is our purified technology dz. In particular, the long-run restriction would label as technology any classical measurement error. Thus, the long-run VAR will not clean out all sources of misspecification.

[26] See Christiano et al. (2003) for details of estimation. We thank Robert Vigfusson for providing the computer code used to calculate confidence intervals in that paper.

ference specification) with our original purified technology series. Estimating the VAR with a 1973 trend-break in productivity brings both correlations to about 0.9. When we define *PR* as aggregate labor productivity (following Galí, 1999a), including the trend break, the correlation of the resulting technology series with our purified series is 0.78 (levels) or 0.75 (differences). (Using annual BLS data on nonfarm business labor productivity and hours per capita, the correlations between our purified technology series and the identified technology shocks in both the levels and difference specifications are about 0.6.) Thus, it is clear that we are identifying a similar shock.

Given the sensitivity to low-frequency correlations discussed in Fernald (2005) and Erceg et al. (2005), one needs caution in interpreting results from long-run restrictions. Nevertheless, because their identification assumptions are very different from ours, they provide useful complementary evidence.

C. *One- and Two-Digit Industry Results*

Results do not arise from aggregation or due to a small number of industries. For our 29 industries and for 9 (approximately one-digit) industries, Table 4 correlates standard industry TFP and purified technology with inputs and gross output. For all 29 industries, the median correlation of inputs with TFP (Corr(dp, dx)) is 0.15; the median correlation with purified technology (dz) falls to -0.33. The median correlation with output falls from 0.57 (TFP) to 0.01 (technology). Technology covaries negatively with inputs in 24 of the 29 industries.

In results not shown, we also correlated industry technology residuals with SVAR-identified innovations (identified as in Section IVB, using growth rates of industry labor productivity and hours). The median industry correlation is 0.71; 27 of the 29 correlations are statistically significant (95-percent level). For 22 industries, industry hours fall on impact when SVAR-identified technology improves.

V. Interpretations of the Results

A. *The Standard RBC Model*

The data show that technology improvements reduce hours and nonresidential investment on impact, while leaving nondurable consumption and output unchanged. By contrast, the standard RBC model (e.g., Cooley and Edward C. Prescott, 1995) predicts that improved technology should raise output, investment, consumption, and labor hours on impact.

Certainly, alternative calibrations of the RBC model could deliver a fall in labor. Technology improvements raise real wages, which has both income and substitution effects. If the income effect dominates, labor input might fall.[27] But even with strong income effects, it is unlikely that we would observe the "overshooting" response of hours that we find in the data. The standard RBC model displays monotonic convergence to the steady state, at least in the linearized dynamics. Thus, if hours fall temporarily due to an income effect, they should remain low persistently, and converge to their long-run value from below.

Nevertheless, the fall in nonresidential investment most strongly contradicts basic RBC theory. In standard calibrations, a permanent technology improvement increases consumption and investment together.[28] Residential investment and consumer durables display the expected pattern, but business investment does not.[29]

On the other hand, the effects after two to three years are clearly consistent with RBC models: output, investment, consumption, and labor hours are all significantly higher. And the size of the long-run output response is

[27] As in Jesper Lindé (2003), positively autocorrelated technology change could also lead workers to take more leisure initially and work harder in the future, when technology is even better. However, our technology process is not autocorrelated.

[28] In an open economy, especially, one can increase imports, so it is easy to increase both consumption and investment.

[29] Our estimates also contradict King and Rebelo's (1999) attempt to "resuscitate" the RBC model. By adding variable capital utilization to the basic RBC model, King and Rebelo improve the model's ability to propagate shocks. They use their calibrated model to back out an implied technology series from observed TFP. By construction, their procyclical technology series, however small, drives business cycles. Our empirical work, by contrast, does not impose such a tightly specified model—and the data reject the King and Rebelo model. Hence, their model is not an empirically relevant explanation of business cycles any more than the basic RBC model is. Instead, the main lesson we take from their paper is the importance of utilization as a propagation mechanism, which applies to more realistic models as well.

TABLE 4—ONE-DIGIT AND INDUSTRY-AVERAGE CORRELATIONS

	TFP and output Corr(dp, dy)	TFP and input Corr(dp, dx)	Technology and output Corr(dz, dy)	Technology and input Corr(dz, dx)
Construction	0.47***	0.15	0.38***	0.07
Manufacturing durables	0.75***	0.64***	−0.44***	−0.49***
Manufacturing nondurables	0.67***	0.32**	−0.10	−0.22
Transport	0.68***	0.27*	0.34**	−0.10
Communications	0.60***	−0.07	0.19	−0.47***
Public utilities	0.66***	0.20	0.16	−0.30**
Trade	0.63***	−0.31**	0.57***	−0.36**
FIRE	0.47***	−0.28*	0.74***	0.11
Services	0.81***	0.26*	0.56***	−0.07
Median of one-digit correlations	0.66	0.20	0.34	−0.22
Median of 29 industries (21 manufacturing, 8 other)	0.57	0.15	0.01	−0.33
Number of industries with negative correlation	3	12	14	24
Memo: aggregate correlations	0.84	0.34	−0.08	−0.48

Notes: The 29 individual industries and the 9 one-digit industries span the private nonfarm, nonmining business economy. Standard industry TFP growth (the Solow residual) is dp. Technology dz is the purified industry technology residual. One-digit aggregates for manufacturing durables, manufacturing nondurables, and utilities use sectoral gross-output weights. All correlations are calculated from 1949 to 1996. For one-digit correlations, *** indicates statistical significance at the 1-percent level, ** at the 5-percent level, and * at the 10-percent level.

quantitatively close to the prediction of a balanced growth model: a 1-percent increase in Hicks-neutral technology should increase output by $1/(1 - \alpha)$ percent, where α is the output elasticity of capital. Assuming constant long-run returns to scale and a capital share of one-third, output should rise by 1.5 percent. The response in Figure 4 (or the cumulated response from Table 3) matches this prediction.

Thus, the short-run (but not the medium- and long-run) effects of technology improvements contrast sharply with the predictions of standard RBC models.[30] Are those models correct, however, in assuming that technology shocks are the dominant source of short-run volatility of output and inputs? Table 5 reports variance decompositions from the impulse responses in Figure 3. At the business-cycle frequency of three years, technology shocks account for more than 40 percent of the variance of output, but only 9 to 18 percent of the variance of different input measures. The patterns are intuitively sensible: hours and utilization respond much more to technology at high frequencies. (Steady-state growth, of course, requires that long-run labor supply be independent of the level of technology.) By contrast, technology accounts for only about 17 percent of the initial short-run variance of measured TFP, but 70 percent with a lag of three years. Again, this pattern accords with our priors: in the short run, changes in utilization and composition account for much of the volatility of measured TFP; but in the long run, TFP reflects primarily technology.

Our findings thus lie between RBC and New Keynesian positions. Technology shocks are neither the main cause of cyclical fluctuations, nor negligible. Future models should allow for technology shocks, while making sure that the impulse responses match those that we and others find.

B. *A Flexible-Price Model with "Real Inflexibilities"*

Francis and Ramey (2005), as well as Robert J. Vigfusson (2004), modify the standard RBC model with habit persistence and investment adjustment costs. Since domestic demand is inertial, hours worked fall when technology improves. As Galí and Rabanal (2005) note, this model is particularly interesting because many business cycle models, with and without nominal rigidities, assume this kind of real inertia in demand.

[30] For more discussion, see Kimball (2003).

TABLE 5—FRACTION OF VARIANCE DUE TO TECHNOLOGY SHOCKS

Lags	Output	Inputs	Hours	Utilization	Solow residual
0	0	30	24	24	17
1	17	14	12	21	56
3	40	13	9	18	70
10	48	9	4	11	76

Notes: Variance decomposition from bivariate VAR of technology and the variable shown. In all cases, we assume technology innovations are white noise with no feedback from the variable shown.

The slow rise of nondurable consumption is broadly consistent with the Francis and Ramey (2005) model, but the investment response is not. In general, although the zero impact effect of technology improvements on output is consistent with their model, the response of output components is not. Empirically, the lack of an immediate output response incorporates sizable jumps in two components of investment, in opposite directions. (Notably, in the first year, nonresidential investment jumps down, while residential investment plus consumer durables purchases jumps up.) These large jumps are not consistent with a model where investment adjustment costs are large.

C. *Price Stickiness/Imperfect Common Knowledge*

Technology improvements can easily reduce both hours and investment in a sticky-price model. Suppose the quantity theory governs the demand for money and the supply of money is fixed. If prices cannot change in the short run, then neither can real balances or output. Now suppose technology improves. Since the price level is sticky and demand depends on real balances, output does not change in the short run. But firms need fewer inputs to produce this unchanged output, so they lay off workers, reduce hours, and cut back on fixed investment. (To keep private output constant, the sum of the other components of output—consumer durables, residential investment, and nondurables and services—has to increase.) Over time, however, as prices fall, the underlying RBC dynamics take over. Output rises, and the higher marginal product of capital stimulates capital accumulation. Work hours eventually return to their steady-state level.

These effects are present in virtually any sticky-price DGE model when the nominal money supply is held constant, such as Kimball's (1998) neomonetarist model. Kimball (1998) finds that on impact, output could even decline when technology improves, reflecting the decline in business investment. Two effects reduce investment. First, the demand for all inputs declines, including the demand for capital services, resulting in a lower rental rate of capital for any given level of output. Second, if a technology improvement leads to an anticipated decline in the price of investment (as well as other) goods, then firms prefer to hold bonds instead of investing in plant and equipment on which they will take capital losses. Price declines follow this pattern in the data: Figure 4 shows that the price of investment goods falls about 1 percent in the first two years following a 1-percent technology improvement.

How can sticky-price models explain the sharp rise in residential investment and consumer durables purchases, when investment in plant and equipment falls? On the demand side, business demand for capital services depends heavily on current levels of other inputs relative to current capital, and this ratio falls after technology improves. By contrast, household demand for the services of consumer durables and housing depends primarily on permanent income, which rises. On the cost side, residential housing purchases appear to be more sensitive to interest rates than is corporate investment. The federal funds rate falls by about a percentage point in the year that the technology shock occurs (see Figure 4) and the real fed funds rate also declines, albeit by less.[31]

Basu (1998) and Basu and Kimball (2004) calibrate DGE models with staggered price

[31] Also, construction prices respond more quickly than the prices of investment goods in general. Robert Barsky et al. (forthcoming) argue that housing prices are relatively flexible, suggesting they might adjust quickly to technology shocks.

TABLE 6—RESPONSES BY SUBPERIOD

	1949–1979			1980–1996				
	dt	*dt*(−1)	*dt*(−2)	*dt*	*dt*(−1)	*dt*(−2)	R^2	DW statistic
Output	−0.10	1.14	1.03	0.33	1.18	0.00	0.47	2.37
	(0.31)	(0.39)	(0.26)	(0.19)	(0.52)	(0.25)		
Hours	−0.62	0.50	0.91	−0.29	0.77	0.32	0.50	1.72
	(0.2)	(0.34)	(0.16)	(0.16)	(0.34)	(0.11)		
Nonresidential investment	−0.65	1.28	2.81	−1.31	0.65	0.62	0.40	1.51
	(0.51)	(0.82)	(0.46)	(0.49)	(1.25)	(0.69)		
Nonfarm business deflator	−1.03	−0.80	−0.50	−0.87	−0.57	−0.06	0.83	1.59
	(0.15)	(0.22)	(0.15)	(0.22)	(0.07)	(0.13)		
Real fed funds	0.14	0.06	0.15	−0.48	−0.60	−0.48	0.77	1.74
	(0.19)	(0.2)	(0.16)	(0.22)	(0.1)	(0.14)		

Notes: Coefficients from bivariate regressions of the growth rate of the variable shown on current and two lags of purified technology growth, *dz*. The coefficients are allowed to differ by subperiod. All regressions include decadal dummies (the 1970s dummy is important for the nonfarm business deflator; the 1980s dummy is important for the real fed funds rate). Heteroskedasticity- and autocorrelation-robust standard errors in parentheses.

setting, and reproduce the impact effect of technology improvements that we find in the data.[32]

Of course, the monetary authority is likely to follow a more realistic feedback rule than simply keeping the nominal money stock constant, as our discussion has assumed so far. Would it accommodate technology improvements by loosening policy, thereby avoiding the initial contraction? In a large class of models where the monetary authority has full information and policy is fully flexible, such an action would be both feasible and optimal. The evidence on the real federal funds rate in Figure 4 suggests that when technology improves, the Federal Reserve does indeed respond by lowering the real fed funds rate. Still, the Fed might not react strongly enough. First, it is hard to be sure in real time that technology has improved. Even if the Fed sees that inflation has fallen for one or two quarters, it might not realize how persistent the effect is. Second, if the Fed smoothes interest rates (for example, Richard Clarida et al., 1999; Petra Gerlach-Kristen, 2004), then by definition, this smoothing slows down the Fed's response to shocks.

The best evidence that the Fed does not take sufficient action to fully offset the contractionary effects of a technology improvement lies in the behavior of the price level. Standard models of optimal monetary policy often imply that inflation or the price level should be kept stable in the face of technology shocks. That is, optimal monetary policy would ensure that the technology shock has no effect on inflation at any horizon, and therefore leaves the price level unchanged.[33] But this is not what we observe in the data: the short-run behavior of prices accords with a model where the Fed does not accommodate technology shocks fully. In fact, the long-run fall in the price level is almost the same as the long-run increase of output, indicating a low degree of effective accommodation (see also Michael T. Kiley, 2003).

Galí et al. (2003) suggest that the contractionary effect on inputs is less pronounced under the Volcker-Greenspan Fed than previously; they hypothesize that monetary policy better accommodated technology improvements in the later period. Table 6 shows regressions where we allow the coefficients on technology (as well as constant terms) to differ by subsample. We include current and two lags of technology. The evidence is mixed: the responses of output and hours are consistent with the hypothesis that monetary policy has become more effective, but the impact decline in nonresidential investment is even larger in the later subperiod. Formal statistical tests for subsample differences, however, do not reject the hypothesis that the responses of the real variables to technology

[32] Because Basu's model has few "real rigidities," the contraction is short-lived. Kimball (1995) shows that one can obtain a "contract multiplier" of any desired size by adding real rigidities to the model.

[33] See, e.g., Woodford (2003b, pp. 461–62), who in turn cites Aubhik Khan et al. (2002) on this point.

shocks are the same in the two subperiods. This is true for both the impact effect and the cumulative effect of technology. The cumulative effect on the price level, but not the impact effect, is marginally different at the 10-percent level. Only the response of the fed funds rate is significantly different across the two subperiods. Hence, our results provide, at best, only weak support for the hypothesis advanced by Galí et al. (2002).

Monetary policy can respond only to changes in aggregate technology. Thus, data on firms, where technical change is presumably mostly idiosyncratic, provide a good test of the sticky-price hypothesis. Some recent firm-level evidence does, in fact, link the short-run effects of technology to sticky prices. Domenico Marchetti and Francesco Nucci (2005) apply exactly our identification method to Italian firm-level data and, like us, find that technology improvements reduce input use. But they also have data on the frequency with which firms change prices. They find that technology improvements reduce input use only at firms that have rigid prices. This evidence ties the contractionary result directly to price rigidity. While theory and evidence link contractionary technology improvements to price inflexibility, prices may be rigid for a variety of reasons. The models cited above assume that price changes are time-dependent in an environment with perfect information. But prices may be rigid endogenously in an environment where there are no barriers to changing prices but information is imperfect, as in Woodford (2003a). Kawamoto (2004) shows that in such a setting, imperfect common knowledge about technology shocks can cause labor input to fall when technology improves. He also emphasizes that models with imperfect information usually do better than perfect-information models with price rigidity at explaining the sluggishness of disinflation after a technology improvement. Imperfect-information models also explain why the Fed does not react more quickly to technology shocks, and thus seem promising candidates to explain many of the results that we find.

D. *Sectoral Shifts?*

Even with flexible prices, if technology change is uneven across sectors, then output and inputs might temporarily fall because reallocating resources is costly. (Ramey and Shapiro, 1998, document these costs for capital.) Our data, however, do not appear to support the sectoral-shifts alternative.

Reallocation pressures presumably depend positively on the dispersion of technology shocks. Thus, we add a measure of technology dispersion to our basic regressions and see whether it significantly explains input and output growth.[34] A natural dispersion measure, *Disp*, is the cross-sectional standard deviation in technical progress,

$$Disp_t = \left[\sum_{i=1}^{N} w_i (dz_{it}^V - dz_t)^2 \right]^{1/2},$$

where i indexes industries, dz_{it}^V is the estimated industry technology shock, scaled to be value-added augmenting as in equation (4), and w_i is the sector's value-added weight.[35]

It seems unlikely that our technology impulse proxies for dispersion effects, since the two variables are close to uncorrelated. More formally, in Table 7 we regress output growth, various measures of input growth (total inputs, hours, and utilization), and business investment on purified technology along with current and two lagged values of *Disp* (adding more lags makes little or no difference).

In all cases, adding *Disp* has relatively little effect on the coefficients, standard errors, and timing patterns of technology and its lags. The addition of the *Disp* variables leads to only a moderate improvement in the R^2 of the regressions—the increase is between 0.02 and 0.07. Interestingly, with a one-year lag, technology dispersion is associated with lower growth in output, utilization, business investment, and, less significantly, hours and total inputs. The one-year lag

[34] David Lilien (1982), who argues for the importance of sectoral shifts, measures reallocation as the cross-industry variance of employment growth. Our measure does not rigorously test the sectoral shifts alternative, since a common aggregate shock affects optimal input use equally in all sectors only if all production and demand functions are homothetic. Nevertheless, even if imperfect, our measure should capture some of the forces leading to input reallocation.

[35] We remove constant terms and trend breaks from industry and aggregate technology before calculating $Disp_t$. Note that dz_t is already defined on a value-added basis.

TABLE 7—EFFECT OF TECHNOLOGY IMPROVEMENTS AND TECHNOLOGY DISPERSION ON GROWTH RATES OF OUTPUT, INPUT, UTILIZATION, AND NONRESIDENTIAL FIXED INVESTMENT

	Output dv	Inputs dx^V	Hours	Utilization	Nonresidential investment
$dz(t)$	0.10	−0.43	−0.56	−0.34	−0.96
	(0.17)	(0.08)	(0.13)	(0.11)	(0.39)
$dz(t-1)$	0.99	0.36	0.48	0.58	0.79
	(0.30)	(0.16)	(0.27)	(0.11)	(0.71)
$dz(t-2)$	0.62	0.47	0.56	0.12	1.69
	(0.19)	(0.10)	(0.15)	(0.15)	(0.47)
$dz(t-3)$	−0.02	0.08	−0.04	−0.23	−0.22
	(0.19)	(0.14)	(0.19)	(0.09)	(0.56)
$dz(t-4)$	−0.66	−0.27	−0.50	−0.33	−1.13
	(0.23)	(0.13)	(0.20)	(0.12)	(0.61)
$Disp(t)$	0.23	−0.01	0.08	0.14	−0.06
	(0.19)	(0.10)	(0.17)	(0.15)	(0.47)
$Disp(t-1)$	−0.69	−0.18	−0.30	−0.37	−1.02
	(0.21)	(0.10)	(0.17)	(0.11)	(0.50)
$Disp(t-2)$	0.32	0.12	0.15	0.17	0.09
	(0.28)	(0.16)	(0.23)	(0.12)	(0.58)
R^2	0.50	0.50	0.47	0.58	0.39
D.W.	2.31	1.45	1.68	2.73	1.51

Note: Each column is a separate regression of the growth rate of the variable shown on current and lagged values of *dz* (purified technology) and *Disp* (the weighted cross-sectional standard deviation of industry technology shocks). Regressions include a constant, not shown. Heteroskedasticity- and autocorrelation-robust standard errors in parentheses. Sample period is 1953–1996.

makes sense, since the costs of sectoral shifts should be associated primarily with changes in employment, which occur with a lag after a technological impulse. Overall, there is some evidence for a sectoral shift effect, but this effect is more or less orthogonal to the evidence for the direct effect that we attribute to sticky prices. Moreover, the effects of sectoral shifts seem to come after the initial year in which we find the most dramatic contractionary effects.

E. *Time to Learn?*

Several authors have argued that technological improvements may reduce measured growth for a time, as the economy adjusts to new production methods. For example, Greenwood and Mehmet Yorukoglu (1997) argue that the introduction of the personal computer caused the post-1974 slowdown in economic growth, since workers and firms had to accumulate new human capital. That is, when new technology is introduced, unobserved investment is high; but since the national accounts do not include investments in human (or, in most cases, organizational) capital as output, market output—and hence measured productivity growth—might be low. Therefore, low observed productivity growth is associated with high input growth, because "full" output is mismeasured. Over time, the investment in knowledge does raise measured output and productivity.[36]

This class of models does not generally predict our results. We do not correct for mismeasured output arising from unobserved investments in knowledge; hence, when technology is introduced, we would conclude (incorrectly) that technology fell. Since measured (as well as unmeasured) inputs are likely to rise at those times, we might find that technology contractions coincide with input expansions. But with a lag, when market output rises, we would measure a technology improvement—coinciding with a boom. Hence, measured technology improvements would appear *expansionary.* Figure 2 suggests that the negative correlation between measured technology and outputs reflects technology improvements as well as declines (relative to trend), so the time-to-learn story is unlikely to explain our results.

[36] Other references include Oded Galor and Daniel Tsiddon (1997) and Basu et al. (2004).

F. *The "Cleansing Effect of Recessions"?*

Could causality run from recessions to technical improvement, rather than the reverse? For example, if recessions drive inefficient firms out of business, then overall productivity might rise.[37] This hypothesis predicts countercyclical productivity, so proponents have argued that "other factors (labor hoarding, externalities, etc.) ... make measured productivity procyclical" (Ricardo J. Caballero and Mohamad L. Hammour, 1994, p. 1365). Possibly, by controlling for these "other factors," we have uncovered the cleansing effects.

With firm-level data, endogenous cleansing would not be a concern. Basu and Fernald (2002) classify such cleansing effects as "reallocations"—a shift in resources from inefficient to efficient firms—not as changes in firm-level technology. Our theory excludes such effects by adding up changes in *firm-level* technology to derive aggregate technology dz. But in practice we use industry data, and estimates of industry technical change could include *intra*-industry reallocations. As noted earlier, however, Marchetti and Nucci (2005) confirm our findings with firm-level data. (Of course, there are no firm-level datasets spanning the economy, so one cannot use firm-level data and address the aggregate macro issues considered here.)

In addition, cyclical reallocations are likely to affect estimated returns to scale rather than the cyclicality of residuals. Suppose that for an industry, $dy = \gamma dx + R + dz$, where intra-industry reallocations R depend, in part, on input growth dx: $R = \delta dx + \xi$. A cleansing effect of recessions implies $\delta < 0$; ξ captures any reallocation effects that are uncorrelated with input growth. Even if our instruments are uncorrelated with technology, they may be correlated with reallocations. Suppose ξ is uncorrelated with either the instruments or any cyclical variables. Then plim $\hat{\gamma} = (\gamma + \delta) < \gamma$, but the estimated technology shocks do not incorporate causation from inputs to technology. ξ is a form of classical measurement error in our residuals. (This cleansing effect could explain why some of our estimated industry returns to scale are less than one.[38])

If, however, ξ is correlated with business-cycle variables—for example, if reallocations depend on the aggregate cycle, even after controlling for the level of activity in the industry—then reverse causality might contribute to correlation of residuals with changes in inputs or output.

The cleansing explanation challenges our basic identifying assumption that industry technical change is exogenous. But Granger-causality tests suggest our results are not being driven by reverse causality. That is, if some of the cleansing effects work with a lag of more than a year, then lagged output or input growth should predict our measure of technology change. (It is sensible to expect lags, since entry and exit of firms could be a relatively slow phenomenon.) But we do not find that lagged output or input growth significantly predicts our measures of technology. This casts doubt on the cleansing interpretation of our results.

A second variant of cleansing models might be termed models of "recessions as reorganizations" (Hall, 1991): firms might reorganize production when demand is low. This reorganization raises firm-level technology, so that even firm-level data do not differentiate the sticky-price versus cleansing alternatives. But this variant of cleansing models generally predicts that when technology improves, investment is also high. The investment may take the form of job search, as in Hall (1991). But we should also observe higher capital investment, as Russell W. Cooper and John C. Haltiwanger (1996) document for the seasonal cycle in the auto industry. Since nonresidential investment falls sharply in the first year following a technology improvement, a flexible-price reorganization model probably cannot explain the results we find.[39]

[37] This idea goes back at least to Josef A. Schumpeter. Lucia Foster et al. (1998) provide empirical evidence on the role of entry and exit in aggregate productivity growth.

[38] There are also other possibilities for explaining estimates of $\gamma < 1$ based on reallocations within an industry. For example, suppose high income-elasticities (leading to high procyclicality of inputs) tend to be associated with high price-elasticities of demand (leading to lower steady-state markups, which in turn lead firms to operate at points on their cost curves with lower γ). Then the cyclicality of input use covaries negatively with returns to scale.

[39] We thank Christopher Foote and Matthew Shapiro for this observation.

VI. Conclusion

We measure aggregate technology by correcting the aggregate Solow residual for the effects of increasing returns, imperfect competition, varying utilization of capital and labor, and aggregation. We find that in the short run, technology improvements significantly reduce input use and nonresidential investment; output changes little. Inputs and nonresidential investment recover sharply and output increases over the next few years.

These results are inconsistent with standard parameterizations of real-business-cycle models, which imply that technology improvements raise input use and nonresidential investment at all horizons. By contrast, we argue that these results *are* qualitatively consistent with the predictions of DGE models with sticky output prices driven by both technology and monetary shocks. Nevertheless, although technology shocks are not the main cause of cyclical fluctuations, neither are they negligible.

Note that our empirical work actually estimates a composite of the partial effect of a technology improvement and the reactions of policy (especially monetary policy) to that technology shock. If the Federal Reserve tries to stabilize inflation, then the true partial effect is even more contractionary than the total effect that we estimate. This point may be especially relevant for estimating the dynamic effects of technology shocks—if the Fed responds in an expansionary way to a fall in inflation and employment, and if some part of Fed policy and its effects operates with a lag of more than one year, it may appear that the economy recovers more quickly from a technology improvement than would be the case without Fed intervention.

We believe that our paper and the structural VAR literature have identified an important stylized fact: technical progress is contractionary in the short run, but has its expected expansionary effect in the long run. We advance price rigidity as the major reason for the perverse short-run effect of technical improvement, as do Galí (1999a) and Galí and Rabanal (2005). (Of course, price rigidity may be due to imperfect information rather than costly price adjustment per se.) The aggregate evidence is broadly consistent with this view, and direct firm-level evidence is provided by Marchetti and Nucci (2005). Nevertheless, it remains possible that other models could be consistent with the evidence as well. Three of the competing explanations are "real inflexibilities" in aggregate demand, sectoral-shifts models, and "cleansing effects" models. We have presented some evidence that these stories do not explain our findings, but additional tests are needed before we can be sure that price inflexibility does explain our results.

Of course, the alternative hypotheses are not mutually exclusive, but could all contain an element of the truth. Indeed, estimated DGE models by Levin et al. (2006), Smets and Wouters (2003), and Galí and Rabanal (2005) suggest that both nominal *and* real rigidities play a role.

If one accepts the view that technology shocks interact with sticky prices, then our results have important implications for monetary policy. First, monetary policy in the United States over the 1949–1996 period did not respond sufficiently to technology shocks to allow actual output to adjust quickly to the new level of full employment output. In this light, the debate in recent years about what is happening to the level of underlying technology—and how monetary policy should react—seems very much on target. Short-run movements in technology growth matter just as much for the proper conduct of monetary policy as the long-run rate of technology growth—if not more—since the main concern of monetary policy is short-run stabilization of the economy around the moving target of full-employment output. To the extent that policymakers can better assess technological movements and respond decisively to them, monetary policy might be improved in the future.

Appendix: Data and Instruments[40]

Industry Dataset

We use updated data described in Jorgenson et al. (1987).[41] (Barbara Fraumeni, Mun Ho, and Kevin Stiroh were major contributors to various vintages of the data.)

We merged the main dataset, which runs from 1958 to 1996, with an earlier vintage of the dataset that runs from 1948 to 1989. We

[40] An on-line Appendix provides more discussion of data and econometric issues.

[41] Downloaded from http://post.economics.harvard.edu/faculty/jorgenson/data/35klem.html (Oct. 2002).

used growth rates from 1949 to 1958 from the older dataset and growth rates from 1959 to 1996 from the newer dataset. Growth rates for the post-1959 overlap period generally line up closely, particularly in the early years, so there are not major inconsistencies between the two data series around the merge point. In addition, qualitative results are robust to using the two datasets separately.

We generally construct indices and aggregates as Tornquist indices. To construct industry input aggregates, however, we use factor shares averaged over the entire sample period, out of concern that observed factor payments might not be allocative period by period, for example, because of implicit contracts. Hence, we take an explicit first-order approximation to the industry production function. We assume that industries earn zero economic profits, so that factor shares sum to one.

Hours per Worker

Where available, we used BLS data on hours/worker for production workers. Where necessary, particularly in early years of the sample, we used supplemental employment and hours data provided by Dale Jorgenson and Kevin Stiroh to construct a long time series for each industry. We then detrended log hours-per-worker using Christiano and Terry J. Fitzgerald's (2003) band pass filter, isolating frequency components between two and eight years. By detrending, our utilization series has zero mean and no trend. We then took the first-difference in this detrended series as our measure of hours-per-worker growth *dh*.

National-Accounts and Exchange-Rate Data

Data from the Bureau of Economic Analysis and Federal Reserve Board were downloaded from the Haver Analytics database on April 7, 2004.

Instruments

Monetary Shocks.—We use the sum for the preceding calendar year of quarterly VAR monetary innovations, following Christiano et al. (1999), Burnside (1996), and others. Following Burnside (1996), we measure monetary policy as innovations to the three-month Treasury Bill rate from a VAR with GDP, the GDP deflator, an index of commodity prices, the three-month T-Bill rate, and M1.[42]

Government Spending.—We sum the quarterly growth rates of real government defense spending from the preceding year, i.e., from the fourth quarter of $t - 2$ to the fourth quarter of $t - 1$, as the instrument for annual input growth from year $t - 1$ to year t.

Petroleum Prices.—Following Knut Mork (1989), we use the "composite" refiner acquisition price (RAP) for crude oil, a series produced by the Department of Energy. The composite price is refiners' average purchase price of crude oil, i.e., the appropriate weighted average of the domestic and foreign prices per barrel. Conceptually, the major difference between RAP and the PPI for crude petroleum arises from the Nixon price controls imposed in the second half of 1971.[43] RAP is available annually from 1968 and monthly from January 1974 on. We follow Mork (1989) in linking the PPI and the annual composite RAP to create an estimated quarterly refiner acquisition price prior to 1974. Following James D. Hamilton (1996), we measure the quarterly oil price "shock" as the difference between the log of the quarterly real oil price and the maximum oil price in the preceding four quarters. (In all cases, we measure the quarterly oil price using the last month of the quarter.) We then use the sum of the quarterly shocks in the preceding calendar year as our instrument.

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[42] We thank Charles Evans for providing RATS code that estimated the VAR and innovations.

[43] Mark French and Robert Vigfusson independently pointed out to us the problems with the producer price index.

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