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ABSTRACT

Yes. We construct a measure of aggregate technology change, controlling for varying utilization of capital and labor, non-constant returns and imperfect competition, and aggregation effects. On impact, when technology improves, input use and non-residential investment fall sharply. Output changes little. With a lag of several years, inputs and investment return to normal and output rises strongly. We discuss what models could be consistent with this evidence. For example, standard one-sector real-business-cycle models are not, since they generally predict that technology improvements are expansionary, with inputs and (especially) output rising immediately. However, the evidence is consistent with simple sticky-price models, which predict the results we find: When technology improves, input use and investment demand generally fall in the short run, and output itself may also fall.

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When technology improves, does employment of capital and labor rise in the short run? Although standard frictionless real-business-cycle models generally predict that they do, other macroeconomic models predict the opposite. For example, sticky-price models generally 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. Sticky-price 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 empirical merits of different business-cycle models.

Measuring these correlations requires an appropriate measure of aggregate technology. We construct such a series by controlling for non-technological effects in aggregate total factor productivity (TFP), i.e., the aggregate Solow residual: varying utilization of capital and labor, non-constant returns and imperfect competition, and aggregation effects.[1] "Purified" technology varies about half as much as TFP. In addition, technology fluctuations are countercyclical, in that contemporaneously, they are significantly negatively correlated with inputs. Contemporaneously, they are uncorrelated with output.

We then explore the dynamic response of the economy to technology. Technology shocks appear permanent and do not appear serially correlated. Technology improvements reduce 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. Non-residential investment falls sharply on impact before rising above its steady-state level. Household spending (especially durable consumption and residential investment) rises on impact and rises further with a lag. Thus, after a year or so, 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 for 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 observed TFP and the business cycle. Demand shocks can explain

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[1] Unless we specifically state otherwise, we use "technology" and "TFP" to refer to growth in these variables. We note that Solow's (1957) original article suggests modifications and extensions (e.g., for factor utilization and monopoly power) necessary for his residual to properly measure technology at business cycle frequencies; he also notes the issue

why, instead, observed TFP is procyclical. When demand increases, output and inputs—including unobserved utilization—increase as well. We find that shocks other than technology are much more important at cyclical frequencies, so changes in utilization make observed TFP procyclical.

We identify technology using the tools of Basu and Fernald (1997) and Basu and Kimball (1997), who in turn build on Solow (1957) and Hall (1990). Basu and Fernald stress the role of sectoral heterogeneity and aggregation. They argue that for economically plausible reasons—e.g., differences across industries in the degrees of market power—the marginal product of an input may differ across uses. Growth in the aggregate Solow residual 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 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.

Galí (1999) independently proposes a quite different method to investigate similar issues. Following Blanchard and Quah (1989), 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 (1999) specification.[2] Francis and Ramey (2003a) 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." But several papers critique Galí's empirical implementation. For example, Christiano, Eichenbaum, and Vigfusson (2003) and Altig, Christiano, Eichenbaum, and Linde (2002) [henceforth CEV and ACEL] endorse the basic long-run identification strategy, but argue for using per-capita hours in log-levels rather than in growth rates. With this subtle change in specification, these two papers conclude that technology improvements *raise* hours

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of aggregation. Basu and Fernald (2001) discuss additional references on technology and TFP.

[2] See Galí and Rabanal (2004) for a recent summary.

worked on impact.[3] Thus, although the SVAR evidence mostly suggests that technology improvements reduce hours, the evidence from this approach is not yet completely conclusive.

Our alternative augmented-growth-accounting approach yields potentially important evidence that relies on completely different assumptions for identification. In addition, our approach offers at least three 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 taxes, and some models of endogenous growth would all imply that non-technology shocks can change long-run labor productivity; allowing these effects might change the estimated impulse responses.[4] Our production-function approach allows these deviations. Second, 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, Faust and Leeper, 1997).[5] Our production-function approach, by contrast, does not rely on these same identification conditions. Third, we can look at the effect of technology shocks on many variables without having to modify our basic identification strategy. In contrast, results from an identified VAR might look very different as more variables are added.

Nevertheless, the SVAR and augmented-growth-accounting approaches are best regarded as complements, with distinct identification schemes and strengths. In addition, two additional approaches also suggest that technology improvements reduce input use. First, estimated structural DGE models (such as Smets and Wouters, 2003, for the euro area and Galí and Rabanal 2004 for the U.S.) tend to find that technology improvements reduce input use on impact. Second, Shea (1998) measures technology as innovations to R&D spending and patent activity and finds that with a lag of several years *process*

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[3] However, Fernald (2004) argues that CEV's specification actually yields *stronger* results than Galí's that technology improvements reduce hours worked once one controls for the early 1970s productivity slowdown and mid-1990s re-acceleration in productivity. (This result arises because long run identification schemes can push low frequency correlations into the estimated impact effects.) Thus, correctly interpreted, CEV's arguments for the levels specification raises our confidence that SVARs imply that technology improvements reduce hours. (CEV and ACEL make several important methodological contributions to the literature, which we view as more substantive than their (fragile) empirical results.) We discuss several related points, including another recent paper by CEV (2004), in Section IV.

[4] In the SVAR context, Uhlig (2004) discusses capital taxes and time-varying attitudes towards leisure in the workplace; Sarte (1997) discusses sensitivity to replacing "zero" long-run effect of demand shocks with other reasonable magnitudes (e.g., coming from hysteresis in labor quality). Barlevy (2003) provides a nice model in which cyclical fluctuations have a substantial effect on long-run growth.

[5] Cooley and Dwyer (1998) and Erceg, Guerrieri, and Gust (2004) estimate SVARs on data from calibrated DGE models and suggest that these concerns could, in some cases, be important in practice. Nevertheless, Fernald (2004) and

innovations increase TFP and simultaneously lower labor input.[6]

Thus, despite differing data, countries, and methods, the bottom line is that the state-of-the-art versions of four very different approaches give similar results. It thus appears we have uncovered a robust stylized fact: technology improvements are contractionary on impact. Given this robustness, we view this as a 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 King and Rebelo's (1999) attempt to "resuscitate" these models. The negative effect of a technology improvement on non-residential investment is particularly hard to reconcile with flexible-price RBC models (including the models suggested by Francis and Ramey, 2003a), given our finding that the full impact of a technology improvement on productivity comes almost immediately. However, our findings are consistent with the predictions of dynamic general-equilibrium models with sticky prices. Consider the simple case where the quantity theory governs the demand for money, so 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 less labor and capital 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 real-business-cycle 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.

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 deflation, which in turn gives the monetary authority room to lower interest rates. In Section V, we argue that technology improvements are still likely to be contractionary, reflecting the fact that central banks react with a lag. Indeed, the experience of observing monetary policy in the United States in the 1990s suggests that central banks observe technology shocks only with a long lag.

Erceg *et al* do conclude that, when interpreted with suitable caution, SVAR results might be informative.

[6] Gali (1998) draws out and discusses this implication of Shea's findings.

[7] Tobin (1955) makes this point in a model with an exogenously fixed nominal wage. Additional issues arise when considering household investment as well as business investment. Investment that adds to capital used in production—with flow benefit equal to a rental rate or marginal product (which depends on the capital/labor ratio and the cost of other inputs)—is distinct from residential investment and consumer durables, for which the rental rate depends largely on income effects and the overall stock of housing and consumer durables.

Clearly, our results are not a "test" of sticky-price models of business cycles, even though the results are consistent with that interpretation. We favor this interpretation in part one wants a model that appropriately captures the economy's response to both monetary and technology shocks, and sticky-price models can generate large monetary non-neutralities. Nevertheless, other explanations are possible, including 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.

Some recent direct evidence does suggest that sticky prices are indeed responsible for our finding. Marchetti and Nucci (2004) apply exactly our identification method to Italian firm-level data and find, like us, 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 the firms that have sticky prices. As well as confirming the negative correlation between technology and hours that we document here, their finding is strong, direct evidence in favor of our preferred interpretation of this fact.

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) industry Hall-style regression equations with a proxy for utilization. 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 I.B. discusses how we control for utilization.

A. Firm and Aggregate Technology

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

$$Y_i = F^i\left(A_i K_i, E_i H_i N_i, M_i, Z_i\right). \tag{1.1}$$

The firm 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. However, firms 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 (i.e., capital's workweek). Total labor input, L_i, is the product $E_iH_iN_i$. The firm's production function F^i is (locally) homogeneous of arbitrary degree γ_i in total inputs. If γ_i exceeds one, then the firm has 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.[8] 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:

$$dy_i = \gamma_i (dx_i + du_i) + dz_i \,, \tag{1.2}$$

where

$$dx_i = s_{Ki} dk_i + s_{Li}(dn_i + dh_i) + s_{Mi} dm_i \ , \tag{1.3}$$

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

and s_{Ji} is the ratio of payments to input J in total cost. Section *I.B* explores ways to measure du_i.

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

$$dz = \sum_i \left(\frac{w_i}{1 - s_{Mi}} \right) dz_i \tag{1.4}$$

where w_i equals $\left(P_iY_i - P_{Mi}M_i\right) \Big/ \sum_i \left(P_iY_i - P_{Mi}M_i\right) \equiv P_i^V V_i \big/ P^V V$, the firm'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 firm's share of aggregate value added. Basu and Fernald (2001, Section III) discuss the interpretation of aggregate technical change as defined in (1.4).[9]

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

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[8] Basu and Fernald (2001) provide detailed derivations and discussion of the equations in this (and the subsequent) subsection. Although we have not noted it explicitly, the detailed derivation allows the firm to potentially charge a markup of price over marginal cost (it must, with increasing returns, to cover its costs). Hence, the resulting estimating equation controls for imperfect competition as well as increasing returns.

[9] This weighting scheme follows 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 $\gamma s_M \to 1$, $\left[1/(1 - \gamma s_M)\right] \to \infty$. This means that positive and negative estimation error does not cancel out; and, indeed, estimation error can have an enormous impact on measured aggregate technology. Domar-weighted residuals are thus more robust to mismeasurement.

$$du = \sum_i \left(\frac{w_i}{1 - s_{Mi}} \right) \gamma_i du_i \tag{1.5}$$

From equation (1.2), $\gamma_i du_i$ enters in a manner parallel to dz_i, so that (1.6) parallels (1.4). (Note that standard aggregate TFP (growth), aka the Solow residual, is the special case where all industries have constant returns, unobserved utilization changes are zero, and factor prices are equal across industries.)

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

B. Measuring Firm-Level Capacity Utilization

Utilization growth, du_i, is a weighted average 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. As in Basu and Kimball (1997), we derive such a relationship from the firm's first-order conditions. 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 the firm's 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.[10]

We assume that firms can freely vary *H, A,* and *E* 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

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[10] Aggregate models (e.g., Burnside and Eichenbaum, 1996), can model variable utilization without *internal* adjustment costs, since the representative firm's input demand affects the real wage and interest rate. But modeling how industries vary utilization in response to idiosyncratic changes in technology or demand requires internal adjustment costs to yield a coherent model of variable factor utilization. (Haavelmo's (1960) treatment of investment makes these observations.)

discrete variations in capital's workday (the number of shifts) are an important mechanism for varying utilization.[11] 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, e.g., if an implicit contract governs wage payments in a long-term relationship.

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

$$\underset{A,E,H,M,I,R}{\text{Min}} \quad E_t \sum_{s=t}^{\infty} \left[\prod_{j=t}^{s} \left(1+r_j\right)^{-1} \right] \left[WG\left(H,E\right)V\left(A\right)N + P_M M + WN\Psi\left(D/N\right) + P_I K\, J\left(I/K\right) \right] \quad (1.6)$$

subject to

$$\overline{Y} = F(AK, EHN, M, Z) \quad (1.7)$$

$$K_{t+1} = I_t + (1-\delta)K_t \quad (1.8)$$

$$N_{t+1} = N_t + D_t \quad (1.9)$$

In each period, the firm's costs in (1.6) are total payments for labor and materials, 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; δ is the rate of depreciation. We assume that Ψ and J are convex.[12] We omit time subscripts.

There are six intra-temporal first-order conditions and two Euler equations, for the state variables K and N. To conserve space, we analyze only the optimization conditions that affect our derivation; Basu and Kimball (1997) discuss further details. λ, the multiplier on constraint (1.7), has the interpretation of marginal cost. F_s, $s = 1, 2, 3$ denotes derivatives of the production function with respect to argument s, and literal subscripts denote derivatives of the labor cost function G. We require the first order conditions for A, H, and E:

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[11] Beaulieu and Mattey (1998) and Shapiro (1996), for example, apply the variable-shifts model to manufacturing data.
[12] 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 optimal H and E move together. We make some normalizations relative to normal or "steady state" levels. Let $J(\delta) = \delta$, $J'(\delta) = 1$, $\Psi(0) = 0$. We also assume that the

A: $$\lambda F_1 K = wNG(H,E)V'(A) \tag{1.10}$$

H: $$\lambda F_2 EN = wNG_H(H,E)V(A) \tag{1.11}$$

E: $$\lambda F_2 HN = wNG_E(H,E)V(A) \tag{1.12}$$

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 (1.11) and (1.12) can be combined into an equation implicitly relating E and H:

$$\frac{HG_H(H,E)}{G(H,E)} = \frac{EG_E(H,E)}{G(H,E)}. \tag{1.13}$$

The elasticity 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, (1.13) 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:

$$de = \zeta\, dh. \tag{1.14}$$

To find a proxy for capital utilization, we combine (1.10) and (1.11). Rearranging, we find:

$$\frac{F_1 AK/F}{F_2 EHN/F} = \left[\frac{G(H,E)}{HG_H(H,E)}\right]\left[\frac{AV'(A)}{V(A)}\right] \tag{1.15}$$

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 *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 (1.15) as:

$$v(A) = (\alpha_K/\alpha_L)g(H). \tag{1.16}$$

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marginal employment adjustment cost is zero at a constant level of employment: $\Psi'(0) = 0$.

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 in *K* and *L*.[13] Under these assumption, the log-linearization of (1.16) is simply

$$da = (\eta/\omega)\,dh\,. \tag{1.17}$$

Thus, equations (1.17) and (1.14) say that the change in hours per worker should be a proxy for changes in *both* unobservable labor effort and the unmeasured workweek of capital. The reason that hours per worker proxies for capital utilization as well as labor effort is that 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 is the time when hours per worker are also high.

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

$$\begin{aligned} dy &= \gamma\,dx + \gamma\left(\zeta s_L + \frac{\eta}{\omega}s_K\right)dh + \; dz \\ &= \gamma\,dx + \beta dh + \; dz. \end{aligned} \tag{1.18}$$

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.[14,15]

In sum, we estimate equation (1.18) on disaggregated data, which controls for non-constant returns, imperfect competition, and utilization. We measure industry technology as the residuals *dz*. We aggregate as in (1.5) to get a measure of aggregate technology that controls for possible aggregation/reallocation effects.

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

[14] 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.

[15] An alternative approach assumes, more restrictively, fixed proportions between an observed and unobserved input. For example, Burnside et al. (1995, 1996) follow Jorgenson and Griliches (1967) and 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.

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 non-farm, non-mining U.S. economy. We use industry data from Dale Jorgenson and collaborators from 1949-1996. The data comprise 29 industries (including 21 manufacturing industries at roughly the two-digit level) that cover the entire non-farm, non-mining private economy. These sectoral accounts include industry gross output and inputs of capital, labor, energy, and materials. Appendix I describes the data and our calculations in more detail.

Given the potential correlation between input growth and technology shocks in equation (1.18), we use instruments uncorrelated with technology change. As described in the data appendix, 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. In all cases, we have quarterly instruments; we sum the four year *t-1* quarterly shocks as instruments for year *t*.[16]

B. Estimating Technology Change

We estimate industry-level technology change from the 29 regression residuals from (1.18), estimated as a system of equations.[17] To conserve parameters, we restrict the utilization coefficient within three groups: durables manufacturing (11 industries, listed in Table 1); non-durables manufacturing (10); and non-manufacturing (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

$$dy_i = c_i + \gamma_i dx_i + \beta dh_i + dz_i \,. \tag{2.1}$$

This parsimonious equation controls for both capital and labor utilization. Note that hours-per-worker growth *dh* essentially enters twice, since it's 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). Once estimated, aggregate 'purified' technology change is then the weighted sum of the industry residuals plus constant terms.

Given the mid-sample productivity slowdown, we tested for a break in the industry constants. We

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[16] The qualitative features of the results in Section III are robust to different combinations and lags of the instruments. Section IV discusses the small sample properties of instrumental variables.

[17] Estimation was via GMM in TSP, with NMA=2 (results are not particularly sensitive to this parameter).

imposed that any break was common to all industries within each group. Following Andrews (1993), we considered break dates between the 15th to the 85th percentile of the sample (1955-1988) for six series: TFP growth for durable manufacturing, non-durable manufacturing, and non-manufacturing; and the corresponding (aggregated) technology residuals for these groups, estimated without a break. Only in non-manufacturing do we reject the null of no break.[18] In durable manufacturing, technology appears to accelerate using conventional critical values but not ones (such as Andrews') that adjust for pre-testing. In non-durable manufacturing, the technology slowdown is insignificant even at conventional levels.

The data do not speak strongly on whether the non-manufacturing break occurred in 1967 or 1973 and results are little affected by this choice. Results below impose a 1973 break. Allowing separate trend breaks for each industry yields results similar to those reported. The breaks are part of the constant terms which we incorporate into technology. Focusing on residuals alone, however, has little effect on the results we report.

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 (2.1). For durable manufacturing, the median returns-to-scale estimate is 1.07; for non-durable manufacturing, 0.89; for non-manufacturing, 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

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[18] For non-manufacturing TFP, the maximum F statistic was about 16 for a break in 1967 or 1973 (slightly higher 1973). For estimated technology, the maximum F statistic was well above 20 in both 1967 and 1973 (slightly higher 1967). These F statistics exceed the 1-percent critical value of 12.35 from Andrews (1993) for $p=1$ and $\pi_0= 0.15$. Boostrapped critical values are similar. For each of durable manufacturing, non-durable manufacturing, and non-manufacturing, we created 10,000 artificial bootstrapped datasets for both TFP and purified technology. On each artificial dataset, we tested every possible break date (1955 to 1988) and recorded the maximum F statistic. The highest 1-percent critical value across the various productivity or technology series was 13.65 (for non-manufacturing technology).

wide variation across industries.[19] (Throwing out 'outlier' industries (e.g., 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 non-durables manufacturing. The coefficient is significant at the 10-percent level in non-manufacturing.

Table 2 summarizes means and standard deviations for TFP (the Solow residual) and "purified" technology. TFP does not adjust for utilization or non-constant returns. Purified technology controls for utilization and non-constant returns, aggregated as in equation (1.5).

For the entire private non-mining economy, the standard deviation of technology, 1.5 percent per year, compares with the 2 percent standard deviation of TFP; indeed, the variance is only 55 percent as high. For both durable and non-durable manufacturing, the standard deviation of purified technology is, perhaps surprisingly, higher than for TFP. The reduction in variance in column one 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 non-residential investment. The top panel plots TFP and technology. Technology fluctuates much less than TFP, consistent with varying input utilization and other non-technological 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 clear contemporaneous comovement between the two series. Particularly in the first half of the sample, the series has the same phase shift as does 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.[20] The fourth panel shows that estimated factor

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[19] Basu and Fernald (2001) discuss the apparent decreasing returns in non-durables manufacturing.

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

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 (with a much wider scale than the others) shows a second central result: non-residential investment often falls when technology improves. Conversely, when technology falls (growth below its mean), investment often rises. (The large investment swings, though, are most likely unrelated to purified technology.)

As expected, the utilization correction explains most of the reduction in pro-cyclicality. 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.[21]

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 appears to have a unit root. With an augmented 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 stationary (with or without a trend); the p-value is less than 0.01. In addition, technology growth shows little evidence of autocorrelation (e.g., the smallest p-value from the Ljung-Box Q test is 0.25, when testing for 3rd-order 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 effects appear small. Thus, in what follows, we assume technology change is a random walk. That said, reported results are robust to using autoregressive residuals

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.)[22] Purified technology is a generated regressor, so correct standard errors must account for the

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[21] As Basu and Fernald (1997) discuss, one reallocation effect comes from the difference in returns to scale between durable and non-durable 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.

[22] Conceptually, we interpret our technology shocks as fundamental shocks to a vector-moving-average representation of each series. Assuming these shocks are orthogonal to other fundamental shocks (an assumption that we do not impose 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 suggest two lags, at most (this remains true in the VARs, below, which add lags of the variable itself). The regressions show more lags for completeness, since adding them has little impact on the dynamics at zero- to two-lags.

estimation error involved in estimating technology from the 'first step' parameter estimates in Table 1 and the underlying industry data. As is typical with generated regressors, the correction depends on the true coefficient on technology as well as the first-step estimation error (we derive this formally in the appendix). In particular, if the true coefficient is zero, then the usual standard error calculation is correct. The standard errors in Table 3 are correct under the null that the true coefficient is zero.

More subtly, however, we want to test the *sign* of the impact-effect coefficient—e.g., can we reject that hours rise when technology rises? This hypothesis requires us to reject not only the null hypothesis that the true coefficient is zero, but that the true coefficient is positive. In principle, with sufficiently large "first-step" estimation error, it might happen that we could reject that the true coefficient is zero but *not* reject that the true coefficient is some positive number. Fortunately, in Appendix II we derive a simple test statistic that allows us to reject this possibility. Hence, if we can reject the null hypothesis of zero, then 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. Line (7) shows the second key point of this paper: on impact, non-residential investment falls very sharply; with a lag of one and two years, non-residential investment rises sharply. Thus, the response of investment looks qualitatively similar to the response of total hours worked.

In contrast, residential investment plus consumer durables purchases *rise* strongly on impact, then rise further with a lag. The different response of business and household investment is not surprising. Non-residential 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 non-durables 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 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 could explain the initial muted response.

The final two rows show the response of inventories and net exports; in both cases, we deal with the possibility of negative values by scaling by GDP. The inventory/GDP ratio falls significantly; net exports/GDP rises, but insignificantly. These are interesting because firms could potentially use these margins to smooth production, even if they don't plan to sell more output today. The decline in inventories could reflect uncertainty about which specific products will be demanded in the future (e.g., if there is idiosyncratic demand for particular products) so that firms don't want to smooth production.

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 on two lags of itself and dz. We derive impulses responses (representing the 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 1-1/2 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; non-residential investment appears only slightly changed from pre-shock levels but the level of household spending rises.

C. Dynamics of Prices and Interest Rates

Figure 4 shows VAR impulse responses of a range of price and interest rate series. (The regressions corresponding to Table 3 yield qualitatively similar results). The top row shows deflators for non-farm business and several economically sensible aggregates: the combination of (residential and non-residential) investment and consumer durables; and consumption of nondurables and services.[23] 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 (non-durables and services), we find very little. (The point estimate suggests that the investment deflator rises slightly but not significantly.) A growing literature focuses on "investment specific" technical change (e.g., Greenwood, Hercowitz, and Krusell, 1997, and Fisher 2003). Since we use chain-linked data, our technology series, in principle, incorporates both "neutral" and "investment specific" technology change. That we don't find a change in the relative price of investment suggests that technical change, on average, is largely neutral.

The remaining responses on the second row show that the nominal fed funds rate and nominal 3-month 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 fed funds rate than for the 3-month Treasury rate, reflecting a narrowing of the spread between the two.)

For completeness, we also include real and nominal values of the exchange rate and wage. We use the

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[23] 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.

growth rate of the Federal Reserve Board's broad trade-weighted exchange rate series (this series is available only since 1973). The exchange rate appears to depreciate very sharply when technology improves. (A word of caution: the sharp appreciation of 1980-85 and depreciation of 1985-88 dominate the data. Adding separate dummies for those two periods reduces both the magnitude and statistical significance of the estimate, which does remain negative.) 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 we are uncertain about the extent to which observed wages are 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. Appendices III and IV discuss econometric issues of input measurement error and small-sample-properties of instrumental variables. Our basic finding that input use covaries negatively with technology survives.

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 non-residential investment response. The 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 from only 1959-1996. 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 (i) allowing for serial correlation of technology in the VAR, i.e., adding lags of technology growth to the technology equation, and (ii) allowing for serial correlation *and* (lagged) feedback of shocks to hours on technology (i.e., putting hours growth or investment growth into the technology equation). In the top panel, the final two dashed lines use BLS nonfarm business hours worked per capita (aged 16+) rather than Jorgenson's hours growth, since the SVAR literature focuses on BLS data (and, indeed, focus 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 non-farm responses.) It is clear that in these "short-run" specifications, the distinction between levels and differences is relatively inconsequential.

The bottom line is that the impact effect is very similar in all cases. They uniformly show that hours and non-residential 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.

CEV (2004) suggest that the *level* of hours per capita Granger-causes the technology series from an earlier version of this paper. Neither in levels nor in growth rates are Jorgenson's or the BLS non-farm business hours series even remotely significant; e.g., the p-value on two lags of the (log) level of non-farm business hours per capita (aged 16+) is 0.35. CEV use private business hours rather than non-farm business hours; the p-value of 0.11 is still insignificant, although it's much closer.

CEV might perhaps argue that farm hours Granger-causes our technology series,[24] but Fernald (2004) argues 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 afterwards): Estimated 1951-96 with two lags, the p-value is 0.02. Since much of the decline in private business hours reflects movements away from farms, non-farm 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 trend break (and industry constant terms), the p-value for BLS private business hours rises to 0.39.[25] Quite clearly, CEV's Granger causality evidence 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

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[24] Hours worked by farmers is poorly estimated relative to the number of employees, which is a reason to prefer non-farm 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 2 percent. Farm employees proxy nicely for the productivity slowdown, since they fall by more than half from 1949 to 1971 but remain fairly level thereafter. This example points out a limitation of Granger causality tests in this context.

[25] With this series, the p-value for whether farm employees Granger-cause purified technology rises to 0.54.

correlated contemporaneously with true technology. But suppose, for example, that a positive 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.[26]

B. Long-Run Restrictions

A growing recent literature estimates structural VARs with the long-run identifying restriction that only technology shocks affect labor productivity in the long run (see, especially, Galí 1999; Francis and Ramey 2003a,b; Christiano, Eichenbaum, and Vigfusson 2003, 2004; and Galí and Rabanal 2004). CEV (2004) suggest replacing labor productivity with our "purified" technology series; they are concerned that there could be high frequency cyclical measurement error that the long-run restriction might clean out.[27] As in that literature, we focus on the impulse 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.

Suppose *prod* is the log of the productivity measure to which one is applies the long-run restriction (e.g., labor productivity or the level of purified technology). Suppose *hrs* is some function of the log of hours worked; the extant literature mainly focuses on the log-level or the growth rate of hours per capita, but other specifications use log hours-per-capita detrended in some way or else use the log-level or difference in actual hours (not per capita). (In larger systems, we can generalize *hrs* to be a vector of variables that are included in the VAR, including some function of hours; we don't consider such systems here). Shapiro and Watson (1988) show that one can estimate "true" technology residuals as the residuals from the following regression:

$$\Delta prod_t = c + A(L)\Delta prod_{t-1} + B(L)\Delta hrs_t + \varepsilon_t^Z$$

A(L) and *B(L)* are polynomials in the lag operator. Note that hrs_t enters the regression in first differences,

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[26] In Beaudry and Portier (2000), current behavior reflects (imperfectly) anticipated future changes in technology. Hence, current variables could in principle Granger-cause even completely exogenous future technology.

[27] CEV cite "countercyclical markups" which, in our setup, presumably translates into countercyclical returns to scale. However, this effect would not 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's (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.

which turns out to be a simple way to impose the restriction that non-technological shocks do not affect the level of labor productivity in the long run. Since technology shocks might well affect the current growth rate of hours worked (or other variables included in *hrs*), we follow Shapiro and Watson and estimate this regression with instrumental variables (a constant and $\Delta prod_{t-s}$, and the levels of hrs_{t-s+1}). [28]

Following CEV (2004), we estimate bivariate VARs with two lags, defining *prod* as 'purified' technology. We identify "true" long-run technology shocks as the estimated VAR shock that affects the long-run level of our purified technology series. We use Jorgenson's hours series per capita (16+) in both log-levels and 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. In particular, both specifications show strong evidence that technology improvements reduce hours worked; hours then recover with a lag. (The difference specification is statistically significant at only about the 10 percent level, but the point estimate is quite similar to the results from short-run identification.)

The resulting technology series has a correlation of 0.82 (levels specification) to 0.97 (difference specification) with our original purified technology series. Estimating the SVAR with a 1973 trend-break in productivity brings both correlations to about 0.9. When we define *prod* as aggregate labor productivity (following Galí, 1999), 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 non-farm business labor productivity and hours per capita, the correlations between 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 (2004) and Erceg, Gust, and Guerrieri (2004), one need 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

Table 5 confirms that results do not arise from aggregation or from a small number of industries. We correlate industry TFP and purified technology with industry gross output and hours for nine (approximately

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[28] We estimate impulse responses by putting the estimated technology shock into a second hours equation, in which *hrs* is regressed on lags of *hrs* and *prod* as well as the identified technology shock. The impulse response is then derived via simulation. See CEV (2003) for a clear exposition.

one-digit) industries. We also show median correlations for all 29 industries. For all 29 industries, the median correlation of industry inputs with standard TFP (Corr(*dp, dx*)) is 0.15; the 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.

We also correlated industry residuals with SVAR-identified shocks (identified as in Part B, using growth rates of industry labor productivity and hours). The median correlation of industry SVAR and purified technology is 0.71; 27 of the 29 correlations are statistically significant at the 95 percent level. For 22 industries, the impact effect of an SVAR-identified technology shock on industry hours is negative.

V. Interpretations of the Results

A. The Standard RBC Model

The data show that technology improvements reduce hours and non-residential investment. By contrast, the standard RBC model (e.g., Cooley and Prescott 1995) predicts that improved technology should have raised 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.[29] 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 non-residential investment most strongly contradicts basic RBC theory. In standard calibrations, a permanent technology improvement increases consumption and investment together.[30] Residential investment and consumer durables display the expected pattern, but business investment does not. Business investment grows strongly in the second year after technology improves. Again, this overshooting pattern is not characteristic of standard RBC models.[31]

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[29] As in 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; i.e., both income and substitution effects tend to push towards lower current labor supply. However, our technology process is not autocorrelated.

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

[31] 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, they 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

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 quantitatively close to the prediction of a balanced growth model: A one 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) match 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. However, are those models right in assuming that technology shocks are the dominant source of short-run volatility of output and inputs? Table 4 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 17 percent 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 18 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 of a model match those that we and others find.

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

Francis and Ramey (2003a) propose a variant of the standard RBC model with inertial consumption and investment (coming from habit formation and standard q-theory adjustment costs). Hence, domestic demand changes little when technology improves, so hours worked fall. As Galí and Rabanal (2004) note, this model is particularly interesting because many business cycle models, with and without nominal rigidities, assume this kind of real inertia in demand.

The slow rise of non-durables consumption is broadly consistent with the Francis and Ramey (2003a)

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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.

model, but the response of investment is not. The fall in non-residential investment followed by a large rise a year later is no more consistent with their model than with the standard RBC model. 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. These large jumps are not what one would predict from a model where investment adjustment costs are highly convex.

C. Price Stickiness

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 output constant, the sum of the other components of output, such as consumer durables, residential investment or non-durables and services would have 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 dynamic general-equilibrium model with sticky prices, such as Kimball's (1998) Neomonetarist model. Kimball (1998) finds that the decline in investment in plant and equipment induced by a technological improvement can even cause output to decline. Two effects work to reduce investment. First, as noted above, 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 goods, then firms prefer to hold bonds instead of investing in plant and equipment on which they will take substantial capital losses. Price declines follow just 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.

Basu (1998) and Basu and Kimball (2004) calibrate DGE models with staggered price setting, and reproduce accurately the impact effect of technology improvements that we find in the data.

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. Would it accommodate technology improvements by loosening policy, thereby avoiding the initial contraction? Basu (1998) allows the monetary authority to follow a Taylor rule, setting the nominal interest rate in response to lagged inflation and the lagged "output gap"—the deviation between current and full-employment output. He still finds that on impact, output barely changes when technology improves, while inputs fall sharply. Monetary policy is insufficiently loose under a Taylor rule in part because the Federal Reserve reacts only with a lag—that is, after the shock affects inflation or the measured output gap.[32]

Why do residential investment and consumer durables purchases rise sharply when technology improves, 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; household demand for the services of consumer durables and housing, however, depends primarily on permanent income, which rises. On the cost side, residential housing purchases appear to be more sensitive to interest rates than corporate investment. The fed 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. Also, construction prices respond more quickly than the prices of investment goods in general. Barsky, House, and Kimball (2004) present evidence suggesting that housing prices are relatively flexible, and they may complete much of their adjustment to the shock within the first year.

The previous discussion suggests that when technology improves, the Fed does indeed respond to the lower inflation (and perhaps to its perception of the output gap) by lowering the real fed funds rate. Still, it is certainly possible that the Fed reacts too little. First, it is difficult to recognize in real time that technology has improved. Even if the Fed perceives that inflation has fallen for one or two quarters, it may not know whether this fall is a transient blip or is more persistent. Second, estimates of the Fed's policy rule suggest the Fed smoothes interest rates (e.g., Clarida, Galí, and Gertler, 1999; Gerlach-Kristen, 2004). Interest rate smoothing by definition slows down the Fed's response to shocks.

The best evidence that the Fed does not take sufficient action to offset the contractionary effects of a technology improvement lies in the behavior of the price level. Since technology shocks change full-employment output, they do not present the monetary authority with a tradeoff between output and inflation stabilization.[33] Thus, optimal monetary policy would ensure that the technology shock has no effect on inflation at any horizon, and thus leaves the price level unchanged. 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

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[32] In Basu's model, the contraction is relatively short-lived, unlike the responses we find. But Basu's model incorporates few "real rigidities." Kimball (1995) shows that one can obtain a "contract multiplier" of any desired size by adding real rigidities to the model, making price adjustment arbitrarily slow.

[33] See, e.g,. Woodford, 2003, pp. 461-462, who in turn cites Khan, King and Wolman, 2002, on this point.

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.)

Galí, López -Salido, and Vallés (2002) suggest that the contractionary effects on inputs is less pronounced under the Volcker-Greenspan Fed than previously, and advance the hypothesis that monetary policy was better at accommodating 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 (we use fewer lags than in Table 3 to conserve degrees of freedom). In the 1949-1979 period, output appears to decline on impact (not significantly), and hours fall sharply and significantly. In the post-1979 period, output actually rises somewhat on impact; for hours, the point estimate suggests the impact effect is still negative, but the magnitude is much smaller and the decline is only marginally significant. The magnitude of the impact decline in non-residential investment is, however, even larger in the later subperiod. Interestingly, there is virtually no difference in the impact effect on inflation despite the fact that the fed funds rate falls much more sharply in the post-1979 period. (In the latter period, the decline in inflation is somewhat less sharp with a lag of one and two years, consistent with the larger and more persistent decline in the fed funds rate.)

Formal statistical tests for subsample differences, however, do not reject the hypothesis that the responses of the real variables to technology shocks are the same in the two sub-periods. This is true for both the impact effect and the cumulative effect of technology. The cumulative effect on the price level, although 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 sub-periods. Hence, our results provide, at best, only weak support for the hypothesis advanced by Galí, López -Salido, and Vallés (2002).

Finally, as evidence for the role of sticky prices in the short-run effects of technology, Marchetti and Nucci (2004) apply the basic methods here to a panel of Italian firm-level data. Not only do they confirm our finding that technology improvements are contractionary, but they find that the observation is driven by the behavior of firms whose prices are rigid for a year or more. The flexible-price firms do not reduce hours worked when technology improves. This evidence ties the contractionary result directly to price rigidity.

D. Sectoral Shifts?

Price stickiness can explain why technology improvements are contractionary. Alternatively, 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 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, $Disp_t = \left[\sum_{i=1}^{N} w_i \left(\widehat{dz}_{it} - \widehat{dz}_t \right)^2 \right]^{1/2}$ where i indexes industries, $\widehat{dz}_{it}$ is the estimated industry technology shock (scaled to be value-added augmenting), and w_i is the sector's value-added weight.

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 and standard errors of technology and its lags. The timing patterns discussed in Section III are unaltered. Most importantly, 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, technology dispersion is associated with lower growth in output, utilization, and business investment with a one year lag; the effect on hours and total inputs appears less significant.) Overall, the evidence seems more consistent with the sticky-price model of contractionary technology improvement than with the sectoral-shifts alternative.

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.[35] For example, Greenwood and Yorukoglu (1997) argue that the introduction of the PC 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 capital as output, market output—and hence measured productivity growth—may be relatively low. Therefore, low productivity growth is associated with high input growth, because "full" output is mismeasured. Over time, the investment in knowledge does lead to an increase in measured output and productivity.

This class of models does not generally predict our results. We do not correct for mismeasured output

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[34] 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.

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 learning-time story is unlikely to explain our results.

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.[36] This hypothesis predicts countercyclical productivity, so proponents (e.g., Caballero and Hammour, 1994, p. 1365) have argued that "other factors (labor hoarding, externalities, etc.) ... make measured productivity procyclical." 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's (1997b) classify such cleansing effects as "reallocations"—a shift in resources from inefficient to efficient firms—not a change 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 (2004) confirm our findings with firm level data. (Of course, there are no firm-level data sets 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 δ<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 $\text{plim}\ \hat{\gamma} = (\gamma + \delta) < \gamma$, but the estimated technology shocks do not incorporate causation from inputs to technology. ξ is a form of classical measurement error in our residuals.

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[35] See, e.g., Galor and Tsiddon (1997), Greenwood and Yorukoglu (1996), and Basu *et al*. (2003).

[36] This idea goes back at least to Schumpeter. Foster, Krizan, and Haltiwanger (1998) provide empirical evidence on the role of entry and exit in aggregate productivity growth.

(This cleansing effect could explain why some of our estimated industry returns to scale are less than one.[37])

However, if ξ is correlated with business-cycle variables—reallocations may, for example, depend on the aggregate cycle as well as sectoral inputs—then some part of our residuals may remain correlated with output and input changes for reasons of reverse causality.

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, providing some evidence against the cleansing interpretation.

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 Cooper and Haltiwanger (1996) document for the seasonal cycle in the auto industry. Since non-residential investment falls sharply in the first year following a technology improvement, a flexible-price reorganization model probably cannot explain the results we find.[38]

VI. Conclusion

In this paper, we measure aggregate technology by correcting the aggregate Solow residual for increasing returns, imperfect competition, varying utilization of capital and labor, and aggregation effects. We reach a robust conclusion: In the short run, technology improvements significantly reduce input use and non-residential investment; output changes little. Inputs, non-residential investment, and output recover significantly during the next several years.

These results are inconsistent with standard parameterizations of real-business-cycle models, which imply that technology improvements raise input use at all horizons. We also find that technology shocks do

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[37] "Non-cleansing" reallocations can also explain returns to scale *(γ)* estimates less than one, e.g., if in sub-industries, the cyclicality of input use covaries negatively with returns to scale. This could arise, for example, if high income elasticities (leading to high procyclicality) 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 .

not account for a very high fraction of the variance of inputs and output at cyclical frequencies. By contrast, we argue that these results *are* qualitatively consistent with the predictions of dynamic general-equilibrium models with sticky output prices driven by both technology and monetary shocks.

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 Fed 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 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 identified-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 stickiness as the major reason for the perverse short-run effect of technical improvement, as do Galí (1999) and Galí and Rabanal (2004). The evidence is broadly consistent with this view. 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, sharper tests are needed before we can be sure that price stickiness does explain our results.

A complication, of course, is that the alternative hypotheses are not mutually exclusive, but could all contain an element of the truth. Indeed, estimated DGE models by Smets and Wouters (2004) and Galí and Rabanal (2004) suggest that both nominal *and* real rigidities play a role.

Nevertheless, 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 1949-96 time 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 whether technology has accelerated—and if so, 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

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

policymakers can better assess technological movements, monetary policy might be improved in the future.

Appendix I: Data and Instruments

We use industry data even though the theory probably applies most naturally to firms. Unfortunately, no firm-level data sets span the economy. Narrowing the focus to a subset of the economy—e.g., using the Longitudinal Research Database—would require sacrificing a macroeconomic perspective, as well as panel length and data quality.

Jorgenson dataset. We use updated data described in Jorgenson, Gollop, and Fraumeni (1987).[39] (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 datset that runs 1948-1989. We used growth rates from 1949 to 1958 from the older dataset and growth rates 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, with log-changes weighted by average nominal shares in periods *t* and *t-1*. However, to construct industry input aggregates, we use factor shares averaged over the entire sample period. We use average factor shares because we are concerned that observed factor payments may not be allocative period-by-period, e.g., because of implicit contracts. This leads us to take an explicit first-order approximation to the industry production function. Results do not appear at all sensitive to this choice, however: Results appear virtually identical using time-varying shares.

We assume that industries earn zero economic profits, so that factor shares sum to one. In U.S. data, pure profits generally appear small (see, e.g., Rotemberg and Woodford 1995). In previous work with older versions of the Jorgenson dataset, we estimated payments to capital as in Hall (1990); estimated profits were generally small, and results were virtually indistinguishable from those that assumed zero profits.

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 hours-per-worker using Christiano-Fitzgerald's (2003) band pass filter, isolating frequency components between 2 and 8 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*. (Detrending log hours/worker with an HP filter or a simple first-difference filter makes little difference to results. In addition, using Jorgenson's hours/worker data yields very similar results, although the resulting technology series is a bit more volatile.)

National accounts data. All series were downloaded from the Bureau of Economic Analysis, via Haver Analytics database, on April 7, 2004.

Instruments

Monetary Shocks. We use quarterly VAR monetary innovations, following Christiano, Eichenbaum, and Evans (1999), Burnside (1996), and others. Following Burnside (1996), we measure monetary policy as innovations to the 3-month Treasury bill rate, since the fed funds market did not exist until the mid-1950s (from 1954:1 through 2003:1, the quarterly average 3-month T-bill rate has a correlation with the fed funds rate of over 0.99). More specifically, we measure monetary shocks as the innovations to the 3-month T-bill rate from a VAR with GDP, the GDP deflator, an index of commodity prices, the 3-month T-bill rate, and M1. (We thank Charles Evans for providing RATS code that estimated the VAR and innovations).[40]

We sum the quarterly series for the preceding year to obtain an annual series. In principle, we could use

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

[40] GDP and the GDP deflator are from the BEA via Haver (downloaded May 22, 2003). The 3-month T-bill rate is the rate quoted on the secondary market, from Federal Reserve Board publication H.15 via Haver. M1 is from the Philadelphia Fed real-time dataset. We spliced data from 2003Q1 (from 1959 onwards) to data from 1973Q4 (which covers the pre-1959 period). Charles Evans provided us with the PCOM data used in Christiano, Eichenbaum, and Evans (1999). We extended his PCOM variable back one year, to 1947, by splicing his series with Conference Board data on raw materials spot prices *SMP* (Haver mnemonic U0M023, downloaded Aug 15, 2003). Following Evans, we filter *SMP* as follows: $PCOM(t) = 1.451 \cdot PCOM(t-1) - 0.586 \cdot PCOM(t-2) + 0.134 \cdot \Delta \ln(SMP(t))$.

the four quarterly shocks separately as instruments, but the first-stage F-statistic falls sharply.

Government Spending. We use the average quarterly growth rate 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*.[41]

Petroleum prices. Following Mork (1989), we base our oil instrument on 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; controls were not completely removed until the early 1980s and bind particularly sharply in early 1974.[42]

RAP is available monthly from January 1974 on. However, an annual average series is available from the late 1960s on. We follow Mork (1989) in linking the PPI and the annual composite RAP to create an estimated quarterly refiner acquisition price.[43] We assume that before 1974, the refiner price moves one-for-one with the PPI, since the annual growth rate in the composite refiner price moves quite closely with the annual growth in the crude petroleum PPI. In particular, domestic purchases accounted for about 80 percent of refiner purchases and price controls were a minor factor: In 1973, for example, the average RAP for domestic crude oil was $4.17 a barrel while the average RAP for imported oil was $4.08 a barrel. (In 1974, by contrast, the domestic price rose to $7.18/barrel, while the imported price rose to $12.52.)[44]

We normalize the estimated pre-1974 oil prices so that the average monthly price in 1973 matches the average price in the reported annual composite RAP. Since the PPI is an index, we make a levels-adjustment to get a monthly oil price for the pre-1974 period. The annual composite RAP in 1973 averaged $4.15, so we normalized our derived monthly series to have an average value of $4.15 in 1973.[45]

Hamilton (1996) recommends focusing on oil price *increases* above the peak level over the preceding 12 months. First, Hamilton and others find a nonlinearity: oil price increases are more contractionary than oil price declines are expansionary. Second, he argues that oil price increases have a larger effect if they follow stable prices than if they simply reverse an earlier decline. Thus, 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.) For annual data, we take as our instrument the sum of the quarterly shocks in the preceding calendar year.

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[41] Downloaded from the Bureau of Economic Analysis via Haver Analytics December 12, 2002.

[42] Mark French and Rob Vigfusson independently pointed out to us the problems with the PPI.

[43] Vigfusson (2002) uses the IMF world spot price of oil. This series, which is not available for our full sample period, moves reasonably closely with the PPI until the first quarter of 1974—when the log change in the IMF world price is 1.37 while the log change in the PPI is 0.32. (IMF Data from International Financial Statistics July 2002 CD-ROM). These changes bracket the price change for U.S. purchasers of oil: In annual data, the 1974 log change in the refiner acquisition price is 0.78; the log-change in the IMF world price is 1.27, compared with 0.52 for the PPI. In sum, we view the composite refiner acquisition price as a better measure of the relative price shock that hit the U.S. economy.

[44] From Haver Analytics, we downloaded the PPI for crude petroleum (mnemonic P0561) and the Composite Refiner Acquisition Price for Crude Oil (PZRAC). We downloaded annual RAP data from the Department of Energy at http://www.eia.doe.gov/emeu/aer/txt/ptb0519.html. (All downloads were December 11, 2002)

[45] Results appear little affected by alternative ways of linking data over the price-control period 1971 to 1974. For example, we tried a further levels adjustment to exactly match the average price in our constructed series to the actual average 1972 composite price. In addition, we tried deflating by the GDP deflator and also using actual log-change in oil prices rather than using oil price increases only. All of these made little or no perceptible difference to results.

Appendix II: The Purified Technology Series as a Generated Regressor[46]

Our application focuses on hypothesis testing. Arguing that some variable s_{t+j} (say current or forwarded employment or investment) covaries negatively with the true technology series $\zeta_t(\Gamma)$, is equivalent to arguing that the true value of θ in the regression

$$s_{t+j} = \alpha + \theta\zeta_t(\Gamma) + \nu_t$$

is negative. The problem is that $\hat{\theta}$ is estimated from the OLS regression

$$s_{t+j} = \alpha + \theta\zeta_t(\hat{\Gamma}) + \nu_t,$$

where $\hat{\Gamma}$ is the estimated value of the "first-step" parameters. Testing the null hypothesis that θ is equal to zero would require no generated regressor correction for the asymptotic hypothesis test.

But arguing that the covariance is negative requires rejecting not only the hypothesis that θ=0, but also rejecting any positive value of θ. Because the test statistic is not monotonic in the true value of θ, rejecting any positive value of θ requires one additional condition beyond rejecting the hypothesis that θ=0. As shown below, the *additional* condition does not depend on any characteristics of s_{t+j}, and so can be interpreted as an overall "quality control" condition on the generated regressor $\zeta_t(\hat{\Gamma})$. As long as this overall "quality control" condition on the generated regressor is satisfied, the uncorrected test statistic is valid for asymptotic tests of the hypothesis that $\theta \geq 0$. The remainder of the appendix demonstrates this claim and spells out the "quality control" condition on the generated regressor.

Our estimation involves a two-step procedure. In the first step, after stacking the industries on top of each other, we can use an instrumental variable row vector q_i to estimate the parameter vector Γ in

$$dy_t = \xi_t\Psi\Gamma + \varepsilon_t,$$

where t is time, dy_t is a vector of changes in industry log gross output, ξ_t is a matrix whose "diagonal" elements are the row vectors $[1, \chi_{(t>73)}, dx_i, dh_i]$ for industry i, Ψ embodies the cross-equation restrictions, and ε_t is a vector of the demeaned industry-level technology changes, which is the first-step error term. For an appropriately defined r_t (see Jeffrey M. Wooldridge 2002, page 140), the estimator satisfies

$$\sqrt{T}(\hat{\Gamma} - \Gamma) = T^{-1/2}\sum_{t=1}^{T} r_t + o_p(1).$$

We assume that r_t is serially uncorrelated. (Since autocorrelation-robust standard errors in the first step are quite similar to uncorrected standard errors, and the estimated aggregate technology shocks themselves are serially uncorrelated, deviations from this assumption are unlikely to be substantial enough to seriously alter the bottom line below.) We can write the usual estimator of the variance-covariance matrix of $\hat{\Gamma}$ as

$$\hat{\Phi} = T^{-1}\sum_{t=1}^{T} \hat{r}_t\hat{r}_t'.$$

Denote the generated demeaned aggregate technology change by $\hat{\zeta}_t = \zeta_t(\hat{\Gamma})$. $\zeta_t(\hat{\Gamma})$ is a linear combination of the estimated first-step errors $\hat{\varepsilon}_i$, and so is a function of the data and the estimated value of Γ. By construction, $\zeta_t(\hat{\Gamma})$ has a mean of zero. As noted in the main text, we cannot reject the hypothesis that the (demeaned) aggregate technology change is white noise. Imposing the assumption that the true (demeaned) technology change $\zeta_t(\Gamma)$ is white noise is helpful in delineating the issues that arise because the estimated technology change $\zeta_t(\hat{\Gamma})$ is a generated regressor. If technology changes are white noise, then the covariance of a current technology change with a given variable conditional on other leads and lags of technology is the same as the unconditional covariance of the current technology change with that variable. The unconditional covariance can be consistently estimated by univariate OLS. The simplicity of univariate OLS greatly clarifies the generated regressor problem in this context.

As above, let s_{t+j} be any variable that is of interest because it might be affected by the technology change

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[46] We thank Jeff Wooldridge and Serena Ng for helping us with this appendix. All errors remain our own.

at time t. For example, s_{t+j} could be the current level of aggregate inputs, or a lead of the aggregate input level. In the second step we estimate

$$s_{t+j} = \alpha + \theta\zeta_t(\hat{\Gamma}) + v_t$$

by OLS. Because our focus is on hypothesis testing, we want to know the variance of the estimate $\hat{\theta}$ conditional on a range of values of the true θ. Following Wooldridge (2002, pp 139-141),

$$\sqrt{T}(\hat{\theta} - \theta) \sim Normal(0, V),$$

where $V = \text{plim}(\hat{A}\theta^2 - 2\hat{B}\theta + \hat{C})$, with

$$\hat{A} = \hat{D}^{-2}T^{-1}\sum_{t=1}^{T}\{\hat{H}\hat{r}_t\}^2 = \hat{D}^{-1}\hat{H}\hat{\Phi}\hat{H}'\hat{D}^{-1},$$

$$\hat{B} = \hat{D}^{-2}T^{-1}\sum_{t=1}^{T}\hat{\zeta}_t\hat{v}_t\hat{H}\hat{r}_t,$$

$$\hat{C} = \hat{D}^{-2}T^{-1}\sum_{t=1}^{T}\hat{\zeta}_t^{\,2}\hat{v}_t^{\,2},$$

$$\hat{D} = T^{-1}\sum_{t=1}^{T}\hat{\zeta}_t^{\,2}$$

and

$$\hat{H} = T^{-1}\sum_{t=1}^{T}\hat{\zeta}_t[\nabla_\Gamma\zeta_t(\hat{\Gamma})],$$

where $\nabla_\Gamma\zeta_t(\hat{\Gamma})$ is the gradient of $\zeta_t(\Gamma)$ with respect to Γ, evaluated at $\hat{\Gamma}$ and expressed as a row vector. The Cauchy-Schwartz inequality implies that $\hat{A}\hat{C} - \hat{B}^2 \geq 0$. Note that $\hat{A}$ is independent of the particular variable being represented by s_{t+j}, since $\hat{v}_t$ does not appear in its formula. All the extra information one needs to know from the first-step in order to calculate $\hat{A}$ is $\hat{\Phi}$, the estimate of the variance-covariance matrix of the first-step parameter vector, together with the gradient vector $\nabla_\Gamma\zeta_t(\hat{\Gamma})$.

We are interested in showing that θ is negative. (For the most important cases, this is the natural direction. Cases in which we want to show that a covariance of technology shocks with a variable is positive can be handled by defining s_{t+j} as the negative of the variable of interest.) Showing that θ is negative can be formalized as a rejection of any hypothesis that has a nonnegative value for θ. That is, for all $\theta \geq 0$, if κ is the designated critical ratio, we need

$$f(\theta) = \frac{\sqrt{T}(\theta - \hat{\theta})}{\sqrt{\hat{A}\theta^2 - 2\hat{B}\theta + \hat{C}}} > \kappa.$$

If the test statistic $f(\theta)$ is monotonically increasing in θ, showing that $f(0) > \kappa$ is enough to guarantee that $f(\theta) > \kappa$ for all $\theta > 0$ as well. However, $f(\theta)$ is not, in general, monotonically increasing in θ. Instead, we demonstrate the following Lemma.

Lemma: *if* $\hat{\theta} < 0$, *then*

$$\min_{\theta\in\Re^+} f(\theta) \geq \min(f(0), f(+\infty)).$$

As a consequence, showing that $f(0) > \kappa$ *and that*

$$f(+\infty) = \lim_{\theta\to+\infty} f(\theta) = \sqrt{\tfrac{T}{\hat{A}}} > \kappa$$

are together sufficient to guarantee that $f(\theta) > \kappa$ *for all* $\theta \geq 0$.

Remarks: As noted above, $\hat{A}$ depends only on the details of the first-step estimation and not on the identity of s_{t+j}, so the condition $\sqrt{\tfrac{T}{\hat{A}}} > \kappa$ can be seen as an overall "quality control" condition for the generated regressor. (It is often true in applications that no generated regressor correction is needed for rejecting a zero value of a parameter. The complication here is that we need to reject *θ>0* as well, which also requires $\sqrt{\frac{T}{\hat{A}}} > \kappa$.)

For our measure of aggregate technology change, we calculated $\hat{A}$, which equals 0.0061. Thus, $\sqrt{\frac{T}{\hat{A}}}$ equals $\sqrt{48/0.0061} = 89$ —far in excess of what's needed to pass this "quality control" condition at any reasonable level of significance.

Proof: The easiest way to demonstrate that $\min_{\theta\in\Re^+} f(\theta) \geq \min(f(0), f(+\infty))$ as promised is to show that *f(θ)* is either (a) monotonically increasing on $\Re^+$, (b) monotonically decreasing on $\Re^+$, or (c) first increasing, then decreasing on $\Re^+$. The derivative of the test statistic is

$$f'(\theta) = T^{1/2}\{\hat{A}\theta^2 - 2\hat{B}\theta + \hat{C}\}^{-3/2}[(\hat{A}\hat{\theta} - \hat{B})\theta + (\hat{C} - \hat{B}\hat{\theta})].$$

Thus, the sign of $f'(\theta)$ is the same as the sign of the linear function $(\hat{A}\hat{\theta} - \hat{B})\theta + (\hat{C} - \hat{B}\hat{\theta})$. If $\hat{C} - \hat{B}\hat{\theta} \geq 0$, then $f'(0) \geq 0$, and *f(θ)* must be either (a) monotonically increasing on $\Re^+$, or (c) first increasing, then decreasing on $\Re^+$, depending on the sign of $\hat{A}\hat{\theta} - \hat{B}$. If $\hat{C} - \hat{B}\hat{\theta} \leq 0$, then the Cauchy-Schwartz inequality $\hat{A}\hat{C} - \hat{B}^2 \geq 0$ implies that

$$\hat{B}^2 \leq \hat{A}\hat{C} \leq \hat{A}\hat{B}\hat{\theta}.$$

Since $\hat{\theta} < 0$, $\hat{B} < 0$ and dividing both sides by $\hat{B}$ indicates that

$$\hat{B} \geq \hat{A}\hat{\theta},$$

so that $\hat{A}\hat{\theta} - \hat{B} \leq 0$. Therefore, if $\hat{C} - \hat{B}\hat{\theta} \leq 0$, then $\hat{A}\hat{\theta} - \hat{B} \leq 0$ as well, and

$$f'(\theta) = T^{1/2}\{\hat{A}\theta^2 - 2\hat{B}\theta + \hat{C}\}^{-3/2}[(\hat{A}\hat{\theta} - \hat{B})\theta + (\hat{C} - \hat{B}\hat{\theta})] \leq 0$$

for all $\theta>0$, implying in turn that *f(θ)* is (b) monotonically decreasing on $\Re^+$.

Appendix III: Classical Measurement Error in Inputs

Classical measurement error in inputs could, in principle, lead to counter-cyclical measurement error in our technology residuals. However, a simple model suggests that such measurement error cannot explain our results. First, for plausible parameterizations of the importance of measurement error, the "true" correlation remains negative. Second, the observed covariance between measured output and technology, which is zero or negative, bounds the covariance between true technology and true inputs, again suggesting a negative "true" correlation.

In our empirical work, we take the entire regression residual as "technology," implicitly assuming that our utilization proxies control fully for all variations in utilization. If they do not, but merely provide unbiased estimates of utilization, then the residual includes non-technological "noise" that is completely analogous to classical measurement error. Our model here abstracts from variations in utilization and does not explicitly consider aggregation across industries; neither changes the basic message.

Suppose the true economic model is given by

$$dy^* = \gamma\, dx^* + dz^*, \tag{A.1}$$

where the starred variables are unobserved, true values. Both output and inputs are measured with error:

$$dy = dy^* + \eta \tag{A.2}$$

$$dx = dx^* + \varepsilon, \tag{A.3}$$

where η and ε are iid, mean-zero variables with variances σ_η^2 and σ_ε^2, respectively. Note that the estimated variances of *dy* and *dx* always exceed their true values: $\sigma_{dx}^2 = \sigma_{dx^*}^2 + \sigma_{\varepsilon}^2$ and $\sigma_{dy}^2 = \sigma_{dy^*}^2 + \sigma_{\eta}^2$.

Now suppose we estimate (A.1) by instrumental variables. If the instruments are uncorrelated with the measurement error, then the estimate of γ is consistent. Hence, in the limit, the only source of error in our estimate of technology change is the measurement error in *dy* and *dx*:

$$dz = dz^* + \eta - \gamma\varepsilon . \tag{A.4}$$

Abstracting from estimation error in γ, equation (A.4) implies that $\sigma_{dz}^2 = \sigma_{dz*}^2 + \sigma_{\eta}^2 + \gamma^2\sigma_{\varepsilon}^2$. Note that for given observed variance of measured technology, as measurement error becomes larger, the variance of true technology shocks dz*must fall. Using equation (A.4), the covariances of estimated technology change with output and input growth are:

$$\operatorname{cov}(dz, dy) = \operatorname{cov}\left(dz^*, dy^*\right) + \sigma_{\eta}^2 \tag{A.5}$$

$$\operatorname{cov}(dz, dx) = \operatorname{cov}\left(dz^*, dx^*\right) - \gamma\sigma_{\varepsilon}^2 . \tag{A.6}$$

Measurement error hence biases up both the estimated covariance between output and technology, and the estimated standard deviation of technology. If the true correlation between output growth and technology change is positive, then the estimated correlation may be biased either towards or away from zero, but cannot turn negative. However, suppose the true correlation between output growth and technology change is negative. Then the estimated correlation is unambiguously towards zero. Thus, our point estimates a negative correlation between output growth and technology change cannot be attributed to measurement error.

However, if the true covariance $\operatorname{cov}(dz^*, dx^*)$ is positive, then the estimated correlation is biased down. If the true input covariance is negative, then the estimated correlation might be biased up or down. To assess the the input-mismeasurement bias, we rewrite (A.6) in terms of correlations: Some algebra yields:

$$Corr(dz*, dx*) = \left[Corr(dz, dx) + \gamma \frac{\sigma_{\varepsilon}^2}{\sigma_{dz}\sigma_{dx}} \right] \left[\frac{\sigma_{dz}\sigma_{dx}}{\sigma_{dz*}\sigma_{dx*}} \right]$$

By specifying returns to scale and variances, we can calibrate this equation to observed correlations and variances. Suppose returns to scale are constant and that output is measured without error (output measurement error strengthens our case by reducing the variance of true technology), then the *maximum* σ_{ε} is 1.41 percent, given that this is the standard deviation of measured technology (since $\sigma_{dz*}^2 = \sigma_{dz}^2 - \sigma_{\eta}^2 - \gamma^2\sigma_{\varepsilon}^2 \geq 0$). In this case, there is no variation in true technology and the true correlation of inputs and technology is undefined. If instead we assume σ_{ε} is 1 percent—still a high number—then σ_{dz*} is also 1 percent. If we define true inputs as the sum of observed utilization plus measured utilization, then observed σ_{dx} is 3.3 percent per year; the "true" correlation between technology and inputs is –0.37, compared with the observed correlation with inputs of –0.50. Even if σ_{ε} is 1.35 percent, the true correlation remains at 0.15.

Finally, we are mostly interested in the signs of the correlations rather than their sizes. We can use the upward-biased output covariance to bound the input-covariance from above. Equation (4.1) implies that

$$\operatorname{cov}\left(dz^*, dy^*\right) \geq \operatorname{cov}\left(dz^*, dx^*\right), \tag{A.7}$$

(since the variance of dz^* is positive and $\gamma \geq 1$). But we see from equation (4.5) that

$$\operatorname{cov}(dz, dy) \geq \operatorname{cov}\left(dz^*, dy^*\right).$$

Our estimated covariance of output and technology appears to be either approximately zero or even negative. Thus, we conclude that the true covariance of technology and inputs must also be zero or smaller. Thus, our surprising results about the effects of technology improvements survive considerations of measurement error.

Since we cannot observe measurement error directly, we cannot say how much it affects our results. However, since the bias works against our finding that technology improvements reduce output, it seems likely that technology improvements are in fact contractionary. Furthermore, unlike the simple model used here, our technology change series takes a weighted average of technology shocks across sectors. If measurement error is relatively independent across industries, averaging should attenuate any biases.

Appendix IV. Small Sample Properties of Instrumental Variables

Could our results arise from a weak instruments problem? For example, the average F statistic from the first-stage regression of industry inputs *dx* on the instruments is 5.4—high enough to be statistically significant. But Staiger and Stock (1997) suggest that instrumental variables estimators sometimes have poor small sample properties when the first-stage F statistic is less than about 10.

Nevertheless, the small sample properties of instrumental variables do not appear to drive our results. First, Staiger and Stock note that LIML has better small sample properties than TSLS. LIML gives results that are qualitatively similar, though with much higher variance, than our preferred results. Second, when we throw out the industries for which the instruments are *particularly* bad (with first-stage F-statistics for dx_i of less than 2), the correlation of technology with hours remains significantly negative.

Third, and more substantively, we pooled industries within groups in order to raise the significance level of the first stage regression; we still find a robust negative correlation of technology and hours. To implement the pooled approach, we stacked industries within groups (durables, non-durables, and non-manufacturing) and then estimated equation (2.1) as a single regression for each group. We thus end up with a separate estimate of γ and β for each group. (In all cases, we removed industry fixed effects by demeaning all variables). The instruments generally appear highly relevant for the stacked regressions, with F statistics for *dx* that range from 15 to 40; the F statistic for *dh* ranges from 8 to 28.[47] After estimating the pooled regressions, we unstack the residuals into industry residuals, and aggregate as before. The resulting technology series has a correlation of 0.9 with our preferred technology series from Tables 2 and 3. The contemporaneous correlation between technology and hours is a statistically significant -0.39. (It is not surprising that the correlation is a bit less negative than before, given that we lose some of the "reallocation" effects that come from allowing for differences in γ 's.)

Finally, we simulated 1000 draws of random, irrelevant instruments and ran our system, deriving 1000 artificial technology series. We then assessed the actual small sample distribution of coefficients and t-statistic from an OLS regression of actual hours growth on estimated technology (contemporaneous only) under the null that the instruments are, in fact, irrelevant. As expected, coefficients are biased towards the OLS estimates—which yield a small positive coefficient, not the negative coefficient we find. In 123/1000 cases, the t-statistic at least as negative as -2; and in 54/1000 cases (5.4 percent), the t-statistic was as negative as (the OLS coefficient) from our main results of -3.6 (this differs slightly from Table 3, since it's a bivariate regression and it does not calculate Newey-West-corrected standard errors). These frequencies are considerably higher than one would expect from a normal distribution, but it nevertheless suggests it is very unlikely that random instruments explain our results.

Since the pooled specification largely replicates our overall results, we examined what the first-stage F statistics with generated instruments look like. With the pooled data, the median (for all three groups) of the first-stage F statistics with random instruments was 0.9 for *dx* and 0.8 for *dh*. Indeed, for *none* of the 1000 replications were *any* of the first-stage F statistics for any of the six variables—*dx* and *dh* for durable manufacturing, non-durable manufacturing, and non-manufacturing—as large as we actually found in the actual data. Hence, it is exceedingly unlikely that weak instruments alone could explain both our large negative regression coefficient and our relatively high first stage F statistic in the pooled specification.

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[47] *dh* in non-manufacturing equal to 8.2 is lower than we would like. But pooled results are virtually unaffected by doing LIML rather than TSLS (since LIML is more robust to small sample issues, though often more variable); and indeed, results are robust to focusing on manufacturing alone.

In addition, we looked more closely at the underlying cases where we found a large negative t-statistic. These generally appear to be cases where one or more of the point estimates of returns to scale are extremely large (e.g., 3 or more). In particular,

- Quantitatively, the negative correlation disproportionately represented the effect of a single industry,[48] in contrast to our results;
- The variance of the derived technology shocks tended to be much larger than for our actual purified technology series. (The median ratio of the variances was 2.8, and in only 1 of the cases was the variance smaller with the random instruments.).

In sum, although weak instruments are a concern, they cannot explain our results.

[48] Since $dz = \sum_i w_i (dz_i / (1 - s_{Mi}))$, the arithmetic contribution $CONT_i$ of each sector to the aggregate correlation (so that $Corr(dz, dx^V) = \sum Cont_i$) is $Cov(w_i dz_i / (1 - s_{Mi}), dx^V) / (stdev(dz) \bullet stdev(dx^V))$. In our reported results, 23 of the 29 industries contribute negatively, with the largest (negative) arithmetic contribution being -0.11 (construction). Of the 123 simulated cases with a negative t-statistic of -2 or larger in magnitude, only 1 simulation had a single contribution as small in magnitude as -0.11. For the cases with a t-statistic at least as large in magnitude as -3, *none* had a single-industry contribution as small in magnitude as -0.11.

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Table 1. Parameter Estimates

A. Returns-to-Scale (γ_i) Estimates

Durable Manufacturing		Non-Durable Manufacturing		Non-Manufacturing	
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)
Non-Elect. 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 Manuf. (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)	Non-Durables Manufacturing	2.13 (0.38)	Non-Manufacturing	0.64 (0.34)

Notes: Heteroskedasticity- and autocorrelation-robust standard errors in parenthesis. Coefficients from regression of output growth on input growth and hours-per-worker growth. (Constant terms and, for non-manufacturing, post-1972 dummy, not shown.) Hours-per-worker coefficient is constrained to be the same within a group (durables, non-durables, and non-manufacturing). Instruments are oil price increases; growth in real defense spending; and VAR monetary innovations (all instruments are sums of quarterly shocks for the preceding year).

Table 2. Means and Standard Deviations of Productivity and Technology

Private Economy, Manufacturing, and non-Manufacturing
(annual percent change)

		Private Economy	Durable Manuf.	Non-Durable Manuf.	Non-Manuf.
Solow Residual	Mean	0.79	1.75	2.07	0.34
	Std Deviation	2.04	3.59	4.18	1.90
"Purified" Residual	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 residuals come from aggregating residuals (including constant terms) for 29 industries covering the non-farm private economy from the regression results shown in Table 1, including growth in hours per worker to control for 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.

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	1.17	0.52	-0.08	-0.48	0.43	2.39
		(0.21)	*(0.34)*	*(0.2)*	*(0.21)*	*(0.2)*		
(2)	Hours	-0.60	0.55	0.51	-0.06	-0.41	0.45	1.70
		(0.14)	*(0.27)*	*(0.12)*	*(0.16)*	*(0.19)*		
(3)	Input	-0.44	0.40	0.43	0.08	-0.21	0.48	1.45
		(0.09)	*(0.17)*	*(0.07)*	*(0.12)*	*(0.12)*		
(4)	Utilization	-0.40	0.68	0.06	-0.26	-0.23	0.53	2.81
		(0.13)	*(0.15)*	*(0.16)*	*(0.12)*	*(0.09)*		
(5)	Employment	-0.52	0.36	0.48	0.11	-0.34	0.42	1.56
		(0.11)	*(0.25)*	*(0.09)*	*(0.15)*	*(0.18)*		
(6)	TFP (Solow residual)	0.44	0.76	0.09	-0.16	-0.27	0.46	2.94
		(0.15)	*(0.2)*	*(0.17)*	*(0.13)*	*(0.11)*		
(7)	Non-residential fixed investment	-1.07	1.04	1.63	-0.20	-0.81	0.35	1.43
		(0.36)	*(0.79)*	*(0.43)*	*(0.42)*	*(0.61)*		
(8)	Resid investment and cons. durables	1.25	2.83	-0.07	-1.70	-1.36	0.44	2.20
		(0.5)	*(0.89)*	*(0.67)*	*(0.47)*	*(0.58)*		
(9)	Consumer Non-Dur and Services	0.10	0.41	0.24	0.03	-0.14	0.40	1.79
		(0.11)	*(0.11)*	*(0.1)*	*(0.08)*	*(0.09)*		
(10)	Δ Inventories/GDP *(not in growth rates)*	-0.14	0.11	0.14	0.04	-0.02	0.42	1.60
		(0.03)	*(0.05)*	*(0.04)*	*(0.05)*	*(0.04)*		
(11)	Net Exports/GDP *(not in growth rates*	0.01	-0.03	0.07	0.19	0.20	0.16	0.29
		(0.13)	*(0.13)*	*(0.1)*	*(0.1)*	*(0.11)*		

Notes: Each row represents a separate OLS regression of the variable shown (in growth rates, unless otherwise indicated) on the current value plus four lags of estimated technology growth, 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 1953-1996. (master_subper_2.4.10lags.xls)

Table 4. Fraction of Variance Due to Technology Shocks

Lags	Output	Inputs	Hours	Utilization	Solow Res.
0	0	28	21	23	18
1	19	13	10	20	56
3	43	13	9	17	70
10	51	11	7	11	76

Notes: Variance decomposition from bivariate VAR of technology and the variable shown.

Table 5. 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*)
Construct.	0.47***	0.15	0.38***	0.07
Manufact. Durables	0.75***	0.64***	-0.44***	-0.50***
Manufact. Non-Durables	0.67***	0.32**	-0.16	-0.27**
Transport	0.68***	0.27*	0.34**	-0.10
Communications	0.60***	-0.07	0.19	-0.47***
Public Utilities	0.66***	0.20	0.17	-0.32**
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***	-0.06
Median of One-Digit Correlations	0.66	0.20	0.34	-0.27
Median of 29 Industries (21 Manufacturing, 8 other)	0.57	0.15	0.01	-0.33
Number of individual industries with negative correlation	3	12	14	24

Notes*:* The 29 individual industries and the 9 one-digit industries span the private non-farm, non-mining business economy. Industry TFP growth, which imposes constant returns and no utilization effects, is *dp*. Technology *dz* is the purified industry technology residual. All correlations are calculated from 1949-1996. For one-digit correlations, *** indicates statistical significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 6. Responses by sub-period

	1949-1979			1980-1996			R^2	DW Statistic
	dt	dt(-1)	dt(-2)	dt	dt(-1)	dt(-2)		
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.

sub2lags-5.xls

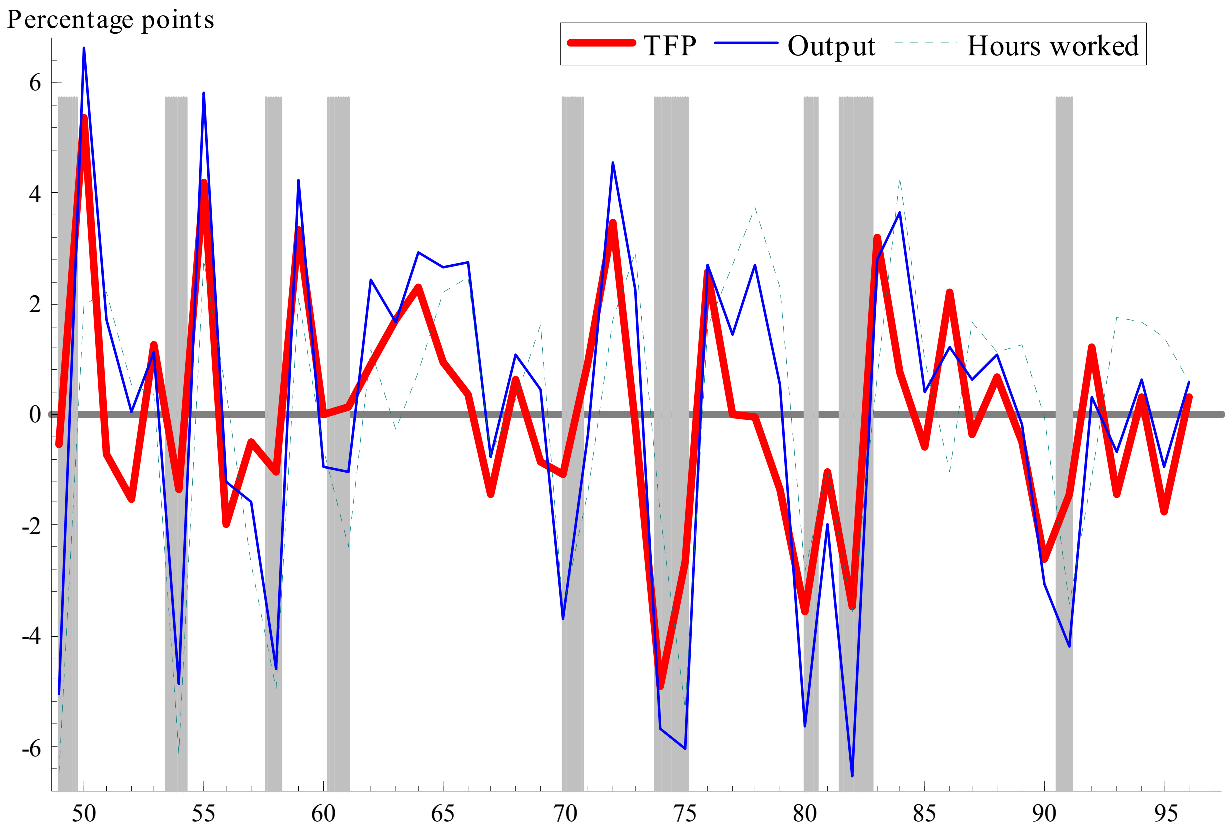
Table 7. Effect of Technology Improvements and Technology Dispersion on Growth Rates of Output, Input, Utilization, and Non-Residential Fixed Investment

	Output *dv*	**Inputs** dx^V	**Hours**	**Utilization**	**Non-residential investment**
dz_t	0.10	-0.43	-0.56	-0.34	-0.96
	(0.17)	(0.08)	(0.13)	(0.1)	(0.39)
dz_{t-1}	0.99	0.36	0.48	0.58	0.79
	(0.3)	(0.16)	(0.27)	(0.11)	(0.71)
dz_{t-2}	0.62	0.47	0.55	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	-0.21
	(0.19)	(0.14)	(0.19)	(0.09)	(0.55)
dz_{t-4}	-0.66	-0.27	-0.50	-0.33	-1.12
	(0.23)	(0.13)	(0.2)	(0.12)	(0.61)
$Disp_t$	0.23	-0.01	0.08	0.14	-0.06
	(0.20)	(0.11)	(0.17)	(0.15)	(0.48)
$Disp_{t-1}$	-0.70	-0.19	-0.31	-0.38	-1.04
	(0.21)	(0.1)	(0.18)	(0.11)	(0.51)
$Disp_{t-2}$	0.32	0.12	0.15	0.18	0.09
	(0.29)	(0.17)	(0.24)	(0.13)	(0.59)
R^2	0.50	0.50	0.47	0.58	0.39
D. W.	2.30	1.45	1.66	2.74	1.51

Note: Each column is a separate regression of the growth rate of the variable shown on purified technology growth, dz_t, and the weighted cross-sectional standard deviation of industry technology shocks, *Disp*. Heteroskedasticity- and autocorrelation-robust standard errors in parentheses (calculated with TSP's GMM command with NMA=3). Regressions include a constant, not shown. Sample period is 1953-1996.

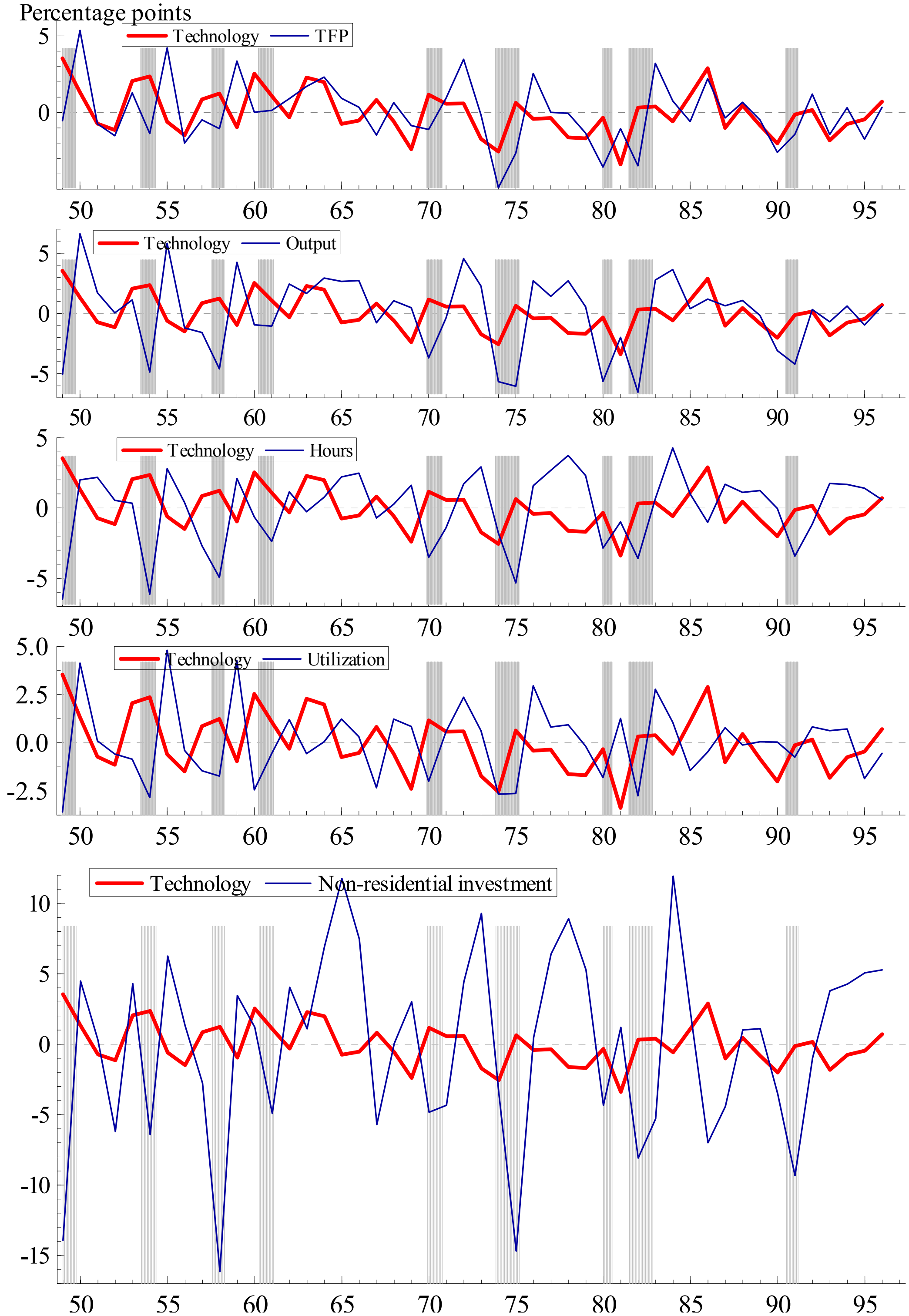
Dispersion-table.xls

Figure 1. TFP, Output, and Hours
(Annual percent change)



Notes: All series are demeaned. Sample period is 1949-96. All series cover the non-farm, non-mining private business economy. Growth in aggregate output is measured as real value added. Growth in inputs is measured as the share-weighted average of growth in primary inputs of capital and labor. TFP is measured as output growth minus input growth. Shaded regions show NBER recession dates.

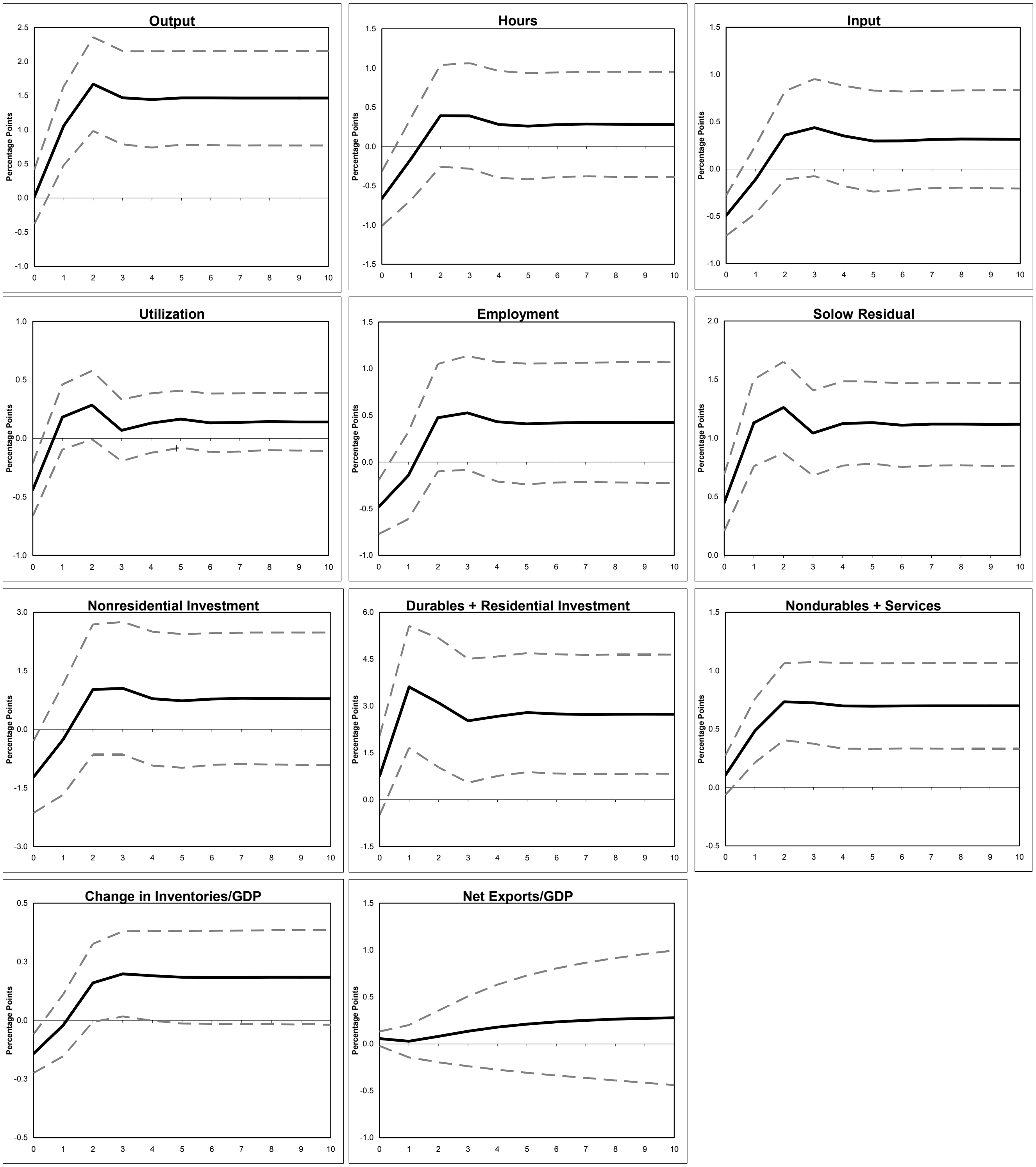
Figure 2. Technology, TFP, Output, Hours, Utilization, and Non-Residential Investment
(Annual percent change)



Notes: All series are demeaned. Sample period is 1949-96. All series cover the non-farm, non-mining 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.

Fig-2-dtcon-4-panel.gwg and investment-and-technology-like-5-panel.gwg

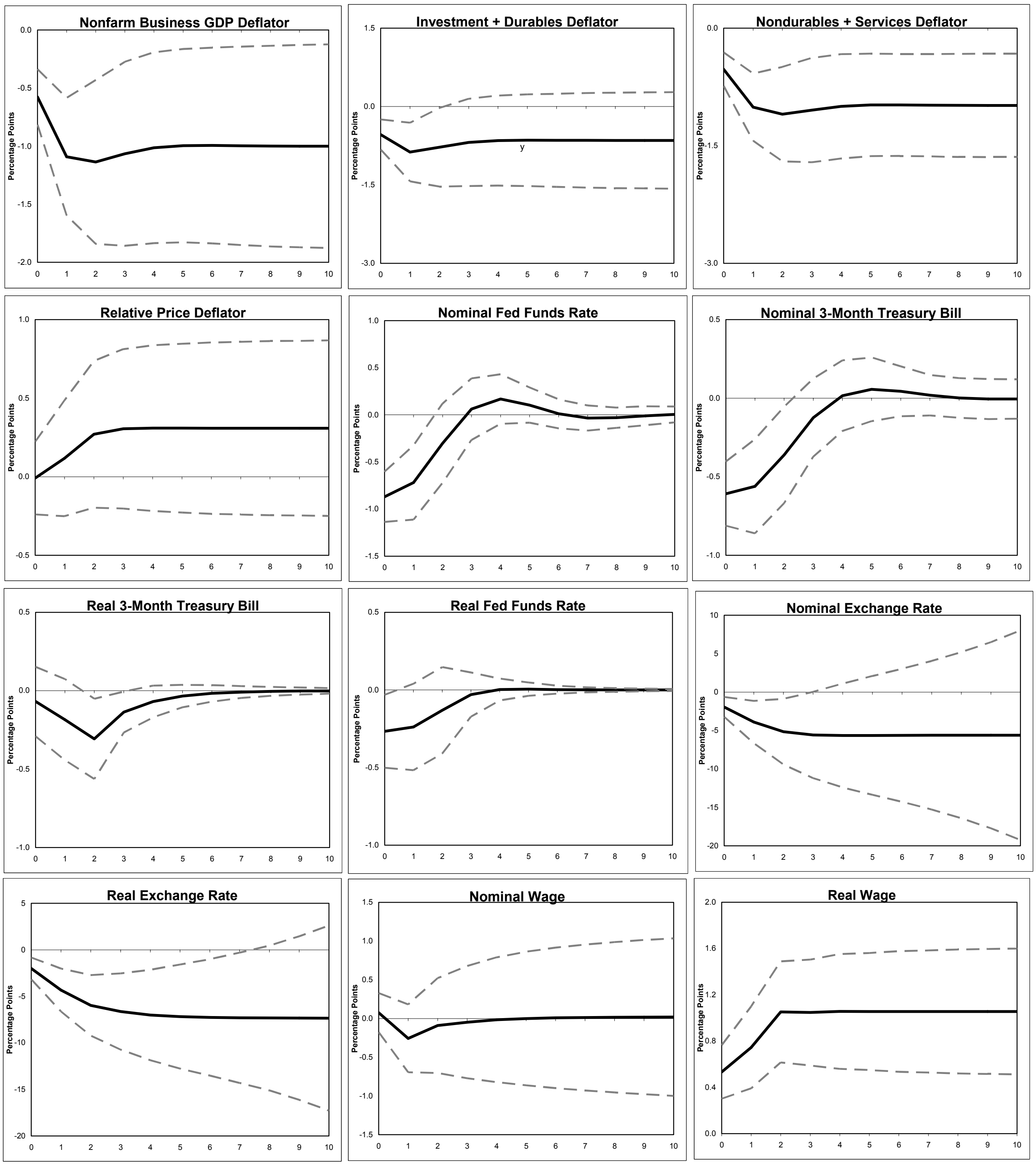
Figure 3. Impulse Responses to Technology Improvement: Quantities



Note: Impulse responses to a 1 percent improvement in "purified" technology, estimated from bivariate VARs where purified technology is taken to be exogenous. All entries are percent changes; horizontal scale represents years after the technology shock. Dotted lines show 95 percent confidence intervals, computed using RATS Monte Carlo method. Sample period is 1952-96.

Figs34_shortrun_IRs_with_dtcon2_5.17.xls

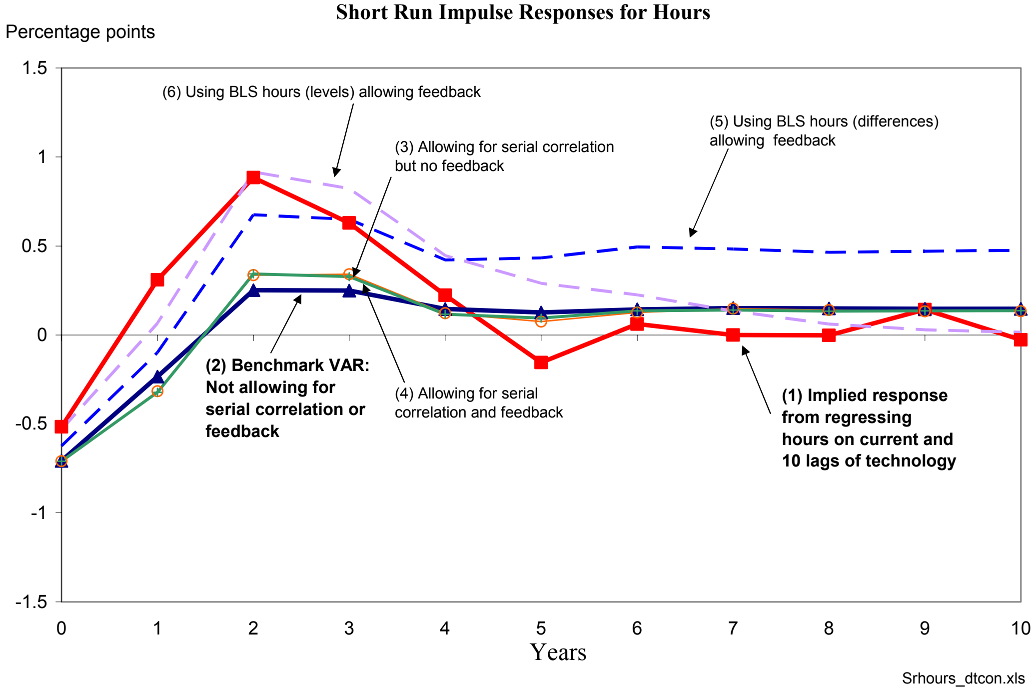
Figure 4. Impulse Responses to Technology Improvement: Prices and Interest Rates



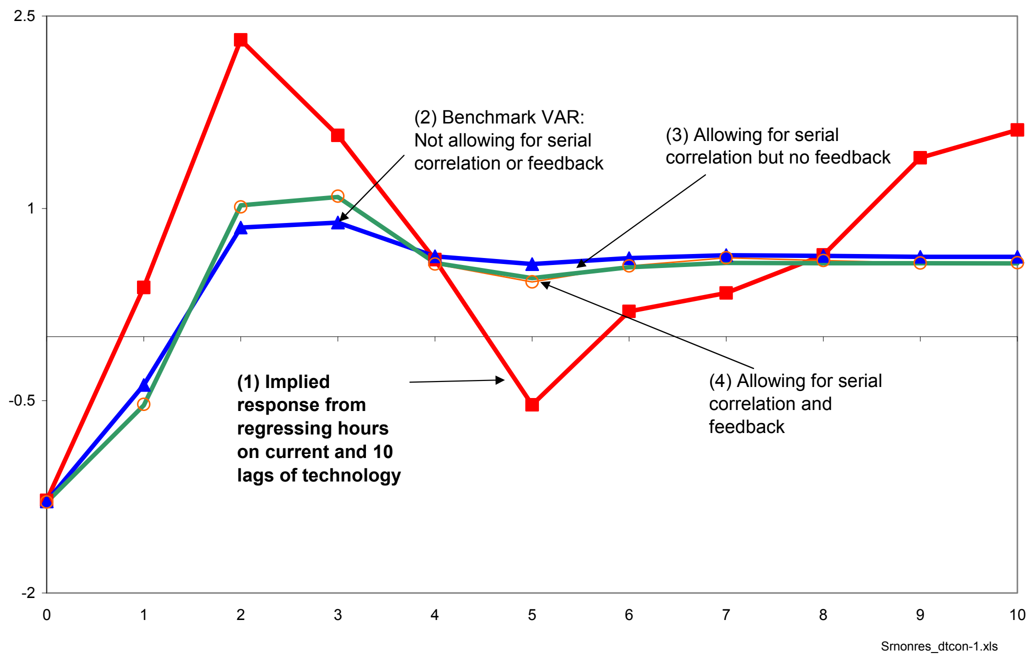
Note: Impulse responses to 1 percent improvement in "purified" technology, estimated from bivariate VARs where purified technology is taken to be exogenous. All entries are percent changes; horizontal scale represents years after the technology shock. VARs for prices, interest rates, and wages include decadal dummy variables for 1970s and 1980s. For nominal and real trade-weighted exchange rate, an increase represents an appreciation. Investment includes residential and non-residential investment; relative price deflator is ratio of deflator for investment (residential and non-residential) and consumer durables to the price deflator for consumer non-durables and services. Dotted lines show 95 percent confidence intervals, computed using RATS Monte Carlo method. Sample period is 1952-96 except for the fed funds rate (1957-96) and real/nominal exchange rate (1973-96). Figs34_shortrun_IRs_with_dtcon2_5.17.xls

Figure 5. Alternative estimates of the hours and investment response to a technology improvement

Short Run Impulse Responses for Hours

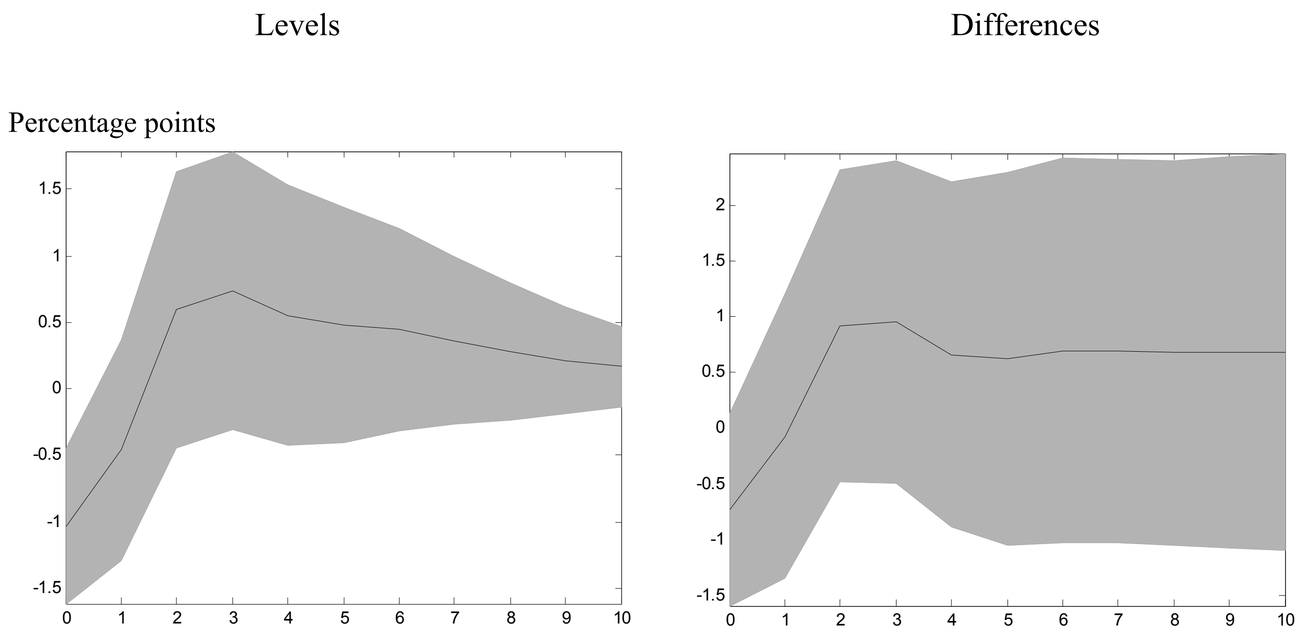


Short Run Impulse Responses for Nonresidential Investment



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 (aged 16 and older).

Figure 6. Estimates from a VAR with long-run restrictions: Hours response to a technology improvement



Notes: Responses identified from the assumption that only "true" technology affects the level of purified technology in the long run. Response shows percentage-point 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 non-farm, non-mining business) divided by the population aged 16 and older. The difference specification uses the growth rate of hours worked per capita. 95 percent confidence interval shown.