Comment on “Bottlenecks: Sectoral Imbalances and the US Productivity Slowdown”

John G. Fernald and Eugenio Piga

Abstract: Acemoglu, Autor, and Patterson’s paper is thought-provoking and full of rich insights. They propose that TFP growth among industry suppliers has become less balanced over time which, in turn, has reduced innovation spillovers. These reduced spillovers became an increasing drag on growth in the 1977-2007 period. In this comment, we first argue that they are silent on the important changes in trend TFP growth that are apparent since World War II—the slowdown after the early 1970s, the speedup in the mid-1990s, and the subsequent slowdown in the mid-2000s. The issue is that the NBER-CES productivity dataset at the core of their analysis looks very different from other datasets. Second, we explore whether factor-share anomalies in their data affect their results; if anything, correcting those anomalies strengthens their apparent results. Nevertheless, given the unusual properties of their data, we hesitate to give it too much weight on its own. Third, we repeat their analysis with higher quality but, unfortunately, less granular, industry data. These results look very different: Unbalanced upstream innovation is, if anything, good for growth. We conclude that the detrimental effects of unbalanced innovation are an intriguing conjecture but fall short of proven fact.

1. Introduction

In his influential AEA Presidential Address in 1998, Arnold Harberger presented a striking mycological metaphor to compare two visions of the growth process: mushrooms versus yeast. As many a lawn owner realizes, mushrooms sprout suddenly, unexpectedly, and unevenly. Yeast, in contrast, causes a loaf of bread to rise smoothly and evenly. Harberger argued that the actual process of productivity growth was inherently uneven and mushroom-like, with selected sectors that grow rapidly and propel the aggregate.

We thought of Harberger's metaphor when we read the intriguing paper by Acemoglu, Autor, and Patterson: “Bottlenecks: Sectoral Imbalances and the US Productivity Slowdown.” The authors argue that a key driver of productivity growth is innovation spillovers from upstream suppliers.

But the spillovers, they find empirically, are maximized when upstream suppliers have a relatively balanced pattern of productivity growth. For example, the likelihood of a car manufacturer successfully innovating may be higher if there are upstream innovations not just in semiconductors, but in glass, metals, and plastics. In other words, if Acemoglu et

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1 Fernald: INSEAD and Federal Reserve Bank of San Francisco. Piga: INSEAD. The views expressed in this note are our own and do not necessarily represent the views of the Federal Reserve Bank of San Francisco, its staff, or others associated with the Federal Reserve System.
al. were social planners organizing dinner parties, they would serve us bread, not sautéed mushrooms.

The nature of innovation spillovers is a potentially important contribution. But the implications, if they hold, may be even more important. Focusing on the 1977 to 2007 time period, Acemoglu et al find that TFP growth among supplying industries has become more mushroom-like over time. This imbalance, they estimate, has led to reduced innovation spillovers which, in turn, provides a novel explanation for a slowdown in productivity.

This is a thought-provoking paper, full of rich insights about the centrally important issues of what drives productivity, and why productivity trends change. Every reader can find something to like, something to wrestle with, something to worry about. The authors have responded to many concerns with copious robustness checks.

In this comment, we first discuss the unusual timing of the productivity slowdown that Acemoglu et al seek to explain. The paper’s abstract highlights the slowdown after the 1970s. The paper itself restricts itself mainly to data from 1977 to 2007 and seeks to explain an apparent productivity slowdown in U.S. manufacturing in the mid-1990s. This is unusual timing: Conventional wisdom as well as a very large existing literature identifies a U.S. productivity speedup in the mid-1990s. After about 2005, however, productivity growth slowed across advanced economies—a slowdown that occurs after the period analyzed in the Acemoglu et al paper.

As we discuss, the reason the authors focus on an apparent mid-1990s slowdown is that the core dataset used in the paper—the NBER-CES six-digit industry dataset—looks anomalous after 1997. But given the 1977-2007 data window used in the paper’s analysis, the results in the paper are, unfortunately, silent on both the 1970s slowdown as well as the mid-2000s slowdown.

Second, we re-estimate Acemoglu et al.’s key equation after making a correction to the factor shares used in the NBER-CES industry data. Our preference would have been for this correction to be the default in the paper; the final version does include this correction in appendix tables. This correction actually appears to strengthen the results in the paper, in that the standard errors are noticeably smaller.

Nevertheless, we remain cautious about putting too much weight on results using the NBER-CES data, given the unusual time-series properties noted above. Whatever causes those time-series anomalies, they could affect the underlying industry TFP variances that drive Acemoglu et al.’s results. The factor-share correction, though an improvement, goes only a small way towards resolving the discrepancies.

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2 The paper brings much more data to bear. Some results add non-manufacturing industries from BLS data; other results use international KLEMS data. But the core analysis focuses on the NBER-CES data because it allows considerable granularity.
Thus, our third point is to re-estimate Acemoglu et al.'s key equation using an alternative, higher quality, dataset. That dataset, which is a joint product of the Bureau of Economic Analysis (BEA) and the Bureau of Labor Statistics (BLS), explicitly follows production theory as laid out in Jorgenson and Griliches (1967) and Jorgenson, Gollop, and Fraumeni (1987). A preliminary exploration with these data suggest that, at a three-digit NAICS level, mushroom-like (uneven) TFP growth is actually good for spillovers. These results thus run counter to those in Acemoglu et al.

An important caveat is that the BEA-BLS data are much more aggregated, so we lose the granularity of the NBER-CES data. This granularity could be important for the spillovers identified in the paper. Nevertheless, given our concerns about the quality of the NBER-CES data, we view the detrimental effects of unbalanced innovation as an intriguing conjecture not as proven fact.

2. The timing of the productivity slowdown

Our first point concerns the anomalous timing of the productivity slowdown addressed by Acemoglu et al. Figure 1 displays the growth of total factor productivity (TFP) in the market economy from 1947 to 2020, using data from the BEA-BLS Integrated Production Accounts. The figure highlights a clear productivity slowdown after the late 1960s or early 1970s. Productivity growth picked up again between 1995 and about 2005. There is a second productivity slowdown that became evident after the mid-2000s.

The slowdown period emphasized in Acemoglu et al. focuses on the period from 1997 to 2007, shown with the vertical lines in the figure. From the perspective of Figure 1, this dating seems surprising: The 1997-2007 period is close to the fastest 10 years of TFP growth since the 1970s! (This period was only a touch slower than the 1995-2005 period.) The remarkable growth coincided with the emergence of the Internet. More broadly, there was a widely documented information-technology-driven transformation across the economy (e.g., Basu, Fernald, Oulton, and Srinivasan, 2004; Fernald, 2015).

3 The historical BEA-BLS Integrated Production Accounts are described in Eldridge, et al. (2020) and run 1947 to 2016; the updated data are described in Bureau of Economic Analysis (2022) and run from 1987 to 2020. We merge the data at 1987. See Fernald, Inklaar, and Ruzic (2023) for details.

4 The previous footnote describes the data. TFP is in value-added terms, and is calculated by aggregating TFP growth across market-sector industries using time-varying Domar weights (nominal gross output relative to market-sector nominal value added).

5 See Fernald, Hall, Stock, and Watson (2017) for a discussion of the timing of apparent breaks in productivity growth.
Figure 1: Market-economy TFP growth, 1947-2020

Source: BEA-BLS Integrated Production Account. TFP is in value-added terms and is constructed by Domar-weighting gross-output TFP growth for market-sector industries. The market economy excludes government, education, health care, and real estate. The figure plots 100 times the cumulated log-change in market-sector TFP since 1947.

The marked decline in market-sector TFP growth occurred only after 2005. Acemoglu et al. end their analysis in 2007. Hence, for all their intriguing analysis, they are largely silent on the important post-2005 slowdown.6

So why do Acemoglu et al. focus on a slowdown from 1997 to 2007? This anomalous timing arises from their core focus on U.S. manufacturing data from the NBER productivity dataset. That dataset is valuable because it provides input and output data for 462 manufacturing industries. But it turns out that, after the mid-1990s, the time-series pattern in that dataset is completely at odds with other, more standard datasets.

Figure 2 compares manufacturing value-added TFP between our benchmark BEA-BLS dataset and the Acemoglu et al. benchmark NBER-CES dataset. The thick solid line shows TFP in the BEA-BLS dataset. It looks qualitatively similar to Figure 1. After the early 1970s, the fastest decade of productivity growth was from 1996-2006; there was a very sharp slowdown after 2007. Indeed, the level of manufacturing TFP has edged down slightly since then.

6 Fernald, Inklaar, and Ruzic (2023) review alternative stories for the advanced-economy TFP slowdown after the mid-2000s, with links to the literature. Their preferred story emphasizes a common trend slowdown in innovation across countries. Such a common trend is consistent with the Acemoglu et al. argument on declining innovation spillovers. Aghion et al. (2022) provide a quite different story about innovation and trend growth that emphasizes the changing returns to innovation.
The thick dashed line shows the sharp difference in the time series pattern that is apparent in the NBER-CES data. The level of TFP roughly tracks the BEA-BLS series up until 1997. But the series diverge during the subsequent decade. The BEA-BLS measure of TFP has its strongest decade of growth since the 1970s. In contrast, NBER-CES measure stagnates after 1997. Although there is considerable volatility, there appears to be almost no TFP growth over the subsequent two decades.

*Figure 2: Manufacturing-economy value-added TFP growth*

![Graph showing manufacturing-economy value-added TFP growth.](image)

Source: BEA-BLS Integrated Production Account and NBER-CES Manufacturing dataset. TFP is constructed by Domar-weighting gross-output TFP growth for manufacturing-sector industries. The figure plots 100 times the cumulated log-change in manufacturing-sector TFP since 1963. The thick solid line shows TFP in the BEA-BLS dataset. The thick dashed line and the thin solid line show TFP in the NBER-CES dataset before and after correcting factor shares.

It is not completely clear what explains the differences in this case. Nevertheless, the NBER-CES dataset suffers some well-known shortcomings relative to standard economic theory à la Jorgenson and Griliches (1967) and Jorgenson, Gollop, and Fraumeni (1987).

First, nominal factor shares in the NBER-CES data are mismeasured. Labor costs exclude fringe benefits, so labor’s share is too low; and intermediate services are omitted, so the intermediate share is too low. The low intermediate share also implies that nominal value added is overstated.

As a consequence, labor’s share is extremely low in the NBER-CES data; residual capital shares are extremely high. In 1997, for example, labor’s share is only 15 percent of gross output and 31 (!) percent of value added. In contrast, in the BEA-BLS dataset, labor’s share is 21 percent of gross output and 58 percent of value added.

Second, real factor inputs in the NBER-CES data are crude relative to state-of-the-art growth-accounting conventions. Production-worker hours are available; but for non-
production and supervisory workers, only the number of employees is available. There is no adjustment for the composition of the labor force by experience or education. Capital is a simple stock measure, not a share-weighted capital-input measure. And as noted, intermediate inputs completely omit services, which become more important over time.

A common correction for the first issue, incorrect factor shares, is to adjust the detailed NAICSs six-digit NBER-CES factor shares using ratios calculated at a three-digit level (e.g., Bils and Chang, 2000). That is, for each three-digit industry, we calculated the ratio of the BEA-BLS labor and intermediate shares relative to the corresponding shares within the three-digit NBER-CES shares. We then apply the same ratio to rescale labor and intermediate factor shares for all six-digit NBER-CES industries that are part of the same three-digit industry. We then aggregate the series to get an adjusted NBER-CES manufacturing value-added TFP series.

The thin solid line in Figure 2 shows the effect of the factor-share adjustment. The adjustment improves the coherence with the BEA-BLS series prior to 1997. The main effect of the adjustment is to give more weight to labor and intermediates relative to capital; this adjustment is particularly important around business cycles, when measured capital is smooth relative to other inputs. After 1997, however, the adjustment goes in the right direction—it raises mean growth—but not by nearly enough to match growth in the BEA-BLS dataset.7

Thus, the bulk of the post-1997 discrepancy between the BEA-BLS and NBER-CES data remains unexplained. A preliminary calculation suggests that, when aggregated across industries, the intermediates-price deflator in the NBER dataset grows more slowly than in the BEA-BLS dataset, so that real intermediate inputs correspondingly grow more quickly. As a result, TFP and real value added grow more slowly. We would emphasize that real value-added in the BEA-BLS manufacturing data match the official national accounts; the NBER-CES data do not.

In sum, the unusual time-series properties of the NBER-CES data leave us somewhat cautious about relying on results using these data. After all, whatever the source of the discrepancy—whether it’s factor shares, deflators, or something else—this discrepancy is built into the underlying industry TFP growth rates and could also affect the variances of industry TFP growth. Those variances are the key input into the Acemoglu et al. empirical specification. We turn to those results now.

7 Mean TFP growth rises more quickly since the corrected shares gives less weight to faster-growing capital. Indeed, labor input in manufacturing was falling after 1997, even as capital kept rising.
3. Refinements to the key empirical results

Our second point is that correcting factor shares in the NBER-CES data makes surprisingly little difference to the estimation results. This may be because, as noted already, correcting the factor shares only modestly changes the dataset’s unusual time-series pattern.\(^8\)

The key estimating equation in Acemoglu et al. is as follows:

\[
\Delta TFP_{it} = \beta_{\text{mean}} \sum_j \alpha_{ijt-1} \Delta TFP_{jt} + \beta_{\text{var}} \left( \Delta TFP_{jt} - \sum_j \alpha_{ijt-1} \Delta TFP_{jt} \right)^2 + \delta_{it-1} + \epsilon_{it}
\]

In this equation, \(\Delta TFP_{it}\) is the five-year growth rate of TFP in industry \(i\); the \(\alpha_{ijt-1}\) are the shares of nominal revenue purchased by industry \(i\) from industry \(j\); \(\delta_{it-1}\) denotes a vector of other covariates; and \(\delta_t\) a full set of time dummies. The first term on the right-hand side is the input-weighted TFP average; the second term (in parentheses) is the input-weighted TFP variance. The main estimates in the paper are uninstrumented, though they do present some results with instruments. We focus here on the OLS results.

The key coefficient is \(\beta_{\text{var}}\), which multiplies the input-TFP variance. A positive coefficient implies that a higher variance of—that is, less balanced—TFP growth among an industry’s suppliers is good for growth. That is, mushroom-like supplier TFP growth is good for innovation spillovers. A negative coefficient implies that mushroom-like growth is bad.

A question raised in the previous section is, how to measure industry TFP in the NBER-CES dataset? There are many potential challenges and pitfalls. For example, non-constant returns, markups, and unobserved variations in factor utilization all mean that measured TFP growth is an imperfect proxy for technology change (e.g., Basu, Fernald, and Kimball, 2006). Time averaging may help—to the extent the non-technological effects are cyclical—but five or even ten years may not be long enough.

We take a simpler approach here. Given that the factor shares used to calculate TFP in the NBER dataset are clearly wrong (underweighting both labor and intermediates), to us the first step—and a natural default—is to correct the shares. Acemoglu et al. instead choose to put those adjustments in appendix tables. This means it is hard for the reader to see all of the implications of adjusting the shares for other findings in the paper.

The first two columns of Table 1 show the benchmark regression results from Acemoglu et al. TFP growth at a six-digit industry is taken directly from the NBER-CES dataset, 

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\(^8\) In our comment on the conference paper, we proposed two adjustments: first, to the factor shares, discussed here; and second, weighting the regression (and Figure 5 of their paper) by nominal gross output rather than fixed 1987 single-deflated value added. The joint effect had a major effect on the implications of the regression results. In the final draft of the paper, Acemoglu et al. have adopted our recommended gross-output weighting, which in our view gives more sensible results. (Single-deflated value added deflates nominal value added by the gross-output deflator; it turns out to have odd properties, as our conference discussion highlighted.) They now include the corrected factor shares that we discuss here as robustness exercises in appendix tables.
without adjusting factor shares. (These correspond to Table 1 of the paper, columns 3 and 7.) In both cases, the coefficient on input-TFP variance is strongly negative: Mushroom-like supplier TFP growth is bad for downstream TFP growth.

In the first column, the regression is unweighted. The coefficient on the variance of input-TFP has a t-statistic of around 8. In the second column, we weight industries based on their 1987 shares of nominal gross output. This sensible weighting reduces the coefficient on the variance of input-TFP by about one third, but the coefficient remains statistically significantly negative.

**Table 1 – Relationship between industry TFP growth and supplier TFP growth using the NBER-CES dataset.**

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
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<tbody>
<tr>
<td>Input-TFP Average</td>
<td>0.65</td>
<td>0.53</td>
<td>0.26</td>
<td>0.31</td>
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<td></td>
<td>(0.074)</td>
<td>(0.13)</td>
<td>(0.089)</td>
<td>(0.097)</td>
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<td>Input TFP Variance</td>
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<td>-0.66</td>
<td>-0.56</td>
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<td>(0.12)</td>
<td>(0.20)</td>
<td>(0.10)</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
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<td>Nom. Sales</td>
<td>None</td>
<td>Nom. Sales</td>
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<td>2772</td>
<td>2772</td>
<td>2772</td>
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<tr>
<td>R-squared</td>
<td>0.37</td>
<td>0.60</td>
<td>0.36</td>
<td>0.61</td>
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</tbody>
</table>

Notes: This table reports estimates of equation (1) (equation (6) in Acemoglu et al.) for 462 NAICS six-digit manufacturing industries from 1977-2007. The left-hand-side variable is the non-overlapping five-year growth rate of industry TFP (so six five-year periods from 1977-2007). The input-TFP average and variance are also calculated over the corresponding five-year periods. Columns 1 and 2 correspond to columns 3 and 7 of Table 1 of the paper; columns 3 and 4 correspond to columns 3 and 7 of Table A4 of the paper. Columns 1 and 3 report unweighted OLS regressions, columns 2 and 4 use the industry’s 1987 share of shipments as weights. Time and industry dummies are included in all regressions. Industries are defined using 1997 NAICS codes. Standard errors (shown in parentheses) are clustered at the industry level.

The third and fourth columns incorporate the factor-share corrections discussed above. In the unweighted specification (column (3)), this correction leads to a notable reduction in the magnitude of the coefficient on the input-TFP variance relative to column (1). In the weighted specification, the correction leads to a more modest reduction in the magnitude of the coefficient relative to column (2). (These regressions correspond to Table A4 of the paper, columns 3 and 7.)

Strikingly, the standard error also declines noticeably: the t-statistic actually increases when using the adjusted factor shares. This underscores the resilience of their results when using the NBER-CES dataset. It also underscores why we would have preferred to
see the factor-share adjusted TFP series taken as the benchmark throughout the paper, since it actually strengthens their empirical findings.

But of course, the magnitude of the coefficient itself is not easily interpretable. Hence, the paper (Figure 5) undertakes a counterfactual exercise to see, if the input-TFP variances didn’t change from their 1977-87 values, what would productivity have been in the subsequent decades? (The counterfactual holds average supplier-weighted TFP growth fixed at their actual values, so that only the input-TFP variance changes from the actual values.)

Figure 3: Magnitude of Bottleneck Estimates

Notes: The left panel reproduces the results from the paper’s Figure 5 for manufacturing. On the right panel, counterfactual TFP (red bar) is computed from our adjusted (nominal sales-) weighed estimates reported in column 4 of Table 1 and represents the TFP growth that would have been observed in the given period if the variance of TFP growth had remained at the same level as during the initial period (1977-1987).

The left panel of Figure 3 reproduces the results from the paper’s Figure 5 for manufacturing. Relative to the paper’s figure, we present results in terms of annualized percent changes and show just the bars for actual and counterfactual manufacturing TFP growth. The blue bars show the sharp slowdown in (gross output) manufacturing TFP growth after 1997. The red counterfactual bars show that, if the variances of input-TFP had been held fixed at their 1977-87 level, there would still have been a slowdown, but it would have been somewhat attenuated. In the 1997-2007 period, the results imply that the increase in input-TFP variance was a drag of about 0.3 percentage points per year to manufacturing TFP growth.

The right panel shows our preferred version of the figure, which incorporates the factor-share adjustments. The biggest difference is that the “actual” bar does not show a slowdown in the 1997-2007 period relative to the preceding decade. That is consistent with Figure 2, where adjusting factor shares (modestly) raises TFP after 1997 (though not enough to close the gap with the BEA-BLS or other datasets). It turns out, however, that the gap between the counterfactual and the actual bar is almost identical to that shown in
the left panel. In principle, despite a coefficient that is only modestly attenuated, the gap could have changed more significantly because the underlying industry TFP variances are also affected. But this turns out not to be the case.

4. Empirical analysis using the integrated BEA-BLS dataset.

Although the results using conventional TFP with the NBER-CES data do seem robust to correcting factor shares, we started this discussion by raising general concerns about that source of data. Hence, our final point is to see if we can replicate the results in a different, higher quality dataset. It turns out we cannot.

In particular, when we repeat the analysis using the BEA-BLS industry dataset, the coefficient on the input-TFP variance is either strongly positive (in manufacturing) or positive but insignificant (in all industries). That is, unbalanced, mushroom-like input-TFP growth appears either positive for spillovers, or unimportant.\(^9\)

Table 2 shows these results. For comparability, we focus on the same 1977-2007 period that Acemoglu et al. do. Column 1 restricts the analysis to 19 manufacturing industries; column 2 considers the 55 industries that span the market economy. These results consistently show that higher input-TFP variance is associated with stronger industry TFP growth. This finding contrasts with the evidence from the paper, and from Table 1 above, which focused on the NBER-CES data.

It is not clear what explains the difference. The BEA-BLS dataset is higher quality, in that it measures inputs and factor shares consistently with economic theory; in addition, the data match the national accounts, which incorporate numerous quality-control checks. At the same time, the data are much more aggregated. The reduced granularity may matter for the results.

Nevertheless, given the higher quality of the BEA-BLS data as well as the uncertain quality of the NBER-CES data, these results give us some pause. Our own take is that the paper’s finding in favor of balanced growth remains a hypothesis rather than a proven fact.

\(^9\) The coefficients are much larger in magnitude than those shown in Table 1. The reason is that, in the three-digit BEA-BLS industry data, the input-TFP variances (the right-hand-side regressors) are much smaller. This could be because the data smooth over the sometimes extreme six-digit TFP variances.
Table 2: Relationship between industry TFP growth and suppliers’ TFP growth using the BEA-BLS dataset.

<table>
<thead>
<tr>
<th></th>
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<tbody>
<tr>
<td>Input Average</td>
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<td>(0.100)</td>
<td>(0.145)</td>
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<td>Input Variance</td>
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<td>3.124</td>
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<td>(7.846)</td>
<td>(5.166)</td>
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<tr>
<td>Methodology</td>
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</tr>
<tr>
<td>Sample</td>
<td>Manufacturing</td>
<td>Market</td>
</tr>
</tbody>
</table>

Notes: This table reports estimates of equation (1) (equation (6) in Acemoglu et al.) from 1977-2007 using the BEA-BLS Integrated Production Account. The dependent variable is an industry’s TFP growth in a (non-overlapping) five-year period and the two right-hand side variables are mean and variance of TFP growth among industry’s suppliers, calculated over the same five-year period. All columns use the industry’s share of shipments as weights. Time and industry dummies (corresponding to linear industry trends) are included in all regressions. Columns 1 and 2 report OLS regression for manufacturing and market-sector industries, respectively. Industries are defined using 1997 NAICS codes. Standard errors shown in parentheses.

5. Conclusions

The Acemoglu et al paper proposes a new hypothesis about the nature of spillovers across firms and industries. They argue that unbalanced, mushroom-like growth reduces innovation spillovers. And because the variance of upstream TFP appears to have increased over time, they find that unbalanced innovation was an increasing drag on TFP growth.

The hypothesis, and the empirical results, are intriguing and thought-provoking. Nevertheless, we remain cautious about this finding. The main dataset that yields this finding—the NBER-CES dataset—looks quite different from other, higher quality datasets. We don’t yet know why. That leaves us hesitant about drawing strong conclusions from these data. This is especially the case given that a higher quality, but more aggregated, dataset gives quite different results.

One point we have not addressed is that TFP growth is an imperfect proxy, even after correcting the NBER-CES factor shares. For example, markups, non-constant returns, shifts in factor utilization, and reallocations within industries or sectors affect TFP growth and the variance of TFP growth. Such effects could easily be correlated through input-output linkages and, thereby, cause biases in the results. Jennifer La’O and Eugenio Piga’s discussion of Acemoglu et al., in this volume, addresses some of these more nuanced and challenging measurement issues.

What this means is that the Acemoglu et al. paper is only a preliminary step in establishing the nature of innovation spillovers through input-output linkages. Subsequent work needs to establish the degree to which the spillovers exist as well as whether the nature of these
spillovers can contribute to our understanding of productivity speedups and slowdowns. Hence, our main takeaway is that there is much more work to be done.
6. References


