Firm Leverage and Regional Business Cycles*

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Abstract

This paper shows that build-ups in *firm* leverage predict subsequent declines in regional employment. Using confidential establishment-level data from the U.S. Census Bureau, we exploit regional heterogeneity in leverage build-ups by large U.S. publicly listed firms, which are widely spread across U.S. regions. For a given region, our results show that increases in firms' borrowing are associated with "boom-bust" growth cycles: employment grows in the short run but declines in the medium run. Across regions, our results imply that regions with larger build-ups in firm leverage exhibit stronger short-run growth, but also stronger medium-run declines, in regional employment. Altogether, our results suggest that the geography of U.S. publicly listed firms' operations plays an important role for regional growth cycles.

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1 Introduction

Large U.S. firms are widely spread across regions. During the 1976–2011 period, the average U.S. publicly listed firm owned establishments in 32.3 counties, 19.9 MSAs, and 8.1 states. When these large firms increase their borrowing, regions are differentially impacted. As this paper shows, regions with larger build-ups in leverage by U.S. publicly listed firms exhibit stronger aggregate regional employment growth in the short run. However, this employment growth is only temporary. In the medium run, regions with larger build-ups in leverage by U.S. publicly listed firms experience stronger *declines* in aggregate regional employment. For a given region, this implies that increases in firms' borrowing are associated with "boom-bust" growth cycles: employment grows in the short run but declines in the medium run. Across regions, our results imply that regions with larger build-ups in firm leverage experience stronger short-run growth, but also stronger medium-run declines, in aggregate regional employment.[1] Altogether, our results suggest that the geographical distribution of U.S. publicly listed firms' operations plays an important role for regional growth cycles.[2]

We measure regional exposure to build-ups in leverage by U.S. publicly listed firms by computing the weighted average leverage ratio of U.S. publicly listed firms operating in a region ("regional firm leverage"). Weights are based on firms' shares of regional employment. We construct regional employment shares for all U.S. publicly listed firms using confidential data from the U.S. Census Bureau's Longitudinal Business Database (LBD), which provides information on employment, location, and firm affiliation for all U.S. business establishments. Firm leverage is obtained from Compustat. Our main regional analysis is at the county level. The sample period is from 1976 to 2011.

[1] Increases in leverage bring about fragility and vulnerability to shocks (e.g., Bernanke and Gertler 1989; Kiyotaki and Moore 1997; Bernanke, Gertler, and Gilchrist 1999; Fostel and Geanakoplos 2008; Brunnermeier and Sannikov 2014) and changes in beliefs (Minsky 1977; Kindleberger 1978; Gennaioli, Shleifer, and Vishny 2015; Bordalo, Gennaioli, and Shleifer 2018). Giroud and Mueller (2017) provide empirical evidence showing that firms with higher leverage in 2006, at the onset of the Great Recession, are more sensitive to local consumer demand shocks during the Great Recession.

[2] The geography of firms' operations also plays a central role in Giroud and Mueller (2018). The authors show that consumer demand shocks during the Great Recession propagate across U.S. regions through firms' internal (geographical) networks of business establishments.

We first study the effects of changes in regional firm leverage on total employment by U.S. publicly listed firms in a given region. Over a three-year period, a one standard deviation increase in regional firm leverage is associated with a 3.8 percent increase in regional employment by U.S. publicly listed firms. In the medium run, however, an increase in regional firm leverage predicts a subsequent *decline* in employment by U.S. publicly listed firms in a given region: a one standard deviation increase in regional firm leverage from $t-3$ to t predicts at decline in employment of 1.8 percent from t to $t+3$, 3.0 percent from $t+1$ to $t+4$, and 2.7 percent from $t+2$ to $t+5$.

Employment changes at U.S. publicly listed firms in a region may be irrelevant if they are offset by employment changes at non-listed firms. Specifically, layoffs at U.S. publicly listed firms may trigger a decline in regional wages and subsequent hiring by non-listed firms in the same region, with the effect that aggregate regional employment remains unchanged. To examine whether, and to what extent, employment changes at U.S. publicly listed firms are "passed through" at the regional level, we consider total employment by *all* (i.e., listed and non-listed) firms in a given region. We find that over a three-year period, a one standard deviation increase in regional firm leverage—the employment-weighted average leverage ratio of U.S. publicly listed firms operating in a region—is associated with a 0.5 percent increase in aggregate regional employment. Importantly, a one standard deviation increase in regional firm leverage from $t-3$ to t predicts a subsequent *decline* in aggregate regional employment of 0.3 percent from t to $t+3$, 0.4 percent from $t+1$ to $t+4$, and 0.4 percent from $t+2$ to $t+5$.

A comparison of the estimates shows that the pass-through from changes in regional employment at U.S. publicly listed firms to changes in aggregate regional employment is nearly perfect. There are no offsetting employment changes at non-listed firms. If anything, there is a small *positive* spillover, or multiplier, effect on non-listed firms in the same region—especially in the non-tradable sector—although it is mostly insignificant. Accordingly, as a rough approximation, a one percent decline in regional employment at U.S. publicly listed firms translates into a 0.128 percent decline in aggregate regional employment, where 0.128 is the average regional (precisely: county-level) employment share of U.S. publicly listed firms.

While our main regional analysis is at the county level, we obtain similar results if we use broader regions based on MSAs or states. Likewise, our results are similar if we conduct separate analyses for tradable, non-tradable, and other industries. In each of these industries, increases in leverage by U.S. publicly listed firms operating in a region predict declines in aggregate (sectoral) regional employment. Finally, we obtain similar results if we condition on national recessions during our sample period (1980–82, 1990–91, 2001, 2007–09). In each of these recessions, regions which experienced larger build-ups in leverage by U.S. publicly listed firms before the recession also experienced larger declines in aggregate regional employment during the recession.

A main empirical concern is that our results could be driven by regional shocks. These shocks could lead to employment changes in the short run, which mean-revert in the medium run. We believe that this is rather unlikely. First, if our results were driven by regional shocks, then we would expect that regional employment by non-listed firms responds as well. But it does not. Second, our results hold even if we *control* for short-run employment changes, though, as we argue, this may be "overcontrolling." Indeed, the effects of changes in regional firm leverage from $t-3$ to t on changes in aggregate regional employment from t to $t+3$, $t+1$ to $t+4$, or $t+2$ to $t+5$ are only slightly weaker if we control for changes in aggregate regional employment from $t-3$ to t.[3] Lastly, our results are similar if we consider employment changes at individual establishments of U.S. publicly listed firms in a setting that includes region $\times$ industry $\times$ year fixed effects. Accordingly, we compare individual establishments in the same region, industry, and year—which are likely exposed to the same regional shocks—that belong to U.S. publicly listed firms with different changes in leverage from $t-3$ to t.

Another possible concern is that our results could be driven by a spurious correlation between changes in firm and household leverage. Prior research has shown that increases in household debt at the country level predict lower subsequent country-level growth (Mian, Sufi, and Verner 2017). In our sample, changes in leverage by U.S. publicly

[3] This also suggests that declines in employment from t to $t+3$, $t+1$ to $t+4$, or $t+2$ to $t+5$ are not simply driven by excessive employment growth from $t-3$ to t. Rather, increases in firm leverage from $t-3$ to t have separate predictive power for subsequent declines in employment.

listed firms and changes in household leverage are practically uncorrelated at the regional level ($\rho = 0.015$). Accordingly, our estimates remain similar if we control for changes in household leverage. Indeed, we find qualitatively similar patterns for both firm and household leverage, suggesting that the predictability of downturns after increases in leverage is a relatively broad phenomenon. Our results are consistent with a supply-side view of leverage growth, in the sense that build-ups in firm leverage in our empirical framework are plausibly unrelated to regional shocks.[4]

Our study informs the debate about the role of credit growth. Theory suggests that increases in borrowing bring about fragility, debt overhang, and vulnerability to shocks and changes in beliefs (see footnote 1). Schularick and Taylor (2012), using data from 14 developed countries from 1870 to 2008, show that credit growth is a powerful predictor of financial crises. Jordà, Schularick, and Taylor (2013, 2017) extend these results by showing that more credit-intensive booms are followed by deeper (normal or financial) recessions. Baron and Xiong (2017), using data from 20 developed countries from 1920 to 2012, show that increases in the ratio of credit to GDP predict future bank equity crashes. Mian, Sufi, and Verner (2017), using data from 30 countries from 1960 to 2012, show that increases in the ratio of household debt to GDP are associated with short-run increases but medium-run declines in GDP. Hence, increases in household debt generate "boom-bust" growth cycles. The authors also find that increases in the ratio of non-financial firm debt to GDP do *not* generate "boom-bust" growth cycles. Moreover, they have only weak predictive power for medium-run GDP growth.[5]

All of the papers cited above use country-level analyses. At the country level, it is difficult to separate the effects of country-level shocks—which may affect both short- and medium-run GDP growth, and possibly also credit growth—from the effects of credit growth. For example, monetary policy (i.e., interest rate changes) may affect both GDP

[4]Sixty years after the seminal works of Modigliani and Miller, the question of how firms choose their capital structure remains the subject of ongoing research. Graham and Harvey (2002) provide evidence from CFO surveys. Frank and Goyal (2009) are a classic empirical reference. Graham and Leary (2011) review the empirical literature on capital structure research.

[5]Other empirical studies use credit *spreads*—as opposed to credit growth—to predict future economic activity (e.g., Gilchrist and Zakrajšek 2012; López-Salido, Stein, and Zakrajšek 2017).

growth and credit growth. Our empirical study attempts to overcome this difficulty using regional analysis. As Nakamura and Steinsson (2018) point out, the use of regional analysis entails two main advantages. First, the number of observations is multiplied by an order of magnitude or more. Second, identification is cross-sectional, based on heterogeneity in regional exposure to a given foreign shock. For example, Autor, Dorn, and Hanson (2013) exploit variation in regional exposure to import competition from China, Nakamura and Steinsson (2014) exploit regional differences in the sensitivity to national military build-ups, and Beraja et al. (2018) exploit regional variation in the sensitivity to interest rate cuts and thus monetary policy. Our paper exploits regional heterogeneity in leverage build-ups by U.S. publicly listed firms to study the implications of firm leverage growth for aggregate regional employment growth. Importantly, as we noted above, build-ups in firm leverage in our empirical setting are unlikely driven by regional shocks.[6] Moreover, they are practically uncorrelated with changes in household debt at the regional level, allowing us to separate the two effects.

Finally, our paper adds to the literature on regional spillover effects (e.g., Glaeser and Gottlieb 2009; Greenstone, Hornbeck, and Moretti 2010; Moretti 2010, 2014; Bernstein et al. 2018; Huber 2018). While we find that the pass-through from changes in regional employment at U.S. publicly listed firms to changes in aggregate regional employment is near perfect *on average*, we find significant spillover effects on non-tradable firms in regions with especially large employment shares by U.S. publicly listed firms. Thus, the incidence and magnitude of regional spillover effects depends on the relative significance of U.S. publicly listed firms for regional economic activity.

The rest of this paper is organized as follows. Section 2 describes the data, variables, empirical specification, and summary statistics. Section 3 studies the relation between changes in regional firm leverage, regional employment growth at U.S. publicly listed firms, and aggregate regional employment growth. Section 4 accounts for the possibility of regional shocks. Section 5 studies changes in regional firm leverage and changes in regional household leverage. Section 6 concludes.

[6] A county is relatively "small" from an individual firm's perspective. For example, in the non-tradable sector, firm-county employment is only 1.7 percent of total firm-level employment.

2 Data, Methodology, and Summary Statistics

2.1 Data and Variables

Our main data source is the Longitudinal Business Database (LBD) provided by the U.S. Census Bureau. The LBD contains information on employment, payroll, location, industry, and firm affiliation for all business establishments in the U.S. with at least one paid employee. An establishment is a "single physical location where business is conducted" (Jarmin and Miranda 2002; p. 5), e.g., a restaurant, department store, or manufacturing plant. Our sample period is from 1976 to 2011.[7]

We match individual establishments in the LBD to firms in Compustat using the Compustat-SSEL bridge maintained by the U.S. Census Bureau. Given that this bridge ends in 2005, we extend the match to 2011 using employer name and ID number (EIN) following the procedure described in McCue (2003). We exclude utilities, financial firms, and firms with missing financial data.

Our main regional analysis is at the county level, albeit we also conduct analyses at the MSA and state level. We obtain county-level employment for both publicly listed and non-listed firms in a county by aggregating employment across the corresponding establishments. To obtain a measure of firm leverage at the county level, we compute the weighted average leverage ratio across all publicly listed firms with establishments in a given county. Weights are based on the firms' county-level employment shares:

$$\text{Lev}_{k,t} = \frac{\sum_j w_{j,k,t} \text{ Lev}_{j,t}}{\sum_j w_{j,k,t}}, \tag{1}$$

where $\text{Lev}_{k,t}$ is firm leverage in county k and year t, $w_{j,k,t}$ is firm j's employment share in county k and year t, and $\text{Lev}_{j,t}$ is the leverage ratio of firm j in year t.[8] Leverage is the ratio of the sum of debt in current liabilities (Compustat item DLC) and long-term debt (item DLTT) to assets (item AT) and is winsorized between zero and one. We proceed

[7] 1976 is the first available year in the LBD. 2011 is the last available year in our Census data project.

[8] The weight $w_{j,k,t}$ represents firm j's share of total employment in county k—not county k's share of total employment by firm j. Accordingly, the sum $\sum_j w_{j,k,t}$ represents the employment share of U.S. publicly listed firms in county k.

analogously when computing firm leverage at the MSA and state level.

We also use data on household debt at the county level from the Consumer Credit Panel provided by the Federal Reserve Bank of New York. Household debt is mortgage, credit card, and auto loan debt normalized by adjusted gross income (from IRS data).

2.2 Empirical Specification

We study the dynamic relation between changes in regional firm leverage—the weighted average leverage ratio across all U.S. publicly listed firms operating in a region—and regional employment growth. We estimate the following equation:

$$\Delta \log(\text{Emp})_{k,t}(t+\tau, t+\tau+3) = \alpha_k + \alpha_t + \beta \, \Delta \, \text{Lev}_{k,t}(t-3, t) + \varepsilon_{k,t}, \qquad (2)$$

where $\tau = -3, \ldots, 2$; $\Delta \log(\text{Emp})_{k,t}(t+\tau, t+\tau+3)$ is employment growth in county k from $t+\tau$ to $t+\tau+3$; Δ $\text{Lev}_{k,t}(t-3, t)$ is the change in firm leverage in county k between $t-3$ and t; and α_k and α_t are county and year fixed effects. In Table 2, we estimate equation (2) for all $\tau = -3, ..., 2$, resulting in six regressions. For example, when $\tau = -3$, the coefficient β captures the short-run effects of changes in regional firm leverage from $t-3$ to t on regional employment growth from $t-3$ to t. As we increase τ, we move towards medium-run effects. For example, when $\tau = 1$, the coefficient β captures the effects of changes in regional firm leverage from $t-3$ to t on subsequent regional employment growth from $t+1$ to $t+4$. In Table 3 and all following tables, we focus exclusively on the predictability of medium-run regional employment growth ($\tau = 0, 1, 2$). For simplicity, we write $\Delta \log(\text{Emp})(-3, 0)$, $\Delta \log(\text{Emp})(-2, 1)$, etc., in lieu of $\Delta \log(\text{Emp})_{k,t}(t-3, t)$, $\Delta \log(\text{Emp})(t-2, t+1)$, etc. Similarly, we write Δ $\text{Lev}(-3, 0)$ in lieu of Δ $\text{Lev}_{k,t}(t-3, t)$. Observations are weighted by county-level employment. Standard errors are double clustered at the county and year level.

In Table 8, we examine the dynamic relation between changes in firm leverage and employment growth at the individual establishment level. We estimate:

$$\Delta \log(\text{Emp})_{i,j,k,l,t}(t+\tau, t+\tau+3) = \alpha_k \times \alpha_l \times \alpha_t + \beta \, \Delta \, \text{Lev}_{j,t}(t-3, t) + \varepsilon_{i,j,k,l,t}, \qquad (3)$$

where Δ $\log(\text{Emp})_{i,j,k,l,t}(t+\tau, t+\tau+3)$ is the employment growth of establishment i of firm j in county k and industry l from $t+\tau$ to $t+\tau+3$, Δ $\text{Lev}_{j,t}(t-3,t)$ is the change in leverage of firm j between $t-3$ and t, and $\alpha_k \times \alpha_l \times \alpha_t$ are county $\times$ industry $\times$ year fixed effects. Accordingly, equation (3) compares the employment growth of establishments in the same county, industry, and year that belong to firms with different leverage changes from $t-3$ to t. Industries are measured at the 4-digit NAICS code level. Observations are weighted by establishment-level employment. Standard errors are double clustered at the county and year level.

2.3 Summary Statistics

Table 1 provides summary statistics. Panel (A) provides firm-level summary statistics for all firms, publicly listed firms, and non-listed firms. As one would expect, publicly listed firms are much larger than non-listed firms—they have more employees and more establishments. Indeed, the typical non-listed firm is a local firm consisting of (little more than) a single establishment. In contrast, the typical publicly listed firm owns 85.5 establishments in 63.6 ZIP codes, 32.3 counties, 19.9 MSAs, and 8.1 states. The average leverage ratio of publicly listed firms is 0.261. And while there are many ups and downs in firms' leverage ratios during our sample period, the *average* three-year change in firm leverage is -0.002 and therefore close to zero.

Panel (B) provides summary statistics at the county level. The average three-year employment growth during the sample period is 5.4 percent for all firms, 4.1 percent for publicly listed firms, and 6.2 percent for non-listed firms. Publicly listed firms account for 12.8 percent of total county-level employment. That being said, the county-level employment share of publicly listed firms varies considerably across industry sectors. It is 13.2 percent in the non-tradable sector, 26.9 percent in the tradable sector, and 7.1 percent in the "other" sector.[9] In robustness checks, we perform separate analyses for each industry sector. The average firm leverage ratio at the county level is 0.288,

[9] Our classification of tradable and non-tradable industries follows Mian and Sufi (2014). Tradable industries are essentially manufacturing industries. Non-tradable industries are restaurant and retail industries. "Other" industries are those that are neither tradable nor non-tradable.

which differs slightly from the corresponding ratio at the firm level due to the uneven geographical distribution of publicly listed firms. The average three-year change in firm leverage at the county level is (again) close to zero, and its standard deviation is 0.08.

3 Firm Leverage and Regional Employment Growth

3.1 Bin Scatterplots

Figure 1 shows bin scatterplots depicting the relation between changes in regional firm leverage and regional employment growth at U.S. publicly listed firms (top panel) or aggregate regional employment growth (bottom panel). The left plots depict the relation between Δ Lev$(-3, 0)$ and Δ log(Emp)$(-3, 0)$ based on 99,300 county-year observations. The right plots depict the relation between Δ Lev$(-3, 0)$ and Δ log(Emp)$(1, 4)$ based on 86,500 county-year observations.[10] Thus, the left plots depict the short-run effects of changes in regional firm leverage, while the right plots depict medium-run effects. For a given percentile bin, the plots show the mean values of Δ Lev$(-3, 0)$ and either Δ log(Emp)$(-3, 0)$ or Δ log(Emp)$(1, 4)$.

In the top left plot, an increase in regional firm leverage is positively associated with short-run regional employment growth at U.S. publicly listed firms. The magnitude of the effect is large: a one standard deviation increase in regional firm leverage (0.08) is associated with a 3.5 percent short-run increase in regional employment at U.S. publicly listed firms ($0.08 \times 0.440 = 0.035$). In the medium run, however, this positive relation becomes negative. In the top right plot, a one standard deviation increase in regional firm leverage predicts a 4.3 percent medium-run *decline* in regional employment at U.S. publicly listed firms. The two bottom plots, which are based on aggregate regional employment, look similar, except that the magnitudes are much smaller. In the bottom left plot, a one standard deviation increase in regional firm leverage is associated with a 0.8 percent short-run increase in aggregate regional employment, while in the bottom right plot, it is associated with a 0.5 percent medium-run decline in aggregate regional

[10] All sample sizes are rounded to the nearest hundred following Census Bureau disclosure guidelines.

employment. All of these figures are based on raw data; hence they must be interpreted with caution. In our regression analysis, which includes county and year fixed effects, we will see that the pass-through from changes in regional employment at U.S. publicly listed firms to changes in aggregate regional employment is roughly proportionate to the average regional employment share of U.S. publicly listed firms.

Table A.1 of the Online Appendix provides the regressions that correspond to the bin scatterplots in Figure 1. While these regressions do not include county or year fixed effects—in accord with Figure 1—they are based on the full sample of 99,300 or 86,500 county-year observations rather than percentile bins. As it turns out, the regression coefficients associated with Δ Lev$(-3,0)$ are remarkably similar to the corresponding slope coefficients in Figure 1 (0.481 vs. 0.440; -0.526 vs. -0.541; 0.097 vs. 0.096; -0.069 vs. -0.064). Three of the four regression coefficients are significant at the one percent level; one is significant at the five percent level. Accordingly, the bin scatterplots in Figure 1 provide an adequate representation of the raw data.

3.2 Main Results

Table 2 presents our main results based on estimating equation (2) for $\tau = -3, ..., 2$. This yields six regressions with dependent variables ranging from short-run ($t-3$ to t) to medium-run (t to $t+3, ..., t+2$ to $t+5$) employment growth. Panel (A) shows the effects of changes in regional firm leverage on regional employment growth at U.S. publicly listed firms. Inspection of all six columns shows that the effects are positive in the short run but negative in the medium run. The sign switches around ($t-2$ to $t+1$), which is why the coefficient in column (2) is small and insignificant. All other coefficients are significant. In particular, the short-run coefficient in column (1) and the medium-run coefficient in column (5) are both significant at the one percent level. The magnitudes of the effects are large. In column (1), a one standard deviation increase in regional firm leverage is associated with a 3.8 percent short-run increase in regional employment at U.S. publicly listed firms, whereas in column (5), it predicts a 3.0 percent medium-run *decline* in regional employment at U.S. publicly listed firms.

Employment changes at U.S. publicly listed firms in a region may be irrelevant if they are offset by employment changes at non-listed firms. To see whether employment changes at U.S. publicly listed firms are "passed through" at the regional level, we study the effects of changes in regional firm leverage on *aggregate* regional employment, i.e., employment by all (listed and non-listed) firms in a region. The results are shown in Panel (B). As can be seen, they are similar to those in Panel (A). The short-run effects are again positive, the sign switches again around ($t-2$ to $t+1$), and the medium-run effects are again negative. Like in Panel (A), the short-run coefficient in column (1) and the medium-run coefficient in column (5) are both significant at the one percent level. That being said, the magnitudes are much smaller than those in Panel (A). In column (1), a one standard deviation increase in regional firm leverage is associated with a 0.5 percent short-run increase in aggregate regional employment, whereas in column (5), it predicts a 0.4 percent medium-run decline in aggregate regional employment. Indeed, both magnitudes are approximately equal to the corresponding magnitudes in Panel (A) multiplied by 0.128—the average regional employment share of U.S. publicly listed firms (note: $3.8 \times 0.128 = 0.5$ and $3.0 \times 0.128 = 0.4$). The same is also true, with minor nuances, for the other columns in Panel (B). We will examine the pass-through from changes in regional employment at U.S. publicly listed firms to changes in aggregate regional employment in more detail in the following section.

In sum, our results show that within a region, increases in U.S. publicly listed firms' borrowing are associated with "boom-bust" growth cycles: regional employment grows in the short run but declines in the medium run. Across regions, our results imply that regions with larger build-ups in firm leverage experience stronger short-run growth, but also stronger medium-run declines, in regional employment. In the remainder of this paper, we focus on the predictability of regional employment declines in the medium run (t to $t+3$, $t+1$ to $t+4$, and $t+2$ to $t+5$).

3.3 Pass-Through

Employment losses at U.S. publicly listed firms may trigger a decline in regional wages and subsequent hiring by non-listed firms in the same region, with the effect that aggregate regional employment remains unchanged. Our results in Table 2 suggest that there are no offsetting employment changes at non-listed firms in the same region: a one percent decline in regional employment at U.S. publicly listed firms translates approximately into a 0.128 percent decline in aggregate regional employment, where 0.128 is the average regional employment share of U.S. publicly listed firms.

Given that we have data on both employment and payroll, we can directly study the effects of changes in regional firm leverage on regional wages. Before doing so, we note that the effects of changes in regional firm leverage on aggregate regional employment are asymmetric. As Panel (A) of Table 3 shows, increases in regional firm leverage predict subsequent drops in aggregate regional employment. However, decreases in regional firm leverage do not predict subsequent growth in aggregate regional employment. While all coefficients have the "right" sign, they are not statistically significant. Thus, the story is very much one of *build-ups* in firm leverage predicting *downturns* in regional economic activity. In light of this fact, Panel (B) shows the effects of changes in regional firm leverage on regional wages separately for positive and negative changes in leverage.[11] As can be seen, while increases in regional firm leverage predict subsequent drops in aggregate regional employment, they do not predict subsequent drops in regional wages: all coefficients have the "right" sign but are insignificant. Hence, as aggregate regional employment drops, wages appear to be (downward) sticky.

Table 4 provides additional evidence on the pass-through from changes in regional employment at U.S. publicly listed firms to changes in aggregate regional employment. As is shown in Panel (A), there are no offsetting employment changes at non-listed firms. If anything, there is a small *positive* spillover on non-listed firms in the same region,

[11] Out of the 99,300 county-year observations associated with Δ Lev$(-3,0)$, 53.4 percent are positive. Conditional on being positive (negative), the mean value of Δ Lev$(-3,0)$ is 0.055 (-0.057), and the standard deviation is 0.058 (0.061). Table A.2 of the Online Appendix shows the *average* effect of Δ Lev$(-3,0)$ on regional wage growth.

though it is not statistically significant. Table A.3 of the Online Appendix provides separate analyses by industry sector. While the regional spillover effect on non-listed firms is relatively small in all three sectors—tradable, non-tradable, and "other"—it is strongest in the non-tradable sector, consistent with laid off workers cutting back on their spending at local restaurants and retail stores (Moretti 2010). We explore this issue further in Panel (B) by splitting regions into quintiles based on their employment share of U.S. publicly listed firms. For each quintile, we separately estimate the regional spillover effect on non-listed firms in the non-tradable sector. As can be seen, the coefficients are increasing across quintiles. Importantly, in the highest quintile—regions in which U.S. publicly listed firms account for 29.3 percent of total employment on average—the regional spillover effect in columns (1) and (2) is marginally significant.

3.4 Industry Sectors

Table 5 provides a breakdown of our results by industry sector. Panel (A) considers tradable industries. Panel (B) considers non-tradable industries. Panel (C) considers all other industries. All variables are specific to a given industry sector. For example, in Panel (A), regional firm leverage is the weighted average leverage ratio across publicly listed firms with *tradable* establishments in a given county, and the weights are based on the firms' shares of *tradable* county-level employment. Likewise, employment growth is the growth rate of *tradable* employment at the county level.

Inspection of all three panels shows that the results are qualitatively similar across industry sectors. Importantly, that the results also hold for tradable industries suggests that they are not driven by regional demand shocks—since demand for tradable goods is national or global (Mian and Sufi 2014). We will explore the issue of regional shocks in more detail in Section 4. To interpret the coefficients, we note that the standard deviation associated with Δ Lev$(-3, 0)$ is 0.108 for tradable industries, 0.086 for non-tradable industries, and 0.091 for all other industries. Take column (2), for example, where the dependent variable is Δ log(Emp)$(1, 4)$. A one standard deviation increase in regional firm leverage predicts a subsequent decline in aggregate regional employment of

0.4 percent both in the tradable and non-tradable sector and 0.6 percent in the "other" sector. Thus, the medium-run effects of changes in regional firm leverage on aggregate regional (sectoral) employment are relatively similar across industry sectors.

3.5 Broader Definitions of Regions

Using counties for our regional analysis entails two main advantages. First, our sample consists of over 80,000 county-year observations, allowing us to precisely estimate all coefficients. Second, with over 3,000 counties, there is ample regional variation in both regional firm leverage and aggregate regional employment growth.

In Table 6, we estimate equation (2) using broader definitions of regions. Panel (A) considers MSAs, while Panel (B) considers states. As is shown, the results are similar to our county-level results. To interpret the coefficients, note that the standard deviation associated with Δ Lev$(-3, 0)$ is 0.043 at the MSA level and 0.044 at the state level. In column (2), for instance, a one standard deviation increase in regional firm leverage predicts a subsequent drop in aggregate regional employment of 0.4 percent at the MSA level and 0.8 percent at the state level, which is of similar magnitude as the corresponding drop in aggregate regional employment at the county level (see Section 3.2). Hence, our results are robust to using broader definitions of regions.

3.6 National Recessions

All our results so far have been unconditional, in the sense that they do not condition on economic downturns. Rather, they *predict* downturns in regional activity following increases in regional firm leverage. We note that increases in regional firm leverage are not simply a by-product of national expansions. During our sample period from 1976 to 2011, the NBER records five recessions: January 1980 to July 1980, July 1981 to November 1982, July 1990 to March 1991, March 2001 to November 2001, and December 2007 to June 2009.[12] Altogether, these five recessions comprise 56 contraction months, leaving us with $35 \times 12 - 56 = 364$ expansion months. Hence, there are about 6.5 times

[12] See http://www.nber.org/cycles.html.

more national expansion months than contraction months.[13] In contrast, increases and decreases in regional firm leverage are fairly balanced during the sample period—about 53 percent of observations are increases, while 47 percent are decreases.

With all that being said, national recessions provide an ideal testing ground for the theories in footnote 1, according to which increases in leverage make regions vulnerable to shocks and changes in beliefs. Cross-sectionally, these theories imply that regions which experience larger build-ups in firm leverage prior to a national recession should also experience larger declines in employment during the recession. Due to the short recovery period between 1980 and 1981, we treat the twin recessions of 1980 and 1981–1982 as a single recession lasting from 1980 to 1982. The results are shown in Table 7. Year "0" is always the year prior to the recession. For example, in Panel (A), Δ Lev$(-3,0)$ is the change in regional firm leverage from 1976 to 1979, while Δ log(Emp)$(1,2)$, Δ log(Emp)$(1,3)$, and Δ log(Emp)$(1,4)$ are the growth in aggregate regional employment from 1980 to 1981, 1980 to 1982, and 1980 to 1983, respectively.

Inspection of Panels (A) to (D) shows that regions with stronger build-ups in firm leverage prior to a national recession also experience larger employment losses during the recession. This is true of all recessions during our sample period. To interpret the coefficients, note that the standard deviation associated with Δ Lev$(-3,0)$ is 0.060 in Panel (A), 0.076 in Panel (B), 0.075 in Panel (C), and 0.105 in Panel (D). Consider, for instance, column (2). In the 1980–1982 and 2001 recessions, the effect of a build-up in firm leverage is similar to the average sample effect in Table 2. In both recessions, a one standard deviation increase in regional firm leverage prior to the recession is associated with a 0.4 percent decline in aggregate regional employment in the recession.[14] In the 1990–1991 recession, the effect of a build-up in firm leverage is much stronger. In that recession, a one standard deviation increase in regional firm leverage before the recession

[13] Dupraz, Nakamura, and Steinsson (2017) measure expansions and contractions solely based on the unemployment rate. Based on their measure, there are 265 expansion months and 155 contraction months during our 1976-2011 sample period.

[14] The average sample effect of 0.4 percent in column (5) of Panel (B) of Table 2 is based on a three-year employment growth window (Δ log(Emp)$(1,4)$), whereas the effect in column (2) of Table 7 is based on a two-year employment growth window (Δ log(Emp)$(1,3)$).

is associated with a subsequent drop in aggregate regional employment of 1.1 percent. Lastly, the effect of a build-up in firm leverage is strongest in the 2007–2009 recession, where a one standard deviation increase in regional firm leverage prior to the recession is associated with a 1.5 percent reduction in aggregate regional employment during the recession. Overall, these results show that the relative significance of leverage build-ups prior to national recessions differs across business cycles.

4 Regional Shocks

A main empirical concern is that our results could be driven by regional shocks. Such regional shocks could lead to employment changes in the short run, which mean-revert in the medium run. Changes in regional firm leverage, in turn, could be driven by these employment changes. Or they could be directly driven by the regional shocks or be spuriously correlated with them.

We believe that this possibility is rather unlikely. Leverage ratios are determined at the overall firm level, not at the individual establishment (or firm-region) level, and the typical U.S. publicly listed firm has operations in 32.3 counties, 19.9 MSAs, and 8.1 states.[15] Perhaps most important, if our results were driven by regional shocks, we would expect that regional employment by non-listed firms responds as well. Indeed, it should respond *more strongly* than regional employment by listed firms, given that the typical non-listed firm is a small local firm (see Table 1), which is likely financially constrained (Giroud and Mueller 2017), and which cannot share the impacts of regional shocks with other units through firms' internal networks (Giroud and Mueller 2018). However, as Table 4 shows, regional employment by non-listed firms remains essentially unchanged.

[15] Firms' leverage ratios may disproportionately depend on factors associated with the region in which the firm's *headquarters* are located. For instance, Gao, Ng, and Wang (2011) find that headquarters-MSA fixed effects are correlated with firm leverage after controlling for time and industry fixed effects as well as time-varying firm-level controls. In Table A.4 of the Online Appendix, we re-compute our measure of regional firm leverage using only firms with out-of-state headquarters (Panel (A)) or headquarters which are located at least 1,000 miles away from the given region (Panel (B)). As can be seen, the results are very similar to our baseline results.

4.1 Controlling for Short-Run Employment Growth

Table 8 provides further evidence suggesting that our results are not driven by regional shocks. In Panel (A), we control for short-run employment changes. Precisely, when estimating the (medium-run) effects of changes in regional firm leverage from $t-3$ to t on aggregate regional employment growth from t to $t+3$, $t+1$ to $t+4$, or $t+2$ to $t+5$, we control for aggregate regional employment growth from $t-3$ to t. Arguably, this may be "overcontrolling," as it precludes the possibility that changes in regional firm leverage affect subsequent employment growth *through* short-run employment changes. Maybe surprisingly, our results become only slightly weaker when we control for short-run employment changes. In columns (1) to (3), the coefficients are close to the original coefficients in Table 2 (-0.030 vs. -0.035; -0.049 vs. -0.053; -0.042 vs. -0.046), and their significance is only slightly reduced ($p-$values of 0.076 vs. 0.049; 0.003 vs. 0.002; 0.013 vs. 0.007). Importantly, and consistent with the theories in footnote 1, the results show that changes in regional firm leverage have separate predictive power for medium-run employment growth—beyond their potential short-run effects.

4.2 Establishment-Level Evidence

In Panel (B), we consider employment growth at the establishment level. This allows us to directly control for regional shocks by saturating our empirical model with highly granular fixed effects. Precisely, we estimate equation (3), which is similar to equation (2), except that leverage and employment are disaggregated at the individual firm and establishment level, respectively. Importantly, equation (3) includes county $\times$ industry $\times$ year fixed effects, where industries are measured at the 4-digit NAICS code level. Accordingly, we compare establishments in the same county, industry, and year—which are likely exposed to the same regional shocks—that belong to U.S. publicly listed firms with different changes in firm leverage from $t-3$ to t. As is shown, increases in firm leverage predict subsequent declines in employment even if we control for regional shocks via highly granular fixed effects.

5 Firm vs. Household Leverage

Another possible concern is that our results could be driven by a spurious correlation between changes in regional firm leverage and changes in regional household leverage. Mian, Sufi, and Verner (2017), using data from 30 countries from 1960 to 2012, find that increases in the ratio of household debt to GDP predict lower subsequent country-level GDP growth. By contrast, increases in the ratio of non-financial firm debt to GDP have only weak predictive power for medium-run GDP growth. An important benefit of our regional setting is that we can separate the effects of changes in regional firm leverage and changes in regional household leverage. Indeed, changes in regional firm leverage in our setting are not based on regional firms. Rather, they measure *regional exposure* to leverage build-ups by large U.S. publicly listed firms, which are widely spread across regions. In contrast, changes in regional household leverage reflect the leverage choices of regional households and, as such, are driven by regional shocks (e.g., shocks to local house prices). In our sample, changes in regional firm leverage and changes in regional household leverage are practically uncorrelated ($\rho = 0.015$).

In Table 9, we include the change in regional household leverage, Δ HH Lev$(-3, 0)$, in our regression. In Panel (A), we estimate our main panel specification. The sample period is from 1999 to 2011.[16] As is shown, we obtain qualitatively similar patterns for firm and household leverage. In both cases, increases in leverage predict subsequent downturns in regional economic activity. To interpret the coefficients, we note that the standard deviations associated with Δ Lev$(-3, 0)$ and Δ HH Lev$(-3, 0)$ are 0.069 and 0.421, respectively. In column (2), for example, a one standard deviation increase in regional firm leverage predicts a subsequent decline in aggregate regional employment of 0.5 percent, which is almost identical to our baseline estimate. Also in column (2), a one standard deviation increase in regional household leverage predicts a subsequent decline in aggregate regional employment of 1.6 percent, which is of similar magnitude as the 2.1 percent decline in GDP growth found by Mian, Sufi, and Verner (2017)—despite

[16]Household leverage is the ratio of household debt (mortgage, credit card, and auto loan debt) to income at the county level. 1999 is the first available year for which we have data on household debt.

differences in samples and variable definitions. Altogether, our results suggest that the predictability of downturns after increases in leverage is a fairly broad phenomenon that holds for increases in both household leverage and firm leverage.

In Panel (B), we consider the cross-sectional specification in Panel (D) of Table 7, which examines the relation between changes in regional firm leverage before the 2007–2009 recession, Δ Lev$(-3, 0)$, and changes in aggregate regional employment during the recession. (Year "0" is 2006.) Similar to what we did in Panel (A), we additionally include the change in regional household leverage, Δ HH Lev$(-3, 0)$, in our regression. Accordingly, we examine whether regions with larger build-ups in either firm or household leverage prior to the Great Recession exhibit larger declines in aggregate regional employment during the Great Recession. As is shown, we obtain similar patterns for firm and household leverage. In both cases, regions with larger build-ups in leverage before the recession fare worse during the recession. To interpret the coefficients, we note that the standard deviations associated with Δ Lev$(-3, 0)$ and Δ HH Lev$(-3, 0)$ are 0.105 and 0.572, respectively. In column (2), for example, this implies that a one standard deviation increase in regional firm leverage before the Great Recession is associated with a 1.3 percent decline in aggregate regional employment during the Great Recession. The economic significance of an increase in regional household leverage is the same.

6 Conclusion

Our paper contributes to a growing literature showing that build-ups in leverage predict downturns in economic activity (e.g., Schularick and Taylor 2012; Jordà, Schularick, and Taylor 2013; Baron and Xiong 2017; Mian, Sufi, and Verner 2017). Our empirical approach differs from prior studies in that we exploit regional heterogeneity in leverage build-ups by large U.S. publicly listed firms, allowing us to separate the effects of leverage build-ups from those of regional shocks that may affect short- and medium-run output growth, and possibly also leverage growth. It also allows us to separate the effects of firm leverage growth from those of household leverage growth. In our data, the two are virtually uncorrelated.

Our paper emphasizes the role of *firm* leverage growth for regional business cycles. Consistent with theories arguing that increases in firm leverage bring about fragility and vulnerability to shocks (e.g., Bernanke and Gertler 1989; Kiyotaki and Moore 1997; Bernanke, Gertler, and Gilchrist 1999), we find that a one standard deviation increase in regional firm leverage—the employment-weighted average leverage ratio of U.S. publicly listed firms operating in a region—predicts a subsequent decline in aggregate regional employment of 0.3 to 0.4 percent. The pass-through from regional employment at U.S. publicly listed firms to aggregate regional employment is nearly perfect: there are no offsetting employment changes at non-listed firms. If anything, there is a small *positive* spillover on non-listed firms in the same region, especially in the non-tradable sector and regions with large employment shares by U.S. publicly listed firms.

Our results have implications for public policy. Prior studies have shown that fiscal or monetary policy shocks differentially impact U.S. regions—because regions are differentially exposed to military build-ups (Nakamura and Steinsson 2014) or home-equity based borrowing (Beraja et al. 2018). Our study suggests that policy measures affecting the borrowing decisions of large U.S. publicly listed firms may differentially impact U.S. regions—because regions are differentially exposed to these firms.

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Figure 1
Bin Scatterplots

This figure shows bin scatterplots depicting the relation between changes in regional firm leverage and either regional employment growth at U.S. publicly listed firms (Panel (A)) or aggregate regional employment growth (Panel (B)). The left plots depict the relation between Δ Lev(-3,0) and Δ log(Emp)(-3,0) based on 99,300 county-year observations. The right plots depict the relation between Δ Lev(-3,0) and Δ log(Emp)(1,4) based on 86,500 county-year observations. For a given percentile bin, the plots show the mean values of Δ Lev(-3,0) and either Δ log(Emp)(-3,0) or Δ log(Emp)(1,4). All variables are described in Table 2.

Panel (A): Regional employment growth at U.S. publicly listed firms

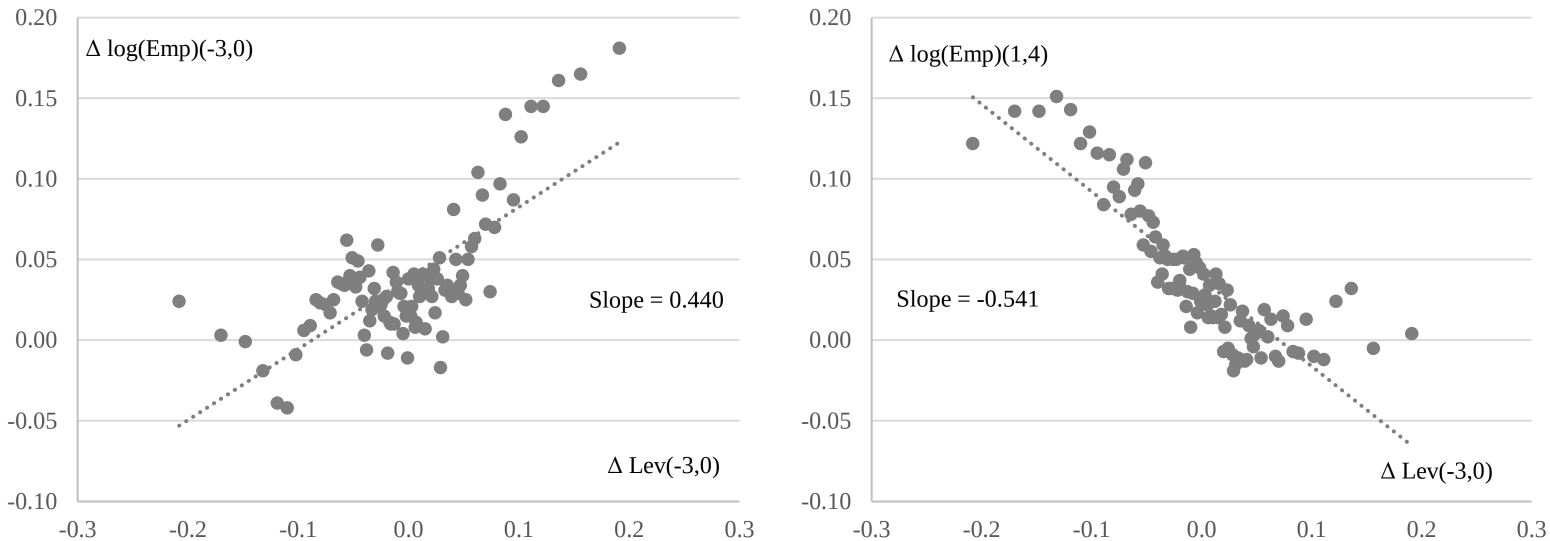
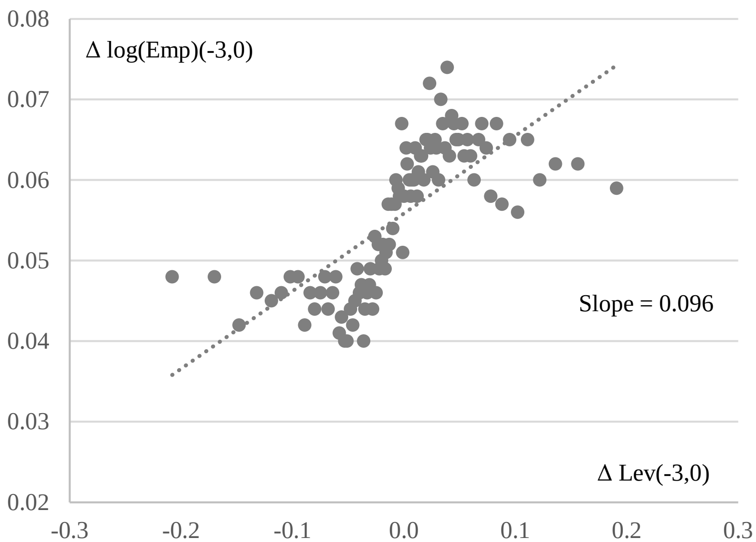


Figure 1
(continued)

Panel (B): Aggregate regional employment growth



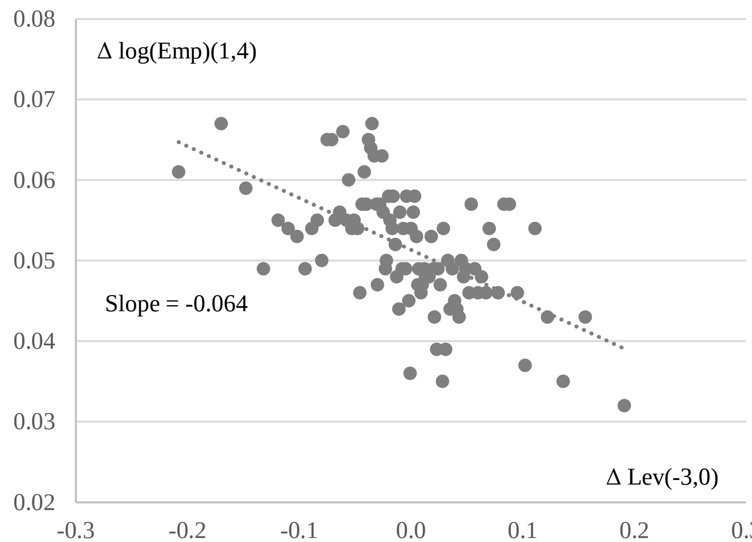


Table 1
Summary Statistics

Panel (A) provides firm-level summary statistics for all firms (column (1)), publicly listed firms (column (2)), and non-listed firms (column (3)). # ZIP codes is the number of ZIP codes in which the firm has establishments. # Counties, # MSAs, and # States are defined analogously. Leverage is the ratio of the sum of debt in current liabilities (Compustat item DLC) and long-term debt (item DLTT) to total assets (item AT). Δ Lev is the change in leverage from $t-3$ to t. Panel (B) provides county-level summary statistics for all firms (column (1)), publicly listed firms (column (2)), and non-listed firms (column (3)). Δ log(Emp) is the county-level employment growth from $t-3$ to t. Employment share is the county-level employment share of publicly listed firms (column (2)) and non-listed firms (column (3)), respectively, across all industries. Employment share (non-tradable), employment share (tradable), and employment share (other) are defined analogously for non-tradable, tradable, and "other" industries, respectively, based on the industry classification in Mian and Sufi (2014). Leverage is the weighted average leverage ratio across all publicly listed firms with establishments in a county. Weights are based on firms' county-level employment shares. Δ Lev is the change in leverage from $t-3$ to t. All figures are sample means. Standard deviations are in parentheses. The sample period is from 1976 to 2011.

Panel (A): Firm-level summary statistics

	All	Publicly Listed	Non-Listed
	(1)	(2)	(3)
Employees	21	4,282	17
	(729)	(19,616)	(457)
Establishments	1.24	85.46	1.18
	(15.81)	(417.62)	(10.24)
# ZIP codes	1.19	63.63	1.14
	(10.31)	(264.83)	(6.86)
# Counties	1.10	32.32	1.08
	(4.05)	(97.21)	(2.84)
# MSAs	1.07	19.94	1.05
	(2.04)	(45.06)	(1.50)
# States	1.03	8.06	1.02
	(0.64)	(11.62)	(0.51)
Leverage		0.261	
		(0.243)	
Δ Lev		-0.002	
		(0.082)	
Observations	181,732,500	145,600	181,587,000

Table 1
(continued)

Panel (B): County-level summary statistics

	All	Publicly Listed	Non-Listed
	(1)	(2)	(3)
Δ log(Emp)	0.054	0.041	0.062
	(0.190)	(0.266)	(0.244)
Employment share		0.128	0.872
		(0.107)	(0.107)
Employment share (non-tradable)		0.132	0.868
		(0.106)	(0.106)
Employment share (tradable)		0.269	0.731
		(0.278)	(0.278)
Employment share (other)		0.071	0.929
		(0.077)	(0.077)
Leverage		0.288	
		(0.073)	
Δ Lev		-0.002	
		(0.080)	
Observations	99,300	99,300	99,300

Table 2
Main Results

The dependent variable, Δ log(Emp) (τ, τ + 3), is employment growth from $t + \tau$ to $t + \tau + 3$, where τ = –3, … , 2. In Panel (A), employment is total employment of all publicly listed firms in a county. In Panel (B), employment is total county-level employment. Δ Lev(–3,0) is described in Panel (B) of Table 1. Observations are weighted by county-level employment. Standard errors are double clustered at the county and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Regional employment growth at U.S. publicly listed firms

	Δ log(Emp) (-3,0)	Δ log(Emp) (-2,1)	Δ log(Emp) (-1,2)	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Lev(-3,0)	0.476***	0.099	-0.278*	-0.231**	-0.381***	-0.338**
	(0.181)	(0.219)	(0.145)	(0.117)	(0.135)	(0.145)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.58	0.57	0.60	0.58	0.60	0.58
Observations	99,300	96,100	92,900	89,700	86,500	83,300

Panel (B): Aggregate regional employment growth

	Δ log(Emp) (-3,0)	Δ log(Emp) (-2,1)	Δ log(Emp) (-1,2)	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Lev(-3,0)	0.062***	0.019	-0.033*	-0.035**	-0.053***	-0.046***
	(0.022)	(0.018)	(0.020)	(0.018)	(0.017)	(0.017)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.38	0.37	0.36	0.35	0.37	0.38
Observations	99,300	96,100	92,900	89,700	86,500	83,300

Table 3
Positive and Negative Changes in Regional Firm Leverage

Panel (A) presents variants of the regressions in columns (4) to (6) of Panel (B) of Table 2 in which Δ Lev(–3,0) is interacted with dummy variables indicating whether the change in leverage is positive or negative. Panel (B) is similar, except that the dependent variable is county-level wage growth, Δ log(Wages), where Wages is the ratio of payroll to employees at the county level. Observations are weighted by county-level employment. Standard errors are double clustered at the county and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Aggregate regional employment growth

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)$^{+}$	-0.046*	-0.062**	-0.058**
	(0.026)	(0.028)	(0.027)
Δ Lev(-3,0)$^{-}$	-0.026	-0.039	-0.029
	(0.034)	(0.042)	(0.047)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.35	0.37	0.38
Observations	89,700	86,500	83,300

Table 3
(continued)

Panel (B): Aggregate regional wage growth

	Δ log(Wages) (0,3)	Δ log(Wages) (1,4)	Δ log(Wages) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)$^{+}$	-0.019	-0.028	-0.007
	(0.025)	(0.020)	(0.022)
Δ Lev(-3,0)$^{-}$	-0.016	-0.021	-0.005
	(0.022)	(0.033)	(0.021)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.62	0.53	0.63
Observations	89,700	86,500	83,300

Table 4
Regional Spillover Effects on Non-listed Firms

Panel (A) presents variants of the regressions in columns (4) to (6) of Table 2 in which employment is total employment of all non-listed firms in a county. Panel (B) considers non-listed firms in the non-tradable sector, and counties are split into quintiles based on their employment share of publicly listed firms. For a given quintile, μ denotes the average county-level employment share of publicly listed firms. Observations are weighted by county-level employment. Standard errors are double clustered at the county and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): All non-listed firms

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.007	-0.009	-0.003
	(0.023)	(0.025)	(0.019)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.48	0.49	0.50
Observations	89,700	86,500	83,300

Table 4
(continued)

Panel (B): Non-listed firms in the non-tradable sector

	Δ log(Emp) (0,3) (1)	Δ log(Emp) (1,4) (2)	Δ log(Emp) (2,5) (3)
1st Quintile (μ = 0.002)	-0.007	-0.003	-0.003
	(0.015)	(0.021)	(0.017)
2nd Quintile (μ = 0.052)	-0.010	-0.011	-0.004
	(0.024)	(0.040)	(0.019)
3rd Quintile (μ = 0.120)	-0.010	-0.013	-0.005
	(0.015)	(0.014)	(0.018)
4th Quintile (μ = 0.192)	-0.012	-0.023	-0.010
	(0.022)	(0.024)	(0.027)
5th Quintile (μ = 0.293)	-0.028*	-0.030*	-0.019
	(0.017)	(0.018)	(0.017)

Table 5
Industry Sectors

This table presents variants of the regressions in columns (4) to (6) of Panel (B) of Table 2 in which the sample is split by industry sector. All variables are sector-specific as described in Section 3.4. Non-tradable, tradable, and "other" industries are described in Table 1. Observations are weighted by county-level employment. Standard errors are double clustered at the county and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Tradable industries

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.039*	-0.040**	-0.055**
	(0.021)	(0.020)	(0.025)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.23	0.23	0.24
Observations	88,300	85,200	82,000

Panel (B): Non-tradable industries

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.034**	-0.044**	-0.038**
	(0.015)	(0.017)	(0.017)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.35	0.36	0.36
Observations	89,700	86,500	83,300

Table 5
(continued)

Panel (C): Other industries

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.037*	-0.063***	-0.060***
	(0.020)	(0.022)	(0.021)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.29	0.30	0.31
Observations	89,700	86,500	83,300

Table 6
Broader Definitions of Regions

This table presents variants of the regressions in columns (4) to (6) of Panel (B) of Table 2 in which regions are based on MSAs (Panel (A)) or states (Panel (B)) in lieu of counties. Observations are weighted by region-level employment. Standard errors are double clustered at the region and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): MSAs

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.071*	-0.110***	-0.097***
	(0.042)	(0.036)	(0.035)
MSA fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.46	0.46	0.49
Observations	11,300	11,000	10,600

Panel (B): States

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.129*	-0.183***	-0.128**
	(0.073)	(0.054)	(0.048)
State fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.60	0.60	0.63
Observations	1,500	1,500	1,400

Table 7
National Recessions

This table presents (cross-sectional) variants of the regressions in columns (4) to (6) of Panel (B) of Table 2 in which the sample is restricted to the 1980-82 recession (Panel (A)), 1990-91 recession (Panel (B)), 2001 recession (Panel (C)), and 2007-09 recession (Panel (D)). Year "0" is always the year before the recession: 1979 in Panel (A), 1989 in Panel (B), 2000 in Panel (C), and 2006 in Panel (D). Observations are weighted by county-level employment. Standard errors are clustered at the county level. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): 1980-82 recession

	Δ log(Emp) (1,2)	Δ log(Emp) (1,3)	Δ log(Emp) (1,4)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.039***	-0.065***	-0.076***
	(0.010)	(0.014)	(0.013)

Panel (B): 1990-91 recession

	Δ log(Emp) (1,2)	Δ log(Emp) (1,3)	Δ log(Emp) (1,4)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.124***	-0.148***	-0.150***
	(0.011)	(0.016)	(0.013)

Panel (C): 2001 recession

	Δ log(Emp) (1,2)	Δ log(Emp) (1,3)	Δ log(Emp) (1,4)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.067***	-0.056***	-0.030
	(0.014)	(0.018)	(0.020)

Table 7
(continued)

Panel (D): 2007-09 recession

	Δ log(Emp) (1,2)	Δ log(Emp) (1,3)	Δ log(Emp) (1,4)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.133***	-0.139***	-0.147***
	(0.032)	(0.038)	(0.035)

Table 8
Regional Shocks

This table presents variants of the regressions in columns (4) to (6) of Panel (B) of Table 2. In Panel (A), the regressions include Δ log(Emp)(–3,0) as a control. In Panel (B), observations are at the establishment-year level, and the regressions include county × industry × year fixed effects. Industries are measured at the 4-digit NAICS code level. In Panel (A), observations are weighted by county-level employment. In Panel (B), observations are weighted by establishment-level employment. Standard errors are double clustered at the county and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Controlling for short-run employment changes

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.030*	-0.049***	-0.042**
	(0.018)	(0.017)	(0.017)
Δ log(Emp)(-3,0)	-0.082***	-0.052***	-0.056***
	(0.013)	(0.010)	(0.010)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.38	0.37	0.39
Observations	89,700	86,500	83,300

Panel (B): Establishment-level evidence

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.173**	-0.177**	-0.148*
	(0.082)	(0.080)	(0.090)
County × industry × year fixed effects	Yes	Yes	Yes
R-squared	0.04	0.04	0.05
Observations	4,491,000	3,869,700	3,338,600

Table 9
Firm vs. Household Leverage

This table presents variants of the regressions in columns (4) to (6) of Panel (B) of Table 2 and Panel (D) of Table 7 in which the change in household leverage, Δ HH Lev(–3,0), is included as a control. Household leverage is the ratio of household debt (mortgage, credit card, and auto loan debt) to income at the county level. Observations are weighted by county-level employment. In Panel (A), standard errors are double clustered at the county and year level. In Panel (B), standard errors are clustered at the county level. In Panel (A), the sample period is from 1999 to 2011. In Panel (B), the sample is restricted to the 2007-09 recession. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Panel estimation

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.041*	-0.075**	-0.057*
	(0.025)	(0.031)	(0.030)
Δ HH Lev(-3,0)	-0.019*	-0.039**	-0.025**
	(0.011)	(0.012)	(0.010)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.66	0.60	0.72
Observations	9,000	8,000	5,900

Panel (B): 2007-09 recession

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.138***	-0.127***	-0.126***
	(0.037)	(0.046)	(0.046)
Δ HH Lev(-3,0)	-0.018***	-0.022***	-0.023***
	(0.005)	(0.004)	(0.004)

Online Appendix

Table A.1
Bin Scatterplot Regressions

This table presents variants of the regressions in columns (1) and (5) of Table 2 without fixed effects. Observations are weighted by county-level employment. Standard errors are double clustered at the county and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Regional employment growth at U.S. publicly listed firms

	Δ log(Emp) (-3,0)	Δ log(Emp) (1,4)
	(1)	(2)
Δ Lev(-3,0)	0.481**	-0.526***
	(0.198)	(0.149)
County fixed effects	No	No
Year fixed effects	No	No
R-squared	0.01	0.01
Observations	99,300	86,500

Panel (B): Aggregate regional employment growth

	Δ log(Emp) (-3,0)	Δ log(Emp) (1,4)
	(1)	(2)
Δ Lev(-3,0)	0.097***	-0.069***
	(0.026)	(0.021)
County fixed effects	No	No
Year fixed effects	No	No
R-squared	0.03	0.01
Observations	99,300	86,500

Table A.2
Aggregate Regional Wage Growth

This table presents variants of the regressions in columns (4) to (6) of Panel (B) of Table 2 in which the dependent variable is county-level wage growth, Δ log(Wages), where Wages is the ratio of payroll to employees at the county level. Observations are weighted by county-level employment. Standard errors are double clustered at the county and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ log(Wages) (0,3)	Δ log(Wages) (1,4)	Δ log(Wages) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.018	-0.024	-0.006
	(0.017)	(0.017)	(0.016)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.62	0.53	0.63
Observations	89,700	86,500	83,300

Table A.3
Regional Spillover Effects on Non-Listed Firms by Industry Sector

This table presents variants of the regressions in Panel (A) of Table 4 in which total employment of non-listed firms in a county is split by industry sector. Non-tradable, tradable, and “other” industries are described in Table 1. Observations are weighted by county-level employment. Standard errors are double clustered at the county and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Tradable industries

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.001	-0.002	-0.001
	(0.023)	(0.025)	(0.025)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.25	0.25	0.26
Observations	88,300	85,200	82,000

Panel (B): Non-tradable industries

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.015	-0.016	-0.010
	(0.014)	(0.014)	(0.016)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.53	0.54	0.54
Observations	89,700	86,500	83,300

Table A.3
(continued)

Panel (C): Other industries

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.006	-0.006	-0.004
	(0.013)	(0.016)	(0.013)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.35	0.36	0.37
Observations	89,700	86,500	83,300

Table A.4
Firms with Distant Headquarters

This table presents variants of the regressions in columns (4) to (6) of Panel (B) of Table 2 in which Δ Lev (-3,0) is based on firms with out-of-state headquarters (Panel (A)) or headquarters which are located at least 1,000 miles away from the given county (Panel (B)). Observations are weighted by county-level employment. Standard errors are double clustered at the county and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel (A): Firms with out-of-state headquarters

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.051**	-0.057**	-0.050**
	(0.017)	(0.019)	(0.020)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.35	0.35	0.37
Observations	87,600	84,500	81,500

Panel (B): Firms with headquarters located 1,000+ miles away

	Δ log(Emp) (0,3)	Δ log(Emp) (1,4)	Δ log(Emp) (2,5)
	(1)	(2)	(3)
Δ Lev(-3,0)	-0.040***	-0.048***	-0.040***
	(0.010)	(0.010)	(0.010)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
R-squared	0.36	0.36	0.38
Observations	60,400	59,000	57,500