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Firm leverage and employment dynamics☆

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ABSTRACT

We examine the dynamic relation between firm leverage buildups and real economic activity using U.S. establishment-, firm-, and region-level data. We find that buildups in firm leverage are predictably associated with boom-bust growth cycles: employment grows in the short run but declines in the medium run. While firm leverage buildups are correlated with firm-level expansions, they continue to predict negative future employment growth if we control for firm-level expansions. Buildups in firm leverage predict a tightening of future firm-level financing constraints, and they only predict negative future employment growth if the level of firm leverage is sufficiently high, suggesting that the dynamic relation between firm leverage buildups and employment growth operates through a financial fragility channel. Our results have aggregate implications: regions with larger buildups in firm leverage experience stronger regional boom-bust growth cycles, and they perform significantly worse during national recessions.

1. Introduction

One of the key findings in the emerging empirical literature on leverage cycles is that buildups in leverage appear to predict subsequent downturns in real economic activity (e.g., Gourinchas and Obstfeld (2012); Schularick and Taylor (2012); Jordà et al. (2013); Baron and Xiong (2017)). As Mian et al. (2017) point out, this relation is primarily driven by increases in household debt, not increases in firm debt. Using data from 30 countries from 1960 to 2012, they find that while increases in the ratio of household debt to GDP are associated with boom-bust growth cycles—GDP grows in the short run but declines in the medium run—increases in the ratio of (non-financial) firm debt to GDP are not associated with boom-bust growth cycles. Moreover, changes in firm leverage have only weak predictive power in the medium run. Mian et al. (2017, pp. 1812–1814) offer several explanations for this finding, including the possibility that firms may be more sophisticated than households and therefore base their borrowing decisions on more realistic expectations of future cash flows.

In this paper, we examine the dynamic relation between firm leverage buildups and real economic activity using U.S. establishment-, firm-, and region-level

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data. Contrary to what has been previously found in cross-country studies using aggregate data, we find that buildups in firm leverage are associated with boom-bust growth cycles: following an increase in firm leverage, employment grows in the short run but declines in the medium run. In fact, the overall effect of the leverage buildup is that firm-level employment falls below its initial level. Our results are not conditional on recessions or other trigger events. Rather, buildups in firm leverage unconditionally predict subsequent declines in employment in U.S. panel data from 1976 to 2011.

To allow a direct comparison of our results with those in previous studies, we use the same "sliding windows" specification as in Mian et al. (2017). The explanatory variable is the change in firm leverage from $t-3$ to t, and the dependent variable is firm-level employment growth from $t-3$ to t, $t-2$ to $t+1$, ..., $t+2$ to $t+5$, yielding six regressions. We find that the relation between changes in firm leverage and employment growth is positive in the short run but negative in the medium run. In the short run, a one standard deviation increase in firm leverage from $t-3$ to t is associated with a 1.3% increase in firm-level employment. In the medium run, the sign flips. For example, a one standard deviation increase in firm leverage from $t-3$ to t predicts a 1.2% decline in firm-level employment from $t+1$ to $t+4$. Thus, changes in firm leverage are predictably associated with boom-bust growth cycles.

While we are not making causal claims, we want to isolate as much as possible the source of variation underlying the dynamic relation between buildups in firm leverage and firm-level employment growth. Our main specification includes both firm and year fixed effects. Moreover, to account for local shocks in the regions in which the firm is operating, we include as a control the employment growth of small unlisted firms. These are typically single-establishment firms, which are highly sensitive to local shocks. In an alternative specification, we consider employment growth at the individual establishment level while including both establishment and highly granular county $\times$ industry $\times$ year fixed effects. Accordingly, we compare establishments in the same county and (4-digit NAICS) industry that belong to parent firms with different changes in firm leverage. Lastly, to estimate the cumulative effect of an increase in firm leverage, we depart from our sliding windows approach and use a local projection approach, where the dependent variable is firm-level employment growth from $t-3$ to t, $t-3$ to $t+1$, ..., $t-3$ to $t+5$, yielding six regressions. Regardless of whether we consider six- ($t-3$ to $t+3$), seven- ($t-3$ to $t+4$), or eight-year ($t-3$ to $t+5$) employment growth, we find that firm-level employment ends up significantly below its initial level.

We consider some obvious candidate explanations for the negative relation between changes in firm leverage and medium-run employment growth. One is that this relation is driven by (over-)expansions, which are merely correlated with firm-level borrowing. As measures of firm-level expansions, we consider changes in capital expenditures (i.e., investment), sales growth, and changes in the number of establishments. We document three facts. First, changes in firm leverage are indeed positively correlated with firm-level expansions. Second, expansions predict positive, not negative, medium-run employment growth. Third, changes in firm leverage continue to predict negative medium-run employment growth if we control for firm-level expansions. Hence, while expansions may well be the reason for why firms increase their borrowing, it is the buildup in leverage, not the expansion, that predicts subsequent drops in employment. Similarly, we ask whether our results are driven by mean reversion in employment growth. Precisely, changes in firm leverage may be negatively correlated with employment growth in the medium run only because they are positively correlated with short-run employment growth. However, we find that short-run employment growth is positively, not negatively, correlated with medium-run employment growth. Importantly, buildups in firm leverage continue to predict negative medium-run employment growth if we control for short-run employment growth.

Models based on financial frictions as well as behavioral models link buildups in leverage to financial fragility and eventually to downturns in real economic activity. We provide some limited evidence suggesting that the dynamic relation between changes in firm leverage and employment growth operates through a financial fragility channel. We first document that buildups in firm leverage predict a tightening of firm-level financing constraints, impairing firms' ability to respond to future shocks. We obtain similar results if we consider covenant violations in lieu of firm-level measures of financing constraints. Second, and this is perhaps the strongest evidence in support of a financial fragility channel, we show that changes in firm leverage predict subsequent employment drops only if the level of firm leverage is sufficiently high. Indeed, changes in firm leverage have no predictive power if the firm's leverage ratio is in the bottom tercile of its empirical distribution.

That increases in firm leverage predict subsequent declines in employment may not matter in the aggregate if workers laid off by firms with high leverage buildups are re-employed by other firms. To see if our results have aggregate implications, we consider aggregate employment at the county level. Changes in firm leverage at the county level are based on the weighted average leverage ratio across firms with establishments in the county ("county leverage"), where the weights are based on firms' county-level employment shares. Our findings mirror those at the firm level: the relation between changes in county leverage and county-level employment growth is positive in the short run but negative in the medium run. In the short run, a one standard deviation increase in county leverage from $t-3$ to t is associated with a 0.5% increase in county-level employment. By contrast, in the medium run, a one standard deviation increase in county leverage from $t-3$ to t predicts a 0.4% drop in county-level employment from $t+1$ to $t+4$. For a given county, this implies increases in firms' borrowing are associated with boom-bust growth cycles: employment grows in the short run but declines in the medium run. Across counties, our results imply that counties with larger buildups in firm leverage experience stronger short-run growth, but also stronger medium-run declines, in aggregate county-level employment. We obtain similar results if we consider MSAs and states in lieu of counties.

Finally, we examine whether counties with larger buildups in firm leverage prior to a national recession experience larger employment drops during the recession. Unlike the rest of this paper, this analysis is conditional (namely, on a recession). While we find that counties with larger leverage buildups perform worse during recessions, we also find that the significance of leverage buildups varies across recessions. For instance, a one standard deviation increase in county leverage prior to the 1980–1982 or 2001 recession is associated with a subsequent decline in county-level employment of 0.4%. By comparison, the effect is almost four times larger in the 2007–2009 recession, where a one standard deviation increase in county leverage before the recession is associated with a 1.5% decline in county-level employment during the recession.

Our results contribute to the empirical literature on leverage cycles by showing that buildups in firm leverage are associated with lower future employment growth, as well as lower levels of employment (see our local projection results).[1] Our findings also have implications for theory. The key challenge, from a theoretical perspective, is to explain why leverage buildups are predictably associated with future employment declines. The existing literature has proposed several explanations. For example, borrowing may impose aggregate demand or pecuniary externalities, leading to overborrowing in equilibrium (e.g., Lorenzoni, 2008; Jeanne and Korinek, 2010; Bianchi, 2011; Korinek and Simsek, 2016). Alternatively, leverage buildups, which may lead to downturns in real economic activity, may result from dispersion in beliefs (e.g., Fostel and Geanakoplos, 2008; Geanakoplos, 2009) or biased expectations (e.g., Bordalo et al., 2018). While these theories are valuable, not all of them are well suited to explain individual firm behavior. Hence, we believe there is still scope for more (traditional corporate finance-style) theories, in particular, models that focus on individual firm behavior.

The rest of this paper is organized as follows. In Section 2, we describe the data, empirical methodology, and summary statistics. In Section 3, we examine the relation between firm leverage buildups and employment growth at the establishment and firm level. In Section 4, we explore alternative channels: firm-level expansions and mean-reverting employment growth. In Section 5, we provide evidence in support of a financial fragility channel. In Section 6, we consider aggregate county-level employment. We conclude in Section 7.

[1] In line with prior studies (e.g., Schularick and Taylor, 2012; Mian et al., 2017), we take buildups in leverage as given and focus on whether they predict future real economic activity. In practice, firms may increase leverage for many (often firm-specific) reasons, including changes in their cost of capital. Frank and Goyal (2009) provide an empirical assessment of the determinants of firm leverage, Graham and Harvey (2002) present related evidence from CFO surveys, and Graham and Leary (2011) review the empirical capital structure literature.

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), for example, a restaurant, department store, or manufacturing plant. Our sample period is from 1976 to 2011.

We match establishments in the LBD to publicly listed firms in Compustat using the Compustat-SSEL bridge maintained by the U.S. Census Bureau.[2] Given that the bridge ends in 2005, we extend the match to 2011 using employer name and ID number (EIN) by applying the methodology described in McCue (2003). Following standard practice, we exclude regulated industries (e.g., utilities, financials), as well as firms with missing financial data. Our final matched Compustat-LBD sample consists of 145,600 firm-year observations.

We obtain firm-level and regional (county, MSA, state) employment by adding up employment across individual establishments. Firm leverage, Capex, and sales are from Compustat. Firm 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. Capex is the ratio of capital expenditures (item CAPX) to property, plant, and equipment (item PPENT) and is winsorized at the 1st and 99th percentiles of the empirical distribution. Sales is Compustat item SALE. In Section 6, we consider firm leverage and aggregate employment at the county level. To obtain a measure of firm leverage at the county level (“county leverage”), we compute the weighted average leverage ratio across all publicly listed firms with establishments in the county. Weights are based on firms’ county-level employment shares.

2.2. Empirical methodology

We study the dynamic relation between changes in firm leverage and subsequent firm-level employment growth. We estimate the following equation:

$$\Delta \log(\text{Emp})_{i,t}(t+\tau, t+\tau+3) = \alpha_i + \alpha_t + \beta\ \Delta\ \text{Lev}_{i,t}(t-3,t) + \text{X}_{i,t} + \varepsilon_{i,t}, \quad (1)$$

where $\tau = -3, \ldots, 2$; $\Delta\ \log(\text{Emp})_{i,t}(t+\tau, t+\tau+3)$ is firm i's employment growth from $t+\tau$ to $t+\tau+3$; $\Delta\ \text{Lev}_{i,t}(t-3,t)$ is the change in firm i's leverage from $t-3$ to t; $\text{X}_{i,t}$ is a firm-specific “regional control” described below, and α_i and α_t are firm and year fixed effects, respectively. We estimate Eq. (1) for all $\tau = -3, \ldots, 2$, resulting in six regressions. For example, when $\tau = -3$, the coefficient β captures the short-run relation between changes

[2] Firm affiliation in Census data is based on common ownership and control. For example, a locally-owned franchise of a national chain is considered a separate firm and may be unlisted even if the national chain itself is publicly listed.

in firm leverage from $t-3$ to t and employment growth from $t-3$ to t. As τ increases, Eq. (1) examines whether changes in firm leverage can (unconditionally) predict future employment growth. For example, when $\tau=1$, the coefficient β captures the medium-run relation between changes in firm leverage from $t-3$ to t and employment growth from $t+1$ to $t+4$. To simplify the notation, we write Δ log(Emp)($\tau, \tau+3$) in lieu of Δ log(Emp)$_{i,t}(t+\tau, t+\tau+3)$ in our tables and figures. Observations are weighted by firm-level employment. Standard errors are double clustered at the state (using the firm's home state) and year level.

While we are not making causal claims, we want to isolate as much as possible the source of variation underlying the relation in Eq. (1). One possible source of variation comes from shocks in the regions in which the firm is operating. To account for such regional shocks, we include as a control variable the weighted average employment growth of unlisted firms—firms in the LBD that do not have a match in Compustat—in regions in which the firm is operating ("regional control"). Specifically, for each county $k \in K$ in which firm i is operating, we compute Δ log(Emp)$_{i,k,t}(t+\tau, t+\tau+3)$ based on the total employment of unlisted firms in county k. We then compute the weighted average of this measure across all counties in which firm i is operating using as weights the counties' shares of firm i's overall employment.

Using the regional employment growth of unlisted firms to account for regional shocks is predicated on the notion that unlisted firms are sensitive to regional shocks. Unlisted firms are small and local and thus unable to diversify shocks across space (Giroud and Mueller, 2019). Indeed, the typical unlisted firm is a single-establishment firm with 17 employees that operates in a single ZIP Code. Not surprisingly, unlisted firms have been found to react strongly to local shocks in empirical studies using Census data (Giroud and Mueller, 2017). Arguably, controlling for regional shocks improves the quality of our estimation only if the publicly listed firms in our Compustat-LBD sample are sensitive to regional shocks. As Table A.1 of the Online Appendix shows, the employment growth of publicly listed and unlisted firms are highly correlated at the regional level, suggesting that both types of firms are sensitive to regional shocks.

2.3. Summary statistics

Table 1 provides summary statistics. Panel A provides firm-level summary statistics separately for all firms, publicly listed firms, and unlisted firms. As one would expect, publicly listed firms are much larger than unlisted firms; they have more employees and more establishments. Indeed, the typical unlisted firm in the LBD is a small firm with (little more than) a single establishment operating in a single ZIP Code. In contrast, the typical publicly listed firm has 85.5 establishments in 63.6 ZIP Codes, 32.3 counties, 19.9 MSAs, and 8.1 states. The average three-year employment growth is 5.5% for all firms, 4.4% for publicly listed firms, and 5.6% for unlisted firms. The average leverage ratio of publicly listed firms is 0.261. And while there are many ups and downs in firm leverage during our sample period, the average three-year change in firm leverage is close to zero, with a standard deviation of 0.082.

Panel B provides summary statistics at the county level. The average three-year employment growth at the county level is 5.4% for all firms, 4.1% for publicly listed firms, and 6.2% for unlisted firms. Publicly listed firms account for 12.8% of total county-level employment. The average

Table 1
Summary statistics.
Panel A provides firm-level summary statistics for all firms (column 1), publicly listed firms (column 2), and unlisted firms (column 3). Publicly listed firms are firms in the matched Compustat-LBD sample. Unlisted firms are firms in the LBD that do not have a match in Compustat. # ZIP Codes denotes the number of ZIP Codes in which the firm has establishments. # Counties, # MSAs, and # States are defined analogously. Δ log(Emp) at the firm level is the growth in firm-level employment from t - 3 to t. Leverage at the firm level 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 firm leverage from t - 3 to t. Panel B provides county-level summary statistics for all firms (column 1), publicly listed firms (column 2), and unlisted firms (column 3). Δ log(Emp) at the county level is the growth in county-level employment from t - 3 to t. Employment share is the county-level employment share of either publicly listed firms (column 2) or unlisted firms (column 3). Leverage at the county level ("county leverage") is the weighted average leverage ratio across all publicly listed firms with establishments in the county. Weights are based on the firms' county-level employment shares. Δ Lev is the change in county leverage from t - 3 to t. All numbers are sample means. Standard deviations are in parentheses. The sample period is from 1976 to 2011.

Panel A: Firm-level summary statistics

	All (1)	Publicly listed (2)	Unlisted (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)
Δ log(Emp)	0.055	0.044	0.056
	(0.457)	(0.472)	(0.455)
Leverage		0.261	
		(0.243)	
Δ Lev		-0.002	
		(0.082)	
Observations	181,732,500	145,600	181,587,000

Panel B: County-level summary statistics

	All (1)	Publicly listed (2)	Unlisted (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)
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)$(t+\tau, t+\tau+3)$, is firm-level employment growth from $t+\tau$ to $t+\tau+3$, where τ ranges from $\tau=-3$ in column 1 to $\tau=2$ in column 6. Δ Lev $(-3,0)$ is the change in firm leverage from $t-3$ to t. Regional control is the weighted average employment growth, Δ log(Emp)$(t+\tau, t+\tau+3)$, of unlisted firms in the counties in which the firm is operating. Weights are based on the counties' shares of the firm's overall employment. Observations are weighted by firm-level employment. Standard errors (in parentheses) are double clustered at the state (using the firm's home state) and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ log(Emp) (3,0) (1)	Δ log(Emp) (2,1) (2)	Δ log(Emp) (1,2) (3)	Δ log(Emp) (0,3) (4)	Δ log(Emp) (1,4) (5)	Δ log(Emp) (2,5) (6)
Δ Lev(-3,0)	0.153***	0.037	0.143***	0.179***	0.144***	0.085***
	(0.028)	(0.029)	(0.029)	(0.030)	(0.029)	(0.029)
Regional control	0.505***	0.302***	0.262***	0.257***	0.251***	0.246***
	(0.009)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.42	0.43	0.45	0.44	0.44	0.44
Observations	145,600	127,600	111,100	98,900	85,100	74,700

firm leverage ratio at the county level ("county leverage") is 0.288, which is slightly larger than the corresponding ratio at the firm level due to the uneven spatial distribution of publicly listed firms. Finally, the average change in county leverage over a three-year period is close to zero, with a standard deviation of 0.080.

3. Firm leverage and employment dynamics

3.1. Bin scatterplots

Fig. 1 provides bin scatterplots depicting the dynamic relation between changes in firm leverage and firm-level employment growth. Panel A shows the short-run relation—the correlation between Δ Lev$(-3,0)$ and Δ log(Emp)$(-3,0)$—based on 145,600 firm-year observations, while Panel B shows the medium-run relation—the correlation between Δ Lev$(-3,0)$ and Δ log(Emp)(1,4)—based on 85,100 firm-year observations. For a given percentile bin, the figures show the mean value of Δ Lev$(-3,0)$ and Δ log(Emp)$(-3,0)$ (Panel A) or Δ log(Emp)(1,4) (Panel B).[3]

The main takeaway from Fig. 1 is that the short- and medium-run relations have opposite signs. In the short run, a one standard deviation increase in firm leverage (0.082) is associated with a 2.3% increase in firm-level employment. By contrast, in the medium run, a one standard deviation increase in firm leverage predicts a 2.1% decline in firm-level employment. Table A.2 of the Online Appendix presents the regressions that correspond to the bin scatterplots. In line with the bin scatterplots, the regressions do not include fixed effects or regional controls, in contrast to our baseline specification. As is shown, the regression coefficients associated with Δ Lev$(-3,0)$ are very close to the slope coefficients in Fig. 1: 0.285 vs. 0.283 and −0.262 vs. −0.257. Both coefficients are significant at the 1% level. Thus, the bin scatterplots in Fig. 1 provide an adequate representation of the raw (i.e., unbinned) data.

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

3.2. Main results

Table 2 presents our main results from estimating Eq. (1) for $\tau=-3,\ldots,2$. This yields six regressions with dependent variables ranging from short-run ($t-3$ to t) to medium-run (t to $t+3,\ldots,t+2$ to $t+5$) employment growth. Consistent with the visual impression from Fig. 1, the relation between changes in firm leverage and employment growth is 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 at the 1% level. The magnitudes are similar to those in Fig. 1. For example, in column 1, a one standard deviation increase in firm leverage is associated with a 1.3% short-run increase in firm-level employment. By contrast, in column 5, a one standard deviation increase in firm leverage predicts a 1.2% medium-run decline in firm-level employment. As we will see in Section 3.4, the overall (i.e., cumulative) effect of an increase in firm leverage is that employment drops below its initial level.[4]

The empirical specification in Eq. (1) narrows down the source(s) of variation underlying the dynamic relation between changes in firm leverage and employment growth. The year fixed effects account for macroeconomic shocks that are common to all firms. The firm fixed effects account for any time-invariant heterogeneity across firms. Finally, the regional control accounts for shocks in the regions in which the firm is operating, as explained in Section 2.2.[5]

[4] Table A.4 of the Online Appendix examines if the medium-run decline in employment is stronger when increases in firm leverage are associated with either share repurchases or dividend increases.

[5] Including this control improves the quality of our estimation. In Table A.3 of the Online Appendix, we provide a version of our baseline estimation without the regional control. The R-squared is consistently lower. In particular, in columns 4 to 6, which pertain to medium-run employment growth, including the regional control improves the R-squared by about 25% to 29%.

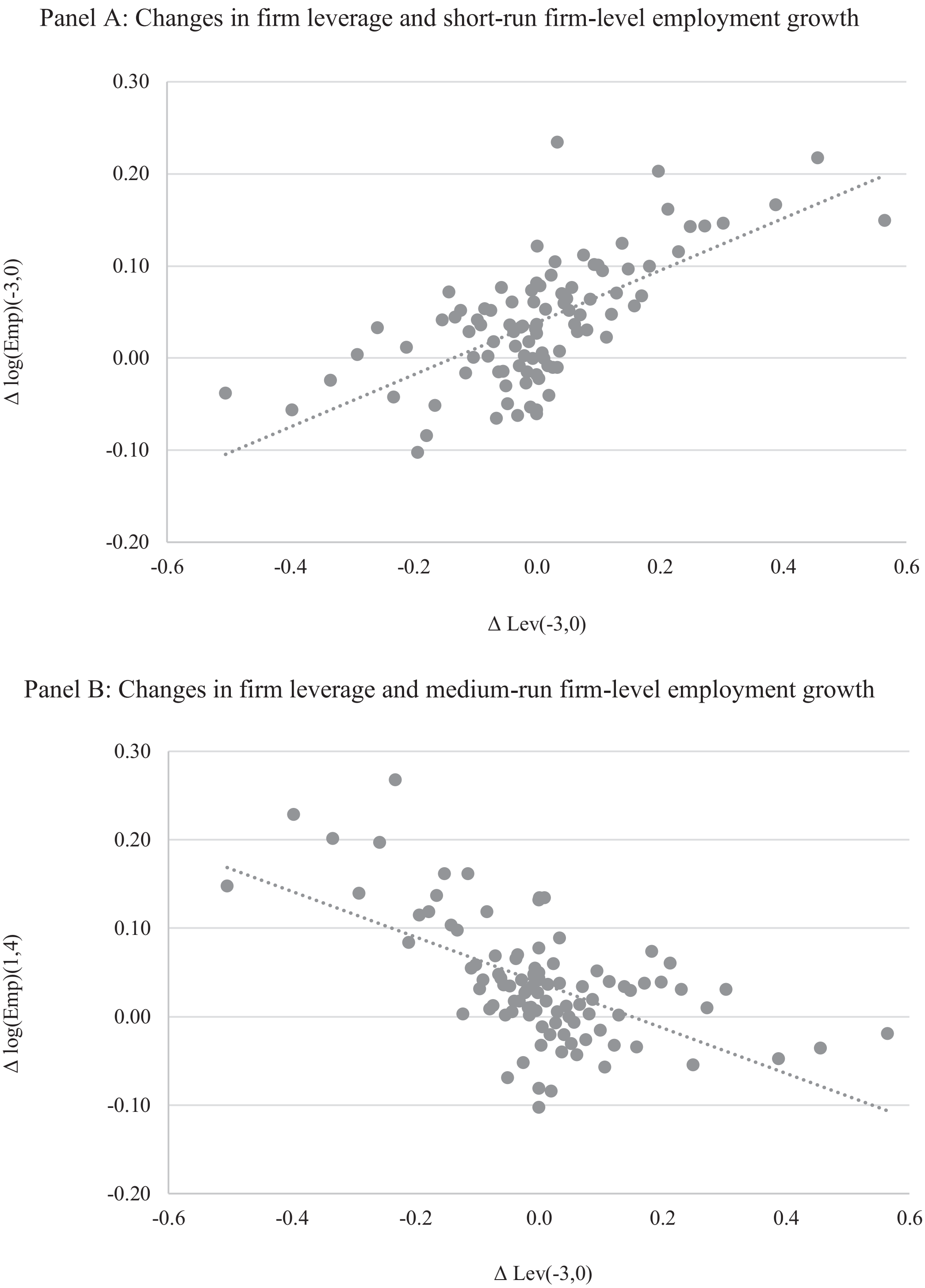


Fig. 1. Firm-level bin scatterplots. This figure provides bin scatterplots showing the relation between changes in firm leverage and firm-level employment growth. The figure in Panel A shows the short-run relation between Δ Lev(-3,0) and Δ log(Emp)(-3,0) based on 145,600 firm-year observations. The figure in Panel B shows the medium-run relation between Δ Lev(-3,0) and Δ log(Emp)(1,4) based on 85,100 firm-year observations. For a given percentile bin, the figures show the mean values of Δ Lev(-3,0) and either Δ log(Emp)(-3,0) (Panel A) or Δ log(Emp)(1,4) (Panel B).

Table 3
Establishment-level employment growth.
This table presents variants of the regressions in Table 2 in which employment growth is measured at the establishment level and the regional control and firm and year fixed effects are replaced with establishment and county × industry × year fixed effects. Industries are measured at the 4-digit NAICS code level. Observations are weighted by establishment-level employment. Standard errors (in parentheses) are double clustered at the firm and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ log(Emp) (3,0) (1)	Δ log(Emp) (2,1) (2)	Δ log(Emp) (1,2) (3)	Δ log(Emp) (0,3) (4)	Δ log(Emp) (1,4) (5)	Δ log(Emp) (2,5) (6)
Δ Lev(-3,0)	0.128**	0.047	0.149**	0.163**	0170**	0.152**
	(0.058)	(0.070)	(0.074)	(0.075)	(0.076)	(0.078)
County × industry × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Establishment fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.32	0.33	0.33	0.35	0.36	0.37
Observations	7,465,800	6,370,600	5,334,600	4,491,000	3,869,700	3,338,600

3.3. Establishment-level employment growth

A firm is a collection of establishments. Hence, we would expect that the same dynamic pattern also holds with respect to establishment-level employment growth. A benefit of studying employment growth at the establishment level is that we can account for regional shocks through the inclusion of highly granular county × industry × year fixed effects (in lieu of the regional control). Accordingly, we compare establishments in the same county, industry, and year that belong to firms with different changes in leverage. Specifically, we estimate the following establishment-level version of Eq. (1):

$$\Delta\ \log(\text{Emp})_{i,j,k,l,t}(t+\tau, t+\tau+3) = \alpha_j + \alpha_k \times \alpha_l \times \alpha_t + \beta\ \Delta\ \text{Lev}_{i,t}(t-3,t) + \varepsilon_{i,j,k,l,t}, \quad (2)$$

where $\Delta\ \log(\text{Emp})_{i,j,k,l,t}(t+\tau, t+\tau+3)$ is the employment growth at establishment j of firm i in county k and industry l from $t+\tau$ to $t+\tau+3$, $\Delta\ \text{Lev}_{j,t}(t-3,t)$ is the change in firm i's leverage from $t-3$ to t, and α_j and $\alpha_k \times \alpha_l \times \alpha_t$ are establishment and county × industry × year fixed effects, respectively. Industries are measured at the 4-digit NAICS code level. Observations are weighted by establishment-level employment. Standard errors are double clustered at the firm and year level.[6]

Table 3 shows the results. As can be seen, the dynamic relation between changes in firm leverage and employment growth also holds at the individual establishment level: following an increase in firm leverage, employment growth at the establishment level is positive in the short run but negative in the medium run. Note that including highly granular county × industry × year fixed effects rules out alternative stories that rely on within county-year variation, such as stories based on county-level household leverage, or county-level shocks more generally, driving firm leverage. Likewise, the fixed effects rule out any stories based on industry shocks, including industry shocks that vary at the county level.

3.4. Local projections

Our main empirical specification considers three-year employment growth at various points in time ("sliding windows"). Alternatively, we may characterize the relation between changes in firm leverage and employment growth using local projections:

$$\Delta\ \log(\text{Emp})_{i,t}(t-3, t+\tau) = \alpha_i + \alpha_t + \beta\ \Delta\ \text{Lev}_{i,t}(t-3,t) + \text{X}_{i,t} + \varepsilon_{i,t}, \quad (3)$$

where $\tau = 0, \ldots, 5$; $\Delta\ \log(\text{Emp})_{i,t}(t-3, t+\tau)$ is firm i's employment growth from $t-3$ to $t+\tau$; $\Delta\ \text{Lev}_{i,t}(t-3,t)$ is the change in firm i's leverage from $t-3$ to t; $\text{X}_{i,t}$ is a regional control; and α_i and α_t are firm and year fixed effects, respectively. The regional control is the same as in Eq. (1), except that employment growth is measured from $t-3$ to $t+\tau$, in accord with the dependent variable. To simplify the notation, we write $\Delta\ \log(\text{Emp})(-3, 0+\tau)$ in lieu of $\Delta\ \log(\text{Emp})_{i,t}(t-3, t+\tau)$ in our tables and figures. Observations are weighted by firm-level employment. Standard errors are double clustered at the state (using the firm's home state) and year level.

Local projections have pros and cons relative to the sliding windows approach in our main specification. A benefit is that they show cumulative changes in firm-level employment. A disadvantage is that cumulative changes mask potentially relevant information, for example, whether the relation between Lev(−3, 0) and Δ log(Emp)(1,4) is statistically significant. As Table 4 shows, regardless of whether we consider six- ($t-3$ to $t+3$), seven- ($t-3$ to $t+4$), or eight-year ($t-3$ to $t+5$) employment growth, the cumulative effect of an increase in firm leverage is that future firm-level employment ends up significantly below its initial level.[7]

4. Alternative channels

4.1. Firm-level expansions

Firms may increase leverage to finance expansions. Accordingly, it is possible that our results are driven by

[6] Clustering at the firm level accounts for the possibility that establishments of the same firm are subject to correlated shocks.

[7] Our results are similar if we drop the regional control. See Table A.5 of the Online Appendix.

Table 4
Local projections.
This table presents variants of the regressions in Table 2 in which the dependent variable, $\Delta log(Emp)(t-3, t+\tau)$, is firm-level employment growth from $t-3$ to $t+\tau$, where τ ranges from $\tau=0$ in column 1 to $\tau=5$ in column 6, and in which the regional control is measured from $t-3$ to $t+\tau$ in accord with the dependent variable. Observations are weighted by firm-level employment. Standard errors (in parentheses) are double clustered at the state (using the firm's home state) and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ log(Emp) (3,0) (1)	Δ log(Emp) (3,1) (2)	Δ log(Emp) (3,2) (3)	Δ log(Emp) (3,3) (4)	Δ log(Emp) (3,4) (5)	Δ log(Emp) (3,5) (6)
Δ Lev(-3,0)	0.153***	0.058**	0.012	0.093**	0.119***	0.105**
	(0.028)	(0.028)	(0.032)	(0.036)	(0.041)	(0.046)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Regional control	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.42	0.48	0.53	0.57	0.60	0.63
Observations	145,600	127,600	111,100	98,900	85,100	74,700

Table 5
Changes in firm leverage and firm-level expansions.
This table presents variants of the regressions in column 1 of Table 2 in which the dependent variable is either the change in Capex (column 1), sales growth (column 2), or growth in the number of establishments (column 3) from $t-3$ to t. Observations are weighted by firm-level employment. Standard errors (in parentheses) are double clustered at the state (using the firm's home state) and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ Capex (3,0) (1)	Δ log(Sales) (3,0) (2)	Δ log(# Est.) (3,0) (3)
Δ Lev(3,0)	0.005**	0.039**	0.029***
	(0.002)	(0.018)	(0.011)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Regional control	Yes	Yes	Yes
R-squared	0.18	0.41	0.44
Observations	145,600	145,600	145,600

(over-)expansions, which are merely correlated with increases in firm leverage. To explore this hypothesis, we consider three measures of firm-level expansions: changes in Capex, (i.e., investment), sales growth, and growth in the number of establishments.

Table 5 shows that, perhaps not surprisingly, changes in firm leverage are indeed correlated with measures of firm-level expansions. Regardless of which measure we use, we find that the correlation with Δ Lev$(-3, 0)$ is positive and significant. In Table 6, we control for measures of firm-level expansions in Eq. (1) to see whether the negative relation between changes in firm leverage and medium-run employment growth is driven by firm-level expansions. In Panel A, we consider changes in investment; in Panel B, we consider sales growth; and in Panel C, we consider growth in the number of establishments. As can be seen, measures of firm-level expansions are either insignificant or positively associated with medium-run employment growth. Importantly, controlling for measures of firm-level expansions does not affect our main results: increases in firm leverage continue to predict negative employment growth in the medium run. Indeed, the coefficient on Δ Lev$(-3, 0)$ is always close to the original coefficient in Table 2. This is true regardless of whether we consider changes in investment, sales growth, or growth in the number of establishments, and regardless of whether we consider Δ log(Emp)(0,3), Δ log(Emp)(1,4), or Δ log(Emp)(2,5). Hence, while expansions may well be the reason for why firms increase their leverage, it is the increase in leverage, not the expansion, that predicts medium-run employment declines.

4.2. Mean reversion in employment growth

Our results could be explained by mean reversion in employment growth. Specifically, buildups in firm leverage may predict negative employment growth in the medium run only because they are positively correlated with employment growth in the short run. To examine this hypothesis, we control for short-run employment growth in Eq. (1). If buildups in firm leverage have no predictive power per se, that is, other than through their correlation with short-run employment growth, then the coefficient on Δ Lev$(-3, 0)$ should become insignificant.

Table 7 presents the results. As is shown, short-run employment growth is positively, not negatively, correlated with medium-run employment growth. Accordingly, there is no mean reversion in firm-level employment growth. Importantly, buildups in firm leverage continue to predict negative employment growth in the medium run if we control for short-run employment growth.[8] While the coefficient on Δ Lev$(-3, 0)$ becomes slightly weaker, it remains significant at the 1% level. This is true regardless of whether we consider Δ log(Emp)(0,3), Δ log(Emp)(1,4), or Δ log(Emp)$(2, 5)$. We thus conclude that buildups in firm leverage have separate predictive power for medium-run employment growth over and above their potential short-run effects.

5. Leverage buildups and financial fragility

Why are buildups in firm leverage associated with subsequent employment declines? In theory models, leverage buildups lead to financial fragility and vulnerabil-

[8] In Table A.6 of the Online Appendix, we use either Δ log(Emp)$(-4, -1)$, Δ log(Emp)$(-5, -2)$, or Δ log(Emp)$(-6, -3)$ as a control in lieu of Δ log(Emp)$(-3, 0)$. The results are similar.

Table 6
Controlling for firm-level expansions.
This table presents variants of the regressions in columns 4 to 6 of Table 2 in which either the change in Capex (Panel A), sales growth (Panel B), or the growth in the number of establishments (Panel C) from $t-3$ to t are included as controls. Observations are weighted by firm-level employment. Standard errors (in parentheses) are double clustered at the state (using the firm's home state) 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 changes in investment			
	Δ log(Emp) (0,3) (1)	Δ log(Emp) (1,4) (2)	Δ log(Emp) (2,5) (3)
Δ Lev(3,0)	0.176***	0.143***	0.085***
	(0.030)	(0.028)	(0.028)
Δ Capex(3,0)	0.264***	0.005	0.027
	(0.045)	(0.042)	(0.042)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Regional control	Yes	Yes	Yes
R-squared	0.44	0.44	0.44
Observations	98,900	85,100	74,700
Panel B: Controlling for sales growth			
	Δ log(Emp) (0,3) (1)	Δ log(Emp) (1,4) (2)	Δ log(Emp) (2,5) (3)
Δ Lev(3,0)	0.177***	0.137***	0.079***
	(0.030)	(0.028)	(0.028)
Δ log(Sales)(3,0)	0.060***	0.057***	0.056***
	(0.005)	(0.005)	(0.005)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Regional control	Yes	Yes	Yes
R-squared	0.44	0.44	0.44
Observations	98,900	85,100	74,700
Panel C: Controlling for growth in the number of establishments			
	Δ log(Emp) (0,3) (1)	Δ log(Emp) (1,4) (2)	Δ log(Emp) (2,5) (3)
Δ Lev(-3,0)	-0.179***	-0.144***	-0.085***
	(0.030)	(0.028)	(0.028)
Δ log(#Est.)(-3,0)	-0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Regional control	Yes	Yes	Yes
R-squared	0.44	0.44	0.44
Observations	98,900	85,100	74,700

ity to shocks (e.g., Bernanke and Gertler, 1989; Kiyotaki and Moore, 1997; Bernanke et al., 1999). When a shock hits, levered units (households, firms) are forced to reduce demand (for goods or capital/labor inputs). This mechanism is central to virtually any model of overborrowing and leverage cycles, including models of belief dispersion (optimism vs. pessimism) and biased expectations (excessive optimism).[9] Crucially, in these models, built-in amplification (or "accelerator") mechanisms ensure that even

Table 7
Mean-reverting employment growth.
This table presents variants of the regressions in columns 4 to 6 of Table 2 in which firm-level employment growth from $t-3$ to t is included as a control. Observations are weighted by firm-level employment. Standard errors (in parentheses) are double clustered at the state (using the firm's home state) and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ log(Emp)	Δ log(Emp)	Δ log(Emp)
	(0,3) (1)	(1,4) (2)	(2,5) (3)
Δ Lev(3,0)	0.165***	0.121***	0.069***
	(0.030)	(0.028)	(0.028)
Δ log(Emp)(3,0)	0.018***	0.009***	0.007**
	(0.003)	(0.003)	(0.003)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Regional control	Yes	Yes	Yes
R-squared	0.44	0.44	0.44
Observations	98,900	85,100	74,700

small shocks can trigger large downturns in real economic activity.

Below we provide some limited evidence suggesting that the dynamic relation between changes in firm leverage and employment growth operates through a financial fragility channel. We first document that buildups in firm leverage predict a tightening of future financing constraints, and we obtain similar results if we consider covenant violations. We subsequently show that buildups in firm leverage only predict medium-run employment drops if the level of firm leverage is sufficiently high. This is perhaps the strongest evidence in support of a financial fragility channel: an increase in firm leverage predicts negative medium-run employment growth if the leverage ratio is high, but it does not predict negative medium-run employment growth if the leverage ratio is low.

Panel A of Table 8 is similar to our baseline estimation in Table 2, except that the dependent variable represents changes in the SA-index (Hadlock and Pierce, 2010), a widely used measure of financing constraints. As can be seen, increases in firm leverage predict tighter future financing constraints. In Panel B, we approach the relation between leverage buildups and financing constraints from a different angle: if buildups in firm leverage matter for future employment growth because they affect future financing constraints, then including changes in future financing constraints as a control should weaken the coefficient on firm leverage buildups. As is shown, this is indeed true; the coefficient on Δ Lev$(-3,0)$ drops by about 40% relative to that in Table 2. Lastly, Panel C is similar to Panel A, except that we replace the SA-index with a tangible measure of financing constraints: covenant violations. Indeed, covenant violations have been shown to trigger employment losses (Falato and Liang, 2016). As can be seen, buildups in firm leverage predict covenant violations several years later.

[9] See the literature cited in the Introduction. In the context of firms, empirical studies find that CEOs are overly optimistic (e.g., Malmendier and Tate (2015)) and, in particular, that firms with overoptimistic CEOs have higher leverage (Malmendier et al., 2011).

Table 8
Financing constraints.
Panel A presents variants of the regressions in Table 2 in which the dependent variable is the change in the SA-index from $t+\tau$ to $t+\tau+3$, where τ ranges from $\tau=-3$ in column 1 to $\tau=2$ in column 6. The SA-index is described in Hadlock and Pierce (2010). Panel B presents variants of the regressions in columns 4 to 6 of Table 2 in which the change in the SA-index from $t+\tau$ to $t+\tau+3$ is included as a control, where τ ranges from $\tau=0$ in column 1 to $\tau=2$ in column 3. Panel C presents variants of the regressions in Table 2 in which the dependent variable is a dummy that equals one if a covenant is violated between $t+\tau$ and $t+\tau+3$, where τ ranges from $\tau=-3$ in column 1 to $\tau=2$ in column 6. The data on covenant violations are from Amir Sufi's website and described in Nini et al. (2012). Observations are weighted by firm-level employment. Standard errors (in parentheses) are double clustered at the state (using the firm's home state) and year level. The sample period is from 1976 to 2011, except in Panel C, where the sample period is from 1996 to 2008. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel A: Predicting changes in the SA-index

	Δ SA-index (3,0) (1)	Δ SA-index (2,1) (2)	Δ SA-index (1,2) (3)	Δ SA-index (0,3) (4)	Δ SA-index (1,4) (5)	Δ SA-index (2,5) (6)
Δ Lev(3,0)	0.010	0.022	0.037*	0.034*	0.046**	0.042**
	(0.016)	(0.018)	(0.019)	(0.019)	(0.021)	(0.020)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Regional control	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.38	0.39	0.36	0.37	0.37	0.38
Observations	145,600	127,600	111,100	98,900	85,100	74,700

Panel B: Controlling for changes in the SA-index

	Δ log(Emp) (0,3) (1)	Δ log(Emp) (1,4) (2)	Δ log(Emp) (2,5) (3)
Δ Lev(3,0)	0.111***	0.084***	0.045*
	(0.026)	(0.025)	(0.026)
Δ SA-index(0,3)	0.514***		
	(0.093)		
Δ SA-index(1,4)		0.512***	
		(0.091)	
Δ SA-index(2,5)			0.497***
			(0.100)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Regional control	Yes	Yes	Yes
R-squared	0.46	0.46	0.46
Observations	98,900	85,100	74,700

Panel C: Predicting covenant violations

	Covenant violation (-3,0) (1)	Covenant violation (-2,1) (2)	Covenant violation (-1,2) (3)	Covenant violation (0,3) (4)	Covenant violation (1,4) (5)	Covenant violation (2,5) (6)
Δ Lev(-3,0)	0.022	0.039**	0.087***	0.057***	0.041**	0.024
	(0.015)	(0.015)	(0.017)	(0.016)	(0.018)	(0.017)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Regional control	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.51	0.52	0.52	0.51	0.51	0.51
Observations	49,700	44,900	40,200	36,100	31,300	28,100

Lastly, we interact changes in firm leverage with the level of firm leverage in $t-3$. As Table 9 shows, the relation between changes in firm leverage and medium-run employment growth is monotonic in the level of firm leverage and only significant if the level of firm leverage is sufficiently high. Notably, the point estimate in the highest firm leverage tercile is between 4.5 and 7.5 times larger than the corresponding point estimate in the lowest firm leverage tercile.

6. Aggregate regional employment dynamics

Our result that buildups in firm leverage predict subsequent declines in employment may not matter in the aggregate if workers laid off by firms with high leverage buildups are re-employed by other firms. Whether, and to what extent, our results have aggregate implications ultimately depends on search and matching frictions in the labor market as well as labor adjustment (e.g., hiring, firing, and training) costs.

Table 9
Interactions with levels of firm leverage.
This table presents variants of the regressions in columns 4 to 6 of Table 2 in which $\Delta Lev(-3,0)$ is interacted with dummy variables indicating whether the level of firm leverage in $t-3$ lies in the first, second, or third tercile of its empirical distribution based on all firm-year observations. Observations are weighted by firm-level employment. Standard errors (in parentheses) are double clustered at the state (using the firm's home state) and year level. The sample period is from 1976 to 2011. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ log(Emp) (0,3) (1)	Δ log(Emp) (1,4) (2)	Δ log(Emp) (2,5) (3)
Δ Lev(3,0) × Lev(3)(1st tercile)	0.062	0.039	0.023
	(0.040)	(0.038)	(0.043)
Δ Lev(3,0) × Lev(3)(2nd tercile)	0.161***	0.170***	0.065*
	(0.037)	(0.038)	(0.038)
Δ Lev(3,0) × Lev(3)(3rd tercile)	0.289***	0.237***	0.173***
	(0.032)	(0.033)	(0.035)
Lev(3)(2nd tercile)	0.005	0.002	0.003
	(0.010)	(0.011)	(0.011)
Lev(3)(3rd tercile)	0.008	0.003	0.006
	(0.012)	(0.013)	(0.012)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Regional control	Yes	Yes	Yes
R-squared	0.44	0.44	0.44
Observations	98,900	85,100	74,700

To see if our results hold at the aggregate level, we consider aggregate county-level employment. Importantly, we consider county-level employment by all firms in a county: publicly listed and unlisted firms. To obtain a measure of firm leverage at the county level ("county leverage"), we compute the weighted average leverage ratio across all publicly listed firms with establishments in the county. Weights are based on the firms' county-level employment shares. Given that our measure of county leverage is based on publicly listed firms, which only account for 12.8% of county-level employment, we would expect the coefficient on Δ Lev$(-3,0)$ to be biased downward. We estimate the following county-level version of Eq. (1):

$$\Delta \log(\text{Emp})_{k,t}(t+\tau, t+\tau+3) = \alpha_k + \alpha_t + \beta\ \Delta\ \text{Lev}_{k,t}(t-3,t) + \varepsilon_{k,t}, \quad (4)$$

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 $t+\tau+3$; $\Delta\ \text{Lev}_{k,t}(t-3,t)$ is the change in county k's leverage from $t-3$ to t; and α_k and α_t are county and year fixed effects, respectively. Observations are weighted by county-level employment. Standard errors are double clustered at the county and year level.

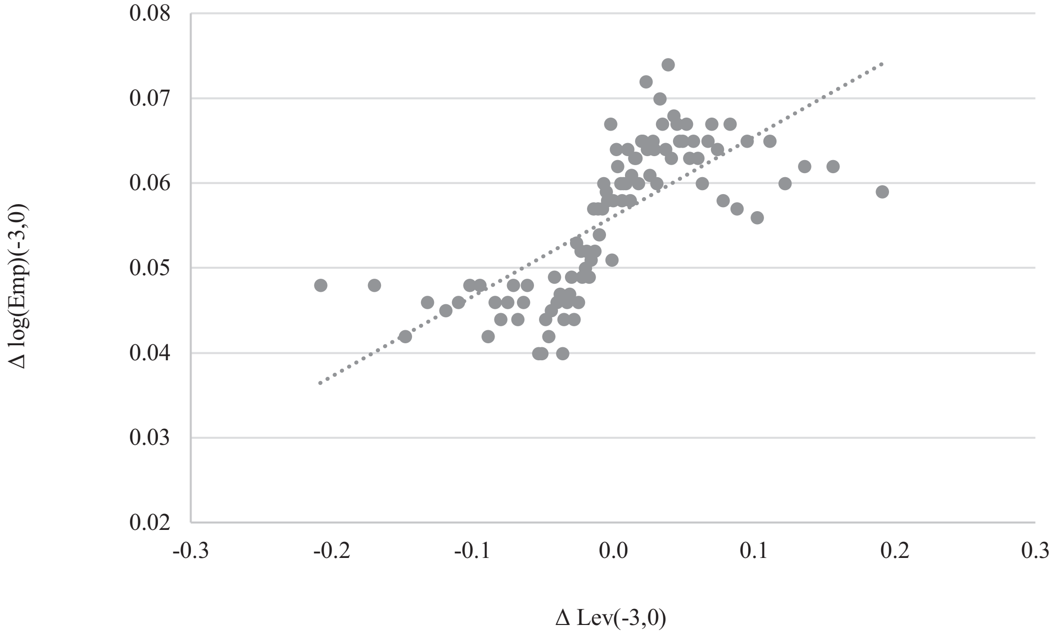
Fig. 2 is the counterpart of Fig. 1 at the county level. Panel A shows the short-run relation between changes in county leverage and county-level employment growth—the correlation between Δ Lev$(-3,0)$ and Δ log(Emp)$(-3,0)$—based on 99,300 county-year observations. Panel B shows the medium-run relation—the correlation between Δ Lev$(-3,0)$ and Δ log(Emp)(1,4)—based on 86,500 county-year observations. As in Fig. 1, the short- and medium-run relations have opposite signs. In the short run, a one standard deviation increase in county leverage (0.080) is associated with a 0.8% increase in county-level employment. By contrast, in the medium run, a one standard deviation increase in county leverage predicts a 0.5% decline in county-level employment. Table A.7 of the Online Appendix presents the regressions that correspond to the bin scatterplots in Fig. 2.

Table 10 is the county-level analogue of our firm-level results in Table 2 based on estimating Eq. (4). As in our firm-level analysis, the relation between changes in county leverage and county-level employment growth is positive in the short run but negative in the medium run. The magnitudes are similar to those in Fig. 2. In column 1, for example, a one standard deviation increase in county leverage is associated with a 0.5% short-run increase in county-level employment, whereas in column 5, it predicts a 0.4% medium-run decline in county-level employment.

The Online Appendix provides robustness tests. Table A.8 shows our main county-level results separately for tradable industries (Panel A), non-tradable industries (Panel B), and all other industries (Panel C).[10] All variables are industry-specific. For example, in Panel A, county leverage is the weighted average leverage ratio across all publicly listed firms with tradable establishments in a county, where the employment weights are based on firms' shares of tradable county-level employment. Likewise, employment growth is the growth rate of tradable employment in a county. In Table A.9, we consider broader definitions of regions: MSAs (Panel A) and states (Panel B). 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 example, a one standard

[10] The industry classification follows Mian and Sufi (2014). They classify an industry as tradable if imports plus exports exceed $10,000 per worker or $500 million in total. Retail industries and restaurants are classified as non-tradable. Industries that are neither tradable nor non-tradable are classified as "other."

Panel A: Changes in county leverage and short-run county-level employment growth



Panel B: Changes in county leverage and medium-run county-level employment growth

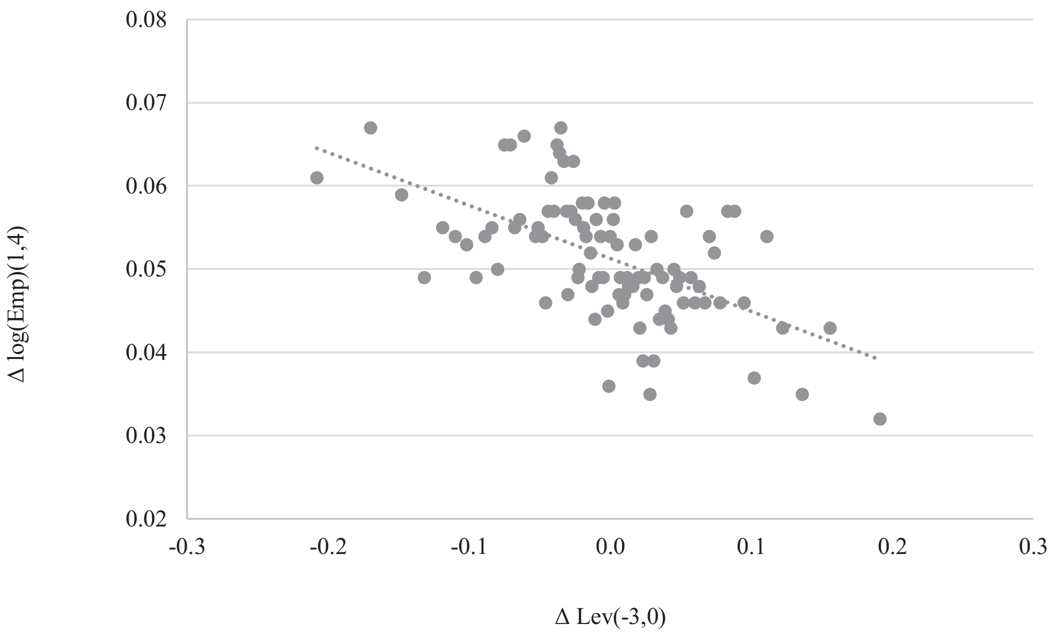


Fig. 2. County-level bin scatterplots. This figure presents variants of the bin scatterplots in Fig. 1 in which changes in leverage and employment growth are measured at the county level. County-level employment growth is based on all (publicly listed and unlisted) firms in a county.

deviation increase in MSA leverage predicts a subsequent decline in MSA-level employment of 0.4%, while a one standard deviation increase in state leverage predicts a subsequent decline in state-level employment of 0.8%.

We finally examine whether regions with larger buildups in firm leverage perform worse during (national) recessions. Recessions constitute significant aggregate shocks. Hence, they provide an ideal laboratory to

Table 10
County-level employment growth.
This table presents variants of the regressions in Table 2 in which changes in leverage and employment growth are measured at the county level and the regional control and firm fixed effects are replaced with county fixed effects. County leverage is the weighted average leverage ratio across all publicly listed firms with establishments in the county. Weights are based on the firms' county-level employment shares. County-level employment growth is based on all (publicly listed and unlisted) firms in a county. Observations are weighted by county-level employment. Standard errors (in parentheses) 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(Emp) (3,0) (1)	Δ log(Emp) (2,1) (2)	Δ log(Emp) (1,2) (3)	Δ log(Emp) (0,3) (4)	Δ log(Emp) (1,4) (5)	Δ log(Emp) (2,5) (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

study whether leverage buildups bring about fragility and vulnerability to shocks.[11] The NBER records five recessions during our sample period: 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. 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.

In Table 11, we examine whether counties with larger buildups in firm leverage before a recession experience larger employment declines during the recession. Year "0" denotes the year before a recession. For example, in Panel A, Δ Lev$(-3,0)$ is the change in county leverage from 1976 to 1979 and Δ log(Emp)(1,2) is the county-level employment growth from 1980 to 1981. As can be seen, counties with stronger buildups in firm leverage prior to a recession experience larger employment losses in the recession. While this holds for all recessions, there are some differences. In the 1980–1982 and 2001 recessions, a one standard deviation increase in county leverage (0.060 and 0.075) prior to the recession is associated with a 0.4% decline in county-level employment during the recession. The effect is much stronger in the 1990–1991 recession: a one standard deviation increase in county leverage (0.076) is associated with a drop in county-level employment of 1.1%. Finally, the effect is strongest in the 2007–2009 ("Great") recession: a one standard deviation increase in county leverage (0.105) before the Great Recession is associated with a 1.5% decline in county-level employment during the Great Recession.[12]

Table 11
County-level employment growth during recessions.
This table presents cross-sectional variants of the regressions in Table 10 in which the dependent variable is county-level employment growth from $t+1$ to $t+1+\tau$, where τ ranges from $\tau=1$ in column 1 to $\tau=3$ in column 3, and $t=0$ is the year immediately before a national recession. For example, in Panel A, $\Delta Lev(-3,0)$ is the change in county leverage from 1976 to 1979 and $\Delta log(Emp)(1,2)$ is the growth in county-level employment from 1980 to 1981. For brevity, the table only displays the coefficients and standard errors associated with $\Delta Lev(-3,0)$. Observations are weighted by county-level employment. Standard errors (in parentheses) are robust standard errors. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Δ log(Emp) (1,2) (1)	Δ log(Emp) (1,3) (2)	Δ log(Emp) (1,4) (3)
	Panel A: 1980-82 recession		
Δ Lev(3,0)	0.039***	0.065***	0.076***
	(0.010)	(0.014)	(0.013)
	Panel B: 1990-91 recession		
Δ Lev(3,0)	0.124***	0.148***	0.150***
	(0.011)	(0.016)	(0.013)
	Panel C: 2001 recession		
Δ Lev(3,0)	0.067***	0.056***	0.030
	(0.014)	(0.018)	(0.020)
	Panel D: 2007-09 recession		
Δ Lev(3,0)	0.133***	0.139***	0.147***
	(0.032)	(0.038)	(0.035)

7. Conclusion

Using U.S. establishment-, firm-, and region-level data, we find that buildups in firm leverage are predictably associated with boom-bust growth cycles: employment grows in the short run but declines in the medium run. Firm leverage buildups continue to predict medium-run employment declines if we control for short-run em-

[11] While recessions constitute significant aggregate shocks, it is important to note that our results are not driven by recessions. There are 56 contraction months and 364 expansion months in our sample period (see http://www.nber.org/cycles.html). In contrast, increases and decreases in county leverage are fairly balanced—about 53% of observations constitute increases and 47% constitute decreases.

[12] All magnitudes discussed in this paragraph are based on column 2, which shows the relation between Δ Lev$(-3,0)$ and Δ log(Emp)(1,3). That the magnitudes are largest for the Great Recession is not surprising. The Great Recession witnessed significant drops in consumer demand due to falling house prices. Consistent with leverage buildups creating fragility and vulnerability to shocks, Giroud and Mueller (2017) find that establishments of firms with higher leverage in 2006, at the onset of the Great Recession, were relatively more sensitive to consumer demand shocks during the Great Recession.

ployment growth or measures of short-run firm-level expansions. While we are not making causal claims, the evidence presented here suggests that the dynamic relation between firm leverage buildups and employment growth operates through a financial fragility channel: increases in firm leverage unconditionally predict a tightening of future firm-level financing constraints, and they only predict negative medium-run employment growth if the level of firm leverage is sufficiently high.

Our main result that buildups in firm leverage predict declines in real economic activity stands in contrast to findings in the cross-country literature. Using aggregate data from 30 countries, Mian et al. (2017, MSV) find that increases in the ratio of household debt to GDP, but not increases in the ratio of (non-financial) firm debt to GDP, predict negative medium-run GDP growth. There are several possible reasons for this difference. For one, our sample goes back to 1976, whereas MSV's sample goes back to 1960. Second, we use data from a single country, the United States, while MSV use data from 30 countries. Lastly, we exploit variation in leverage buildups at the establishment-, firm-, and regional level using disaggregated data, whereas MSV exploit variation in leverage buildups at the country level using aggregate data.

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