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Labor Unions and Corporate Financial Leverage: The Bargaining Device versus Crowding-out Hypotheses

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ABSTRACT

We examine the empirical relation between labor unions and firm indebtedness in the contemporary United States. Our identification strategy exploits two negative exogenous shocks in union power and the threat of unionization. Further, in the context of panel regressions, we develop a novel firm-level proxy for the bargaining power of labor using collective bargaining information from mandatory IRS filings from 1999 to 2013. Across a battery of tests, we document evidence in favor of a crowding-out hypothesis - namely, a substitution effect between labor power and financial leverage. Notably, this effect is more pronounced in firms in labor-intensive and unionized industries.

JEL classification: J31, J51, G32, G33, K31.

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In this paper, we exploit both a theoretical tension and a quasi-natural experimental design to uncover new and meaningful insights on the empirical relation between labor union power and corporate financial leverage. Our study is motivated from the view that, despite a decline in trade unionism in the United States over recent decades, labor unions still have potentially significant effects on corporate decision-making and firm outcomes. Notably, the capital structure literature has advanced two competing views regarding the relation between the bargaining power of labor and financial leverage: (a) leverage used as a bargaining device; (b) a labor vs. leverage crowding-out effect. While plausible arguments can be mounted in either of these directions, it becomes an empirical question whether/which one of these views dominates the other in contemporary market settings.

The *bargaining device* view predicts a positive relation between unionization and financial leverage. Specifically, this perspective argues that financial leverage operates as a strategic tool, limiting the appropriation of rents by workers (see, e.g., Matsa (2010), Bronars and Deere (1991)). The general intuition behind these models is straight-forward. Unions and managers effectively bargain over the distribution of future cash flows. Before meeting at the bargaining table, the manager can lever up the firm and simultaneously return capital to shareholders (e.g., through a leveraged buyback or special dividend)—thereby capturing a portion of future cash flows. Further, the increase in leverage moves the firm closer to financial distress/bankruptcy, at some point reducing the quantum of rents that can be extracted by labor without risking wide-spread worker displacement. Critically, these bargaining strategies are not only appealing to currently unionised firms but (because workers can easily organize themselves into a union) are also relevant to those non-unionized firms that are facing the real *threat* of unionization (Bronars and Deere, 1991). Thus, the fundamental testable implication of these models is that financial leverage is increasing in union power and the threat of unionization—which we label the *bargaining device* hypothesis.

In contrast, the *crowding-out* perspective generates a negative labor-leverage prediction. The intuition begins with the premise that unions impose economically significant costs on firms. Of course, the most obvious cost is the union wage premium. In fact, the general consensus is that the union wage premium is historically about 15% in the United States (Aidt and Tzannatos, 2002). Additionally, union workers are more likely to receive costly fringe benefits such as severance pay, paid holidays, paid sick leave, and access to pension plans. Because the financial claims of labor out-rank debt-holders in the event of default, these increased costs can be viewed as cash claims on a super-senior "debt-like" contract. Accordingly, these claims "crowd-out" financial leverage, thereby reducing the firm's debt capacity. Moreover, unions also induce a degree of operating inflexibility in that they reduce the manager's discretion in hiring and firing decisions. This operating inflexibility is hypothesized to reduce financial leverage (see, e.g., (Kuzmina, 2013, Simintzi, Vig, and Volpin, 2015)). In combination, these forces deliver a *crowding-out* hypothesis predicting a substitution effect between financial leverage and unionization.

We execute a battery of tests which pit these two competing perspectives against one another, in the contemporary United States using firm-level data. Exploring this US context is particularly important because major relevant changes have occurred in both labor and capital markets which could have plausibly (and non-trivially) transformed the process driving previous empirical findings. For example, although Matsa (2010) documents a positive relation between the bargaining power of labor and financial leverage, his findings are largely based on quasi-experiments employing data drawn from a very different, and now somewhat historical, era (in the 1950s - 1970s). A complete discussion of these contextual changes is deferred to Section 1.

Our identification strategy involves two parts. Our primary approach exploits exogenous variation in both union power and the threat of unionization using Right-to-Work legislation in Oklahoma (2001) and Indiana (2012). We estimate the effects of the passage of these laws on leverage using both difference-in-differences and a matching experiment. To supplement this quasi-natural experimental design and to estimate the effect of union power on financial leverage, we develop a novel measure of union power using mandatory IRS filings. Armed with this new variable, we examine the relation between firm-level union power and financial leverage using panel regressions with high-dimensional fixed effects (1999 to 2013).

Our main findings are readily conveyed. Both our quasi-experiments as well as our panel regressions concordantly reveal a *negative* relation between the bargaining power of labor and financial leverage, that is, we find evidence favoring the crowding-out hypothesis. Moreover, this labor-leverage substitution effect is substantially stronger in firms which are more vulnerable to rent-seeking (i.e., firms operating in labor-intensive and traditionally union-intensive industries). We also construct a test (albeit informal) designed to challenge the view that the mechanism driving this negative relation is indeed related to excess labor costs. We are unable to falsify this mechanism.

Our paper contributes to the literature on capital structure and labor by documenting robust evidence of a negative relation between the bargaining power of labor and financial leverage in the contemporary United States. We argue that our findings reflect the fact that (for reasons fully explained in the paper) the "rules of the game" have changed and, accordingly, that the bargaining device channel is no longer dominant. More broadly, we contribute to the growing literature on the effects of labor on corporate financial decisions. Furthermore, we overcome the challenge of finding a readily available and conceptually sound firm-level proxy for the bargaining power of labor using information derived from annual mandatory IRS filings. This novel measure brings significant advantages over extant measures.

The remainder of our paper is organized as follows. Section 1 lays out the paper's motivation and provides a brief review of the relevant literature. Section 2 outlines our quasi-experimental design and presents the results. Section 3 explains and presents the results of our panel regression model, and finally, Section 4 concludes.

1 Motivation and Literature Review

In one of the earliest papers on capital structure and labor, Bronars and Deere (1991) develop a model which predicts that firms who face a higher *threat* of unionization will adopt more aggressive leverage policies. The fundamental idea being that if bankruptcy is costly to workers, debt can be used to protect shareholder wealth from union wage demands. In their wage-setting model, workers balance the marginal costs of bankruptcy (i.e., job loss) with the marginal benefits of a higher wage. The wage they select is inversely related to the level of leverage. Using industry-level collective bargaining information, they find evidence of a positive relation between the threat of unionization and financial leverage at the cross-section.

Similarly, Matsa (2010) also finds evidence in favor of the bargaining device perspective by analyzing the effects of state labor regulations in the 1950s (i.e., Right-to-Work laws), 1960s, and early 1970s (i.e., work stoppage provisions in unemployment insurance laws). However, we argue that non-trivial changes in both labor and capital markets have plausibly affected the underlying processes driving previously observed relations.

One such significant change was the introduction of reorganization procedures under Chapter 11 of Title 11 of the United States Bankruptcy Code in 1978. This legislation provides that any business can file for bankruptcy protection, allowing the debtor to retain control of the business and to have six months to propose a reorganization plan. It also imparts other salient rights such as the ability to refinance with new lenders and grant them priority over existing creditors. These new provisions increase the firm's bargaining power vis-à-vis its' creditors and can enable the firm to force creditors into accepting an unfavorable renegotiation (Tirole, 2006). In fact, under Chapter 11, labor unions may be eligible for positions on creditor committees. The Committee has a variety of powers including the ability to investigate debtors for fraud and incompetence as well as to request the dismissal of managers (Campello, Gao, Qiu, and Zhang, 2015). Indeed, Campello, Gao, Qiu, and Zhang (2015) show that unionized firms are 15% more likely to emerge from bankruptcy (though union firms are also 6% more likely to refile—indicating that labor unions can lead to inefficient reorganizations). Thus, Chapter 11 can be thought of as effecting two relevant changes. First, it increased the inherent renegotiability of debt and, second, it increased the influence of labor in default proceedings. Taken together, these factors plausibly and significantly blunt the effect of debt as a bargaining device. Consistent with the notion that financial debt is no longer an effective bargaining device, Brown, Fee, and Thomas (2009) find that leveraged recapitalizations are not associated with negative abnormal returns to suppliers.

However, a second, and perhaps even more significant change was the emergence of plant-level, rather than purely multi-employer and firm-level collective bargaining. This new form of bargaining could have made redundant 'strategic debt' by allowing the development of alternative dominant strategies that do not require the use of financial debt. For example, in a plant-level setting the firm is able to threaten to downsize by closing one or more local plants. This threat initiates a zero-sum game between plants in which local unions are forced to out-concede each other. Consistent with this conjecture, there are several pieces of persuasive anecdotal evidence in the early 1990s that managers threatened to close local plants to extract concessions from workers (Katz, 1993).[1] To this same end, advances in technology

[1]As an additional example, Boeing shifted work from union plants in Washington State to a new non-union

since the 1970s have resulted in improved capital mobility (Eschuk, 2002). This improvement allows more firms to credibly threaten to relocate plants or to even outsource abroad if workers attempt to extract excessive quasi-rents.

These alternative bargaining strategies are intuitively more appealing than the issuance of financial debt when one considers the inherent moral hazard that arises between managers and stockholders. Specifically, bankruptcy is a negative state of nature not only for non-executive labor but also for top management. For example, Gilson (1989) finds that financial distress increases managerial turnover from 19% to 52%. Further, Hotchkiss (1995) finds that 70% of CEOs are replaced by the time a reorganization plan is implemented under Chapter 11. Taken as a whole, it is hard to imagine why management would prefer to take a personal risk for the benefit of stockholders when they could simply rely upon the tactics made available by decentralization and increased capital mobility. Unlike debt, these new alternatives do not increase the probability of executive turnover, while still extracting concessions from non-executive labor.[2] Notably, the CFO survey of Graham and Harvey (2001) fails to find substantial evidence that the bargaining advantage of debt is a consideration in capital structure decisions.

These market and regulatory changes, coupled with recent evidence on the use and efficacy of debt as a bargaining device motivates our study of the relation between unionization, the *threat* of unionization and financial leverage in the contemporary United States.

2 Quasi-Experimental Design

Our analysis begins with a quasi-experimental design exploiting two separate, state-level negative exogenous shocks in the bargaining power of labor. Specifically, our design begins with the analysis of Right-to-Work legislation (RTW) in Oklahoma in 2001 using the Difference-in-differences estimator. Oklahoma represents the cleanest regulatory change event relevant to our research question. First, it is the only RTW event since the 1970s not confounded by corporate taxation effects. And second, it also coincides with a significant increase in the private sector union wage premium (Blanchflower and Bryson, 2004), increasing the potential significance of

factory in South Carolina (Trottman, 2011).

[2] It is important to note that we are not suggesting that the moral hazard *prevents* the use of debt as a bargaining device as in Matsa (2010). We are, however, suggesting that in its presence managers will exploit these new alternative strategies instead.

a policy intervention designed to weaken the bargaining power of labor.

2.1 Right-to-Work and Bargaining Power

Right-to-Work legislation provides that employees cannot be contractually compelled to join or financially support a union even if they directly benefit from the actions of the union (e.g., a union shop clause). This legislative change makes it very difficult for the union to avoid the free-rider problem. This reduces the amount of funding available per worker covered by the union. Most importantly, this reduction in funding limits the union's ability to contract high quality experts (e.g., law firms) to assist in negotiations, thereby reducing the bargaining power of organized labor (Matsa, 2010).[3]

However, RTW legislation also unambiguously reduces the bargaining power of *non-organized* labor by reducing the *threat* of unionization. This diminution of power occurs because the advent of the free-rider problem reduces the net present value of the union formation investment by workers. This exogenous reduction in the *threat* of unionization allows firms to pay their non-organized workers lower wages. Indeed, consistent with this, Farber (2005) provides evidence that the passage of Right-to-Work laws led to a reduction in non-union wages in both Idaho (1987) and Oklahoma (2001).[4]

When considering the bargaining device literature, these two simultaneous effects generate a simple prediction: Both unionized and non-unionized firms in the treated state are expected to *reduce* their level of financial leverage following the intervention. Right-to-Work laws exogenously reduce both union power *and* the threat of unionization. Conversely, the crowding-out hypothesis predicts that all firms will *increase* leverage as the diminution in union power and the threat of unionization reduces expected payouts to union and non-union workers. This moderates the crowding-out effect of excess wages, allowing firms to take on more debt. More formally:

H1a (Bargaining Hypothesis): firms reduce their financial leverage following the passage of Right-to-Work laws.

H2a (Crowding-out Hypothesis): firms increase their financial leverage following the passage

[3]Eren and Ozbeklik (2011) find that RTW laws caused a 14.5% reduction in union membership in Oklahoma.

[4] Though results for Oklahoma were statistically insignificant—probably because he uses Current Population Survey data terminating in 2002, which was one year after the adoption of the RTW legislation in Oklahoma.

of Right-to-Work laws.

2.2 Right-to-Work in Oklahoma

2.2.1 Data

We form our main sample using a Compustat file containing a total of 51,173 firm-years from 1998 ($t-3$) to 2004 ($t+3$), being three years before and three years after the adoption of RTW legislation in Oklahoma in 2001 ($t = 0$). Following standard practice, we drop all firm-years for which the primary sector is financial (SIC: 6000–6999) or public utilities (SIC: 4900–4999) as well as firm-years with missing data for dependent or independent variables. Additionally, we drop non-U.S. firms and firm-years with negative assets, shareholder equity, and minority interests (balance sheet). This leaves us with 22,527 firm-year observations.

2.2.2 Difference-in-Differences Setup

The first estimation technique we employ is the difference-in-differences estimator ("DID") with additional covariates and fixed effects. As described above, because Right-to-Work laws affect *both* union and non-union employees, we cannot use non-union firms as a within-state control group. To this end, the chosen specification is given by:

$$Flev_{i,t} = \alpha_0 + \beta_1 RTW + \delta_0 POST + \delta_1 RTW \times POST + \beta_k X_{k,i,t-1} + \gamma_{j,t} + \eta_i + \epsilon_{i,t} \tag{1}$$

where i, j, and t index firms, industries, and years; $Flev_{i,t}$ represents a chosen measure of financial leverage. Following the literature, we use the ratio of financial debt to book assets as our primary choice to proxy the dependent variable. RTW is an indicator variable coded 1 for firms incorporated in Oklahoma, and 0 otherwise; POST is an indicator variable coded 1 for observations after 2001, being the year of the legislative change and 0 otherwise.

The object $X_{k,i,t-1}$ is a vector of time-variant financial controls typically used in leverage studies and includes firm size, market-to-book, return on assets, asset tangibility, and modified Z-score (see, e.g., (Matsa, 2010, Rajan and Zingales, 1995)). These variables are defined in Panel B1 of Appendix B. The inclusion of covariates accounts for the possibility that the samples within a group have systematically different characteristics in the two time periods

(Wooldridge, 2010).[5]

The parameter $\gamma_{j,t}$ represents industry-by-year fixed effects for industry j at time t. The inclusion of industry-by-year fixed effects avoids the potential confounding linked to unobserved time-varying industry-level effects (Heider and Ljungqvist, 2015). Additionally, and consistent with Matsa (2010), we include firm fixed effects, represented by η_i, to avoid confounding by time-invariant firm-level heterogeneity. Standard errors are clustered by state, allowing for unspecified within-state correlation and are heteroskedastic-consistent. Further, in additional tabulated robustness tests, we exclude those firms most likely to have been affected by the Dot-com bubble (SIC 737) as well as those firm-years in which the sampled firm is not headquartered in either Oklahoma or one of its' neighboring states (Arkansas, Colorado, Kansas, and Missouri, New Mexico, and Texas).

2.2.3 Descriptive Statistics

Panel A of Table 1 reports summary statistics for the alternative proxies for our dependent variable—financial leverage, while Panel B presents summary statistics for our chosen determinants of financial leverage between 1998 and 2004. On average, a sample firm holds about 15% (20%) of long-term debt relative to its' total assets (book capital). When we include short-term debt in our financial leverage measures, the recorded value of a firm's debt holdings increases by about 5-6%. On average, a sample firm has $1,045 million in net sales, ROA of 4%, 25% of its' total assets in fixed assets, and a market-to-book ratio of 2.00. The average corporate default spread is about 0.89% in our sample period. Further, the growth in gross state product is 3.78%, while the average state unemployment rate is 4.87%. Notably, the descriptive statistics reported above are qualitatively similar to prior capital structure studies such as Heider and Ljungqvist (2015).

[Place Table 1 about here]

[5]To ensure that our results are not driven by different specifications from Matsa (2010), we follow his specification closely in our paper. For example, we include the same set of control variables, firm fixed effects, industry by year fixed effects in our leverage regression and cluster standard errors by state.

2.2.4 Firm Headquarter Locations

Because Compustat only reports the address of the firm's *current* headquarters, the use of the 'state' variable reported in the file results in an imperfect assignment of firms to treatment and control groups (Heider and Ljungqvist, 2015). For example, if a firm is currently headquartered *outside* Oklahoma but was headquartered *in* Oklahoma during the treatment period, it would be erroneously assigned to the control group (i.e., *misidentified as control*). Similarly, if a firm is currently headquartered in Oklahoma but was *not* headquartered in Oklahoma during the treatment period, it would be erroneously assigned to the treatment group (i.e., *misidentified as treated*). Both types of misidentification, as just described, have the effect of biasing δ_1 (our DID estimator in equation (1)) towards zero, a type II error. In other words, *ex-ante*, this has the effect of *increasing* the probability of finding in favor of the null.

To fully correct for this, we would need to manually verify the headquarter location for all firm-years using 10-Q forms.[6] Given that this is an extremely onerous task (i.e., involving over 22,000 observations) *and* it is not perceived to be a material threat to the sign or significance of our findings *ex-post*, we elect to fully correct for 'misidentified as treatment' errors but only correct for 'misidentified as control' errors for Oklahoma's six neighboring states (New Mexico, Missouri, Colorado, Arkansas, Kansas, and Texas). To do so, we manually verify the headquarter location using SEC's EDGAR service for each relevant firm-year using the most recent 10-Q form. These checks yield seven observations that would have been 'misidentified as treated' (3.7% of the treatment group), and 15 observations that would have been 'misidentified as control' (7.9% of the treatment group). We also identify eight observations in which the firm moved its' headquarters overseas as well as 472 observations in which firms headquartered in a control state had relocated to another control state. We correctly reassign the 22 treatment observations and delete the eight overseas observations, yielding a corrected sample of 22,519 firm-years. This corrected sample includes 49 unique Oklahoman firms, with an average of 27 in each year (minimum of 22).[7]

[6] Heider and Ljungqvist (2015) find that 10.1% of firm-years in their sample from 1989 to 2011 have incorrect headquarters location on Compustat. However, most of these errors will *not* correspond to a misidentification as they will relate to firms in control states relocating to other control states.

[7] To reduce any concerns that our results are driven by differences in the composition of our Oklahoman sample pre- and post-intervention, we rerun the analysis with the added restriction that an Oklahoman firm must appear at least once pre- and post-intervention. This analysis (untabulated) yields qualitatively similar results.

2.2.5 Results for Right-to-Work In Oklahoma

Given the above discussion, in equation (1) the coefficient δ_1 is interpreted as the impact of the Right-to-Work law on leverage. According to the bargaining device hypothesis (Hypothesis 1a), we expect to find that firms in Oklahoma decrease their leverage (i.e., negative δ_1) following the intervention. Conversely, for the crowding-out hypothesis (Hypothesis 2a), we expect to find that firms in Oklahoma increase their leverage (i.e., positive δ_1) following the intervention.

[Place Table 2 about here]

Table 2 presents the outcome of the test just described. The coefficient of interest in all regressions is the "Right-to-Work (Oklahoma) $\times$ Post" coefficient. Consistent with the dominance of the crowding out effect (i.e., H2a), Panel A of Table 2 indicates that the average firm in Oklahoma meaningfully *increased* leverage following an unambiguous *decrease* in the bargaining power of labor. Column 1 of Table 2 presents the estimation of equation (1) using our full sample of firm-years as described in Section 2.2.4. To control for the potential impact of the Dot-com recession, Column 2 presents the results when we restrict our sample to include only those firm-years for which the primary industry group was not listed as computer programming, data processing, and other computer related services (i.e., 3-digit SIC 737). Additionally, and to ensure that our results are not being driven by local conditions, Column 3 presents the results when our sample of non-internet firm-years is further restricted to Oklahoma and its' neighboring states (Arkansas, Colorado, Kansas, Missouri, New Mexico, and Texas). Finally, Column 4 provides results when we include state-level union membership as an additional control variable to the Column 3 subsample. State-level union membership is defined as the proportion of workers in a state who are union members (non-agricultural) and is provided by Hirsch and Macpherson (2003). We include this variable in our specification because it is the most reliable determinant of states' right-to-work status (Moore, 1998).

Thus, our "strictest" estimate of the effect of RTW on leverage is provided in Column 4 of Panel A/Table 2 and indicates that the average firm in Oklahoma increased financial leverage by 210 basis points. Specifically, this estimate is not confounded by: time-invariant firm-specific characteristics (firm fixed effects), industry-level shocks (industry-by-year fixed effects), changes in time-varying determinants of capital structure (control variables), local shocks (neighboring states only), by firms affected by the Dot-com bubble (non-internet firms only) or by changes in state-level union membership.

To get a clearer picture of the timing of the effects, we execute a dynamic analysis in which we estimate the year-by-year difference in the difference between financial leverage in Oklahoma and its' neighboring states relative to the first year in our window (1998). The regression specification is given in equation (2) and includes the same fixed effects and control variables as the equation (1) regression:

$$Flev_{i,t} = \alpha_0 + \sum_{m=-3}^{+2} \beta_{t-m} Year_{t-m} + \sum_{m=-3}^{+2} \delta_{t-m} RTW \times Year_{t-m} + \beta_k X_{k,i,t-1} + \gamma_{j,t} + \eta_i + \epsilon_{i,t} \quad (2)$$

where i, j, and t index firms, industries, and years; Flev represents a measure of financial leverage; Year is a series of year dummies taking a value of one for its' respective year from 1999 to 2004, else 0. RTW is an indicator variable coded 1 for firms incorporated in Oklahoma, and 0 otherwise. The coefficient on each interaction variable $RTW \times Year_{t-m}$ represents the difference between the average financial leverage of firms in Oklahoma and firms in its' neighboring states in a given year relative to the base case (1998). For example, $RTW \times Year_{1999}$ is the difference-in-differences (the difference of Oklahoma in 1999 and neighboring states in 1999 *relative* to the difference of Oklahoma in 1998 and neighboring states in 1998) in financial leverage.

Given our Panel A Table 2 findings, we expect to find that the difference-in-differences relative to 1998 are statistically significant and positive for each year following the treatment in 2001. Failure to find such a pattern would suggest evidence of a spurious analysis. The results are presented in Panel B of Table 2. As expected, the estimated coefficients on "Right-to-Work (Oklahoma) $\times Year_{2001\ to\ 2004}$" are statistically significant, positive and economically important in all years post-treatment. For example, in terms of the effect magnitude, average incremental leverage is 450 basis points higher than base in 2004. Further, there is no evidence of a "pre-treatment" effect as indicated by the insignificant coefficients on "Right-to-Work (Oklahoma) $\times Year_{2000}$" and "Right-to-Work (Oklahoma) $\times Year_{1999}$".

Finally, we also expect to find that the results are much stronger in labor/union-intensive industries. To test this, we re-estimate our primary DID model separately for those firms in labor-intensive industries versus less labor-intensive industries. Specifically, each year we classify an industry (2-digit SIC) as labor intensive if this industry is ranked above the median based on the ratio of the number of employees to total assets (employee ratio). Similarly, we also follow Matsa (2010) and examine historically union-intensive industries. The economic intuition here is that industries with a history of unionism are likely to have a stronger ex-ante threat of unionization. Thus, Right-to-Work laws should affect firms in these industries more than others. To this end, we classify an industry as union-intensive if it has at least 25% of its' workforce covered by collective bargaining agreements in 1983 (Matsa, 2010).[8]

The results of these tests are presented in Panel C of Table 2. Specifically, Columns 1 and 2 presents the results when we restrict our sample to include only labor-intensive industries and union-intensive industries, respectively. As expected, the estimated coefficient on the "Right-to-Work (Oklahoma) × Post" interaction term is positive and significant. For comparison, Columns 3 and 4 present the results when we restrict to the less labor-intensive and less union-intensive counterpart subsamples. As predicted for these cases, the estimated coefficients on these interaction terms are both statistically insignificant. We further perform a triple difference analysis with the interaction term of "RTW × Post × LaborIntensiveIndustryDummy" ("RTW × Post × UnionIntensiveIndustryDummy") to formally test whether the crowding-out effect coefficients are statistically different in labor vs. less-labor intensive industries, and unionized vs. less-unionized industries, respectively. Our triple-difference analysis results in Columns (5) and (6) confirm that the crowding-out effect is statistically more pronounced in both labor-intensive and union-intensive industries, respectively.

2.2.6 Abadie-Imbens Matching Estimator

To further probe the robustness of the crowding out effect for the average firm, we also use the matching estimator developed by Abadie and Imbens (2002). As with other commonly applied matching methods, this technique estimates the average treatment effect by comparing the treatment group to a set of counterfactuals. Here, the counterfactuals are chosen from the universe of possible controls by minimizing the Mahalanobis distance between user-specified vectors of covariates (nearest neighbor). Finally, because matching on multiple covariates can induce bias, a further adjustment is made to minimize any remaining bias (Abadie and Imbens,

[8]Union-intensive industries include the following 2-digit SIC codes: 10, 12, 14, 15, 16, 17, 20, 21, 23, 26, 29, 30, 31, 32, 33, 34, 36, 37, 40, 41, 42, 44, 45, 48, 49, 54, 78, and 82.

2002).[9]

Similar to our DID analysis, we match firms on size, tangibility, market-to-book ratio, ROA, and Zprob. We also restrict our universe of potential controls to include only Oklahoma's neighboring states. To increase the ex-ante power of the test, we make the same headquarters corrections described in Section 2.2. The outcome variable is the difference between the pre- and-post-three-year average of financial debt to book capital. We use heteroskedastic-consistent standard errors for inference.

The outcome of the Abadie-Imbens test is presented in Panel A of Table 3. Similar to the difference-in-differences method, this technique produces statistically significant results (at the 10% level) that support the existence of a negative relation.

[Place Table 3 about here]

Again, we also check for pre-existing trends by executing a placebo test in the style of Almeida, Campello, Laranjeira, and Weisbenner (2012). For this test, all aforementioned design aspects remain intact with one critical exception: the "treatment" period is arbitrarily reassigned to a pre-treatment year. We do this for each of the three years prior to the treatment (i.e., the year 2000, 1999, and 1998). A significant result for any of these placebo tests could be viewed as evidence of a pre-existing trend, or placebo effect (Almeida, Campello, Laranjeira, and Weisbenner, 2012). The results of this test are presented in Panel B of Table 3. Based on conventional levels of significance, the null hypothesis cannot be rejected, suggesting that there is no evidence of any pre-existing trends.[10]

2.2.7 Discussion of the Case of Oklahoma

Despite our best efforts to ensure the validity of our results, if there are any other contemporaneous events that impact capital structure decisions we might still be erroneously attributing the effects of these unobserved changes to the RTW legislation. Possible confounding events

[9] Due to a small sample, we do not rely on exact industry matching. This ensures that our results will not be driven by "distant" matches with respect to important covariates (e.g., comparing small firms to big firms in the same industry). Having said that, in a separate analysis of firms in union-intensive industries (with sector matching) we find qualitatively similar results. Details of this analysis are available upon request.

[10] To augment the matching estimator results presented in this sub-section, we also obtain estimates using the Synthetic Control Method (Abadie, Diamond, and Hainmueller, 2010, 2015). The details of these analyses are provided in the Internet Appendix (see Table IA.1 and Figure IA.1). The Synthetic Control Method compares the average change in financial leverage for firms headquartered in Oklahoma versus a 'Synthetic' Oklahoma. This alternative estimator obtains qualitatively similar results to our analysis here.

include state-level changes to the corporate tax rate and governance requirements. However, it is not apparent that any such changes to Oklahoma's tax code occur during the relevant window—the closest we can find is a change to Oklahoma's corporate tax rate which occurred 11 years prior to the treatment in 1990 (Heider and Ljungqvist, 2015). Further, we are unaware of any state-level shocks to governance requirements.

2.3 Right-to-Work in Indiana

Heretofore, we have only presented experimental evidence on a single case, Oklahoma. This is because it is the only "clean" RTW experiment that has occurred since 1976. For example, while RTW also became effective in Idaho in 1987, this is contaminated by a coinciding corporate income tax increase. Similarly, RTW legislation was amended in Texas in 1993, however, in 1992 the Franchise Tax levied on corporations in Texas was rewritten to include a 4.5% tax on "earned surplus" which includes corporate profits plus compensation paid to officers and directors.[11]

Most recently, RTW legislation was also adopted in Indiana and Michigan in 2012. In the case of Michigan, this event is confounded by the replacement of the Michigan Business Tax with the Corporate Income Tax.[12] Such a regime switch may have led to uncertain tax positions during transition. In Indiana however, changes to corporate taxation involved the commencement of staggered *reductions* in the corporate income tax rate. Because leverage does *not* respond to tax cuts (Heider and Ljungqvist, 2015) and the changes are unambiguous, the case of Indiana represents an additional opportunity to falsify our findings.[13]

The setup here mirrors our analysis of the Oklahoma case. The final sample includes all active and inactive U.S. firm-years from 2009 to 2014 with non-missing data and for which the primary sector is non-financial (SIC: 6000–6999) or public utilities (SIC: 4900–4999). We also exclude Michigan from the analysis due to its' adoption of RTW in 2013 and corporate tax rate changes in 2012.[14] Headquarter locations are corrected following the same procedure

[11] We are also concerned that the amendment in Texas was merely symbolic. Texas initially adopted RTW in 1947.

[12] The Michigan Business Tax included a 4.95% income tax as well as a 0.8% receipts tax. In 2012, this tax was effectively repealed and was replaced by a 6% corporate income tax.

[13] Notably, if Indiana firms do respond to tax cuts by reducing their leverage as prescribed by the trade-off theory, then it should work against us finding support for the crowding-out effect.

[14] The inclusion of Michigan in our sample does not affect our findings.

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as before.[15] This sample includes 14,830 firm-years (with 181 firm-years in Indiana across 44 firms). Our specification is given by equation (1) and is repeated below for convenience:

$$Flev_{i,t} = \alpha_0 + \beta_1 RTW + \delta_0 POST + \delta_1 RTW \times POST + \beta_k X_{k,i,t-1} + \gamma_{j,t} + \eta_i + \epsilon_{i,t} \qquad (1)$$

where i, j, and t index firms, industries, and years; $Flev_{i,t}$ represents a measure of financial leverage (the ratio of financial debt to book assets); RTW is an indicator variable coded 1 for firms incorporated in Indiana, and 0 otherwise; POST is an indicator variable coded 1 for observations after 2012, being the year of the legislative change and 0 otherwise. The parameter $X_{k,i,t-1}$ is the same vector of time-variant financial controls used in our Oklahoma test. The parameter $\gamma_{j,t}$ represents industry-by-year fixed effects for industry j at time t. The parameter η_i represents firm fixed effects. Following Matsa (2010), Heider and Ljungqvist (2015), standard errors are clustered by state, allowing for unspecified within-state correlation and are heteroskedastic-consistent.

The results from these tests are presented in Table 4. Because we only have two years of post-intervention data, and because Right-to-Work laws were initially found to be unconstitutional in late 2013, we provide two alternative estimation windows: 2009 to 2013 (one year post-RTW in Indiana) in Panel A; and 2009 to 2014 (two years post) in Panel B. Because of the successful legal challenge just mentioned as well as the fact that RTW was not upheld by the Supreme Court until November 2014, we expect to find stronger results using the shorter estimation window (Panel A).

[Place Table 4 about here]

Based on the results reported in Table 4, we are unable to falsify our previous finding that firms in RTW-affected states, increase their leverage following legal intervention. Results in Panel A indicate that firms in Indiana increased book leverage in response to the passage of Right-to-Work. Consistent with expectations, our findings are much stronger in the shorter-period. In fact, results for the longer estimation window are statistically insignificant (Panel B). This is plausibly explained by the aforementioned challenge to the legality of RTW in Indiana.[16]

[15] We correct 3.01% of headquarter locations in the 2009 to 2014 sample.

[16] In addition to RTW legislation, we also search for changes to Work Stoppage Provisions in Unemploy-

3 Panel Regression Evidence

The second step of our identification strategy involves the exploitation of a novel treatment variable designed to capture within-firm evolutions in union power. This is critically important because Right-to-Work laws (and labor laws in general) might not be passed in a *random* portfolio of laws. For instance, the passage of RTW could be accompanied with other generally pro-business legislation or may signal an ex-ante increased probability of observing such legislation in the near future. Thus, any examination of labor laws in isolation runs the risk of faulty inference as a result of unobserved, correlated legislation.[17] To combat this concern, as a supplementary analysis, we exploit a panel regression model with high-dimensional fixed effects which is not vulnerable to these types of threats.

3.1 Data Sources and Sample Selection

We use two databases to construct our regression sample. We obtain financial and accounting information from the merged CRSP-Compustat database and data on firm-level collective bargaining coverage from the Department of Labor's (DOL) Open Government Dataset, EBSA (Employee Benefits Security Administration) Form 5500 series. These collective bargaining data span the period 1999 to 2013.[18]

ment Insurance Legislation (WSP) using Bureau of Labor Statistics reports on changes in state and federal unemployment insurance laws. These searches reveal one significant change to WSPs in Michigan (2002). This change provides that workers who become unemployed as a result of a strike that is not allowed under the bargaining contract or is a wildcat strike, have their ineligibility period for monetary unemployment benefits extended from 6 to 13 weeks. This would arguably reduce the bargaining power of labor in Michigan since it increases the personal risk of striking, which is a powerful bargaining device for labor. In unreported analysis, we redo our difference-in-differences financial leverage regression by replacing "RTW × Post" with "WSP × Post", we document statistically significant and positive coefficient for "WSP × Post" which is consistent with a negative relation between the bargaining power of labor and financial leverage. However, this is not a fully reliable falsification test because the change in WSP in Michigan might be confounded by a contemporaneous increase in the level of general unemployment benefits by approximately 21%, which is also hypothesized to induce a positive leverage response (Agrawal and Matsa, 2013).

[17]This problem is still present in large samples of events.

[18]In addition to the modern dataset, raw data for 1991 to 1998 are available from the Department of Labor through a Freedom of Information request and a small processing fee. These data are delivered as simple text files with a codebook describing the position of each item. No additional instructions are provided. We do not rely on these data for three main reasons. First, unlike the more recent dataset, they do not take advantage modern, electronic filing technologies, dramatically increasing the number of expected errors. Second, the 1999 reporting reforms combined several 5500 forms into a single, streamlined submission, more than tripling the number of observations. Third, in the instructions to filers, the description of active participants changed in 1999. After 1998, the description of active participants no longer explicitly mentions that participants include those employees who are below the permitted disparity level on plans that are integrated with social security. This may have led to a revision in participant counts by some filers.

The development of our full sample proceeds as follows. We start with all active and inactive firms covered by the merged CRSP-Compustat database. This file contains 178,313 firm-year observations between 1999 and 2013. Following conventional practice, we use the same basic filtering process as described in Section 2.2.1 by excluding those firm-years for which the primary industry was either financial (SIC: 6000–6999) or public utilities (SIC: 4900–4999). We do this because these firms are heavily influenced by regulation. This leaves us with 118,536 firm-years. We then drop all non-U.S. firms and firm-years with negative values for assets, stockholder's equity, or minority interests (balance sheet). This leaves 86,840 firm-years.

Next, we merge this sample with the EBSA Form 5500 datasets by the sponsor's Employer Identification Number ("EIN") and year and obtain matches for 44,338 firm-years. Finally, we exclude firm-year observations with missing data for dependent, treatment, and control variables (Matsa, 2010). This yields an unbalanced panel of 28,574 firm-year observations encompassing a cross-sectional dimension including 4,596 firms operating across 60 different sectors (2-digit SIC). Continuous variables are then winsorized at the 1st and 99th percentiles to eliminate the effect of outliers (Matsa, 2010).

3.2 Dependent Variable: Financial Leverage

Mirroring the quasi-experimental design, our primary dependent variable is the ratio of financial debt to book assets. However, we also provide estimates for eight alternative and meaningful measures of leverage. Our findings hold regardless of how we define leverage.[19]

3.3 Treatment Variable: Largest Pension Bargaining Unit

Similar to Becker and Olson (1992), Ramírez Verdugo (2006), our treatment variable is constructed using collective bargaining information from IRS Form 5500 submissions. Employers who have established a pension- or welfare-benefit plan covered by the Employee Retirement Income Security Act (1974) are required to file a Form 5500 with the Department of Labor every year. These annual filings, which are open for public inspection, are required for each plan offered by the employer and include two pages of general plan information as well as 13 accompanying schedules.[20] In order to improve compliance as well as the fidelity of the digi-

[19] All of these measures, including our control variables are defined in Appendix B.

[20] An example Form 5500 can be downloaded from http://www.dol.gov/ebsa/pdf/2014-5500.pdf.

tized records, the Department of Labor significantly revised the Form in 1999 to make it more consistent with a corporate tax return and launched EFAST (Electronic Filing Acceptance System). The EFAST system enables filers to submit machine-print and machine-readable forms and schedules that can be recorded much more reliably.[21]

The information we extract from each plan-level observation includes: (1) the sponsor's employer identification number (EIN); (2) the declaration of collective bargaining; (3) the total number of *active* employees covered; and (4) benefit codes identifying key plan features. More specifically, we interpret the declaration that the plan was collectively-bargained as evidence that the submission relates to a union plan rather than a non-union plan. Next, we identify filings related to retirement pensions using codes that identify the plan as either a defined contribution plan or a defined benefit plan.[22]

For our primary analyses, we proxy for the bargaining power of labor using the ratio of the *largest* pension bargaining unit (LPBU) to total employees:

$$LPBU_{i,t} = \frac{Active\ Employees\ on\ Largest\ Collectively\ Bargained\ Pension\ Plan_{i,t}}{Total\ Employees_{i,t}} \tag{3}$$

where subscripts i and t index firm and year, respectively. Our estimate for the total number of employees comes from Compustat. Plan participants are classified as 'active employees' if they are currently in employment covered by the plan and are earning or retaining credited service under the plan. This includes non-vested (e.g., recently hired) employees as well as eligible non-participants. We interpret LPBU as the relative size of the largest potential collective bargaining unit at the firm-level.[23]

[21]No errors in electronic filings were found in the 2005 audit of EFAST recordings by the Office of the Inspector General. In 2009, this system was further improved with the launch of EFAST2. The two most significant improvements in this second iteration of EFAST included the complete transition to accurate, paperless filing as well as the introduction of monthly updates to datasets from 2009. These monthly updates automatically remove erroneous, obsolete filings that would otherwise remain in the dataset. Full records for the modern dataset (1999 to 2013) are available from the Department of Labor's website in comma separated text files.

[22]Results from our panel regressions remain qualitatively similar when we only consider defined benefit plans.

[23]In unreported analyses we also estimate a triple difference-in-differences specification using LPBU. Although we document evidence consistent with our primary findings in Section 2, multicollinearity renders such an analysis as less than reliable.

3.3.1 LPBU versus Extant Measures

We argue that our measure (equation (3)) better reflects variation in the bargaining power of labor vis-à-vis the firm relative to alternative proxies that rely on *total unionization* for two main reasons. First, is the emergence of fragmented collective bargaining (e.g., plant-level bargaining) where the firm separately negotiates with its' bargaining units (Eschuk, 2002, Katz, 1993). More specifically, because estimates of total unionization either implicitly or explicitly sum across all unions in the numerator, they do not consider this new reality. Second and relatedly, there is convincing evidence that fledgling collective bargaining units do not induce a leverage response in U.S. firms in at least the first three years (Lee and Mas, 2012). This unresponsiveness is likely because collective bargaining units require time to develop and establish themselves within a firm or plant. In fact, in some cases it might even take up to a decade before a collective bargaining unit has any effective bargaining power (Lee and Mas, 2012). Since we are interested in proxying for bargaining power, by tracking changes in only the *largest* collective bargaining unit, we find appealing a measure like this since it reduces the amount of variation caused by changes in less powerful and fledgling collective bargaining units.[24]

In addition to these conceptual improvements, we argue that our measure also offers four practical advantages over estimates of total unionization. First, LPBU does not rely upon voluntary disclosures of collective bargaining (see, e.g., Eschuk (2002), Matsa (2010), Cheng (2011)). This avoids the problems associated with potential biases caused by firms disclosure incentives. For example, because news of increased union activity is associated with declines in value (Lee and Mas, 2012), we expect that estimates of total unionization derived from voluntary disclosures will be downwardly biased. For example, if a firm experiences an increase in union activity, managerial incentives could plausibly lead to either a misreporting or even the complete cessation of disclosure. Thus, by relying upon disclosures that are mandated by ERISA and the Internal Revenue Code, we avoid these types of biases.

Second, our construct does not require extensive hand collection, allowing for easy replication and adoption. Third, our measure allows for the systematic identification of an evidence-based control group, avoiding the need to include random firms and assume that they are

[24] Results remain very similar when we sum across all union retirement plans at the firm level in the numerator of equation (3).

non-unionized (see, e.g., Matsa (2010)). Fourth, our measure generates a richer panel in the sense that it produces viable estimates across a larger, more diverse set of firms—well beyond a relatively narrow subset of manufacturing firms (approximately 1,140 firm-years in total across 1977 and 1987), as is commonly constrained by the surveys of Hirsch (1991).

Finally, we acknowledge that our matching algorithm will not consider collectively bargained pension plans sponsored by the firm's wholly owned subsidiaries reporting under different EINs.[25] In order to fully correct for this, we would need the EINs of every wholly-owned subsidiary of every firm for every year in our sample. Such a research design choice is prohibitive because these data are not readily available, especially over sufficiently large and representative samples. A random check of our algorithm suggests that the potentially distorting effect of wholly-owned subsidiaries is negligible in our sample.[26]

3.4 Summary Statistics

3.4.1 Largest Pension Bargaining Unit

Table 5 presents summary statistics for our treatment variable, LPBU. Panel A indicates that across our full sample, the conditional density of the largest pension bargaining unit is 34.49% with a standard deviation of 31.42%. Panel B provides unconditional means and standard deviations for each of eight industry divisions defined by the Department of Labor as well as for all firms. We find evidence of union pension bargaining in all divisions, however, this activity is most intense in the manufacturing, transportation and communications, wholesale trade, and mining divisions. Across the full sample, the unconditional density of the largest pension bargaining unit is 4.10% with a standard deviation of 15.55%.

[Place Table 5 about here]

Figure 1 depicts the trend in the relative size of the largest pension bargaining unit over time for each of eight industry divisions in our full sample with more than 1,000 firm-year observations. In general, LPBU has increased over time in our sample.

[25] We find several instances in which subsidiaries report using their parent firm's EIN.

[26] Specifically, we investigate the potential significance of this issue by randomly selecting 60 firm-years from 2005 (30 with union pensions and 30 without union pensions) and check the accuracy of our algorithm. Although we find a large number of plans being reported by subsidiaries with different EINs, they tend to be significantly smaller than plans reported by the parent firm. In fact, we find that our algorithm correctly identifies the numerator of our measure in 59 of 60 observations. In practical terms, this suggests that our matching algorithm is unaffected by wholly owned subsidiaries reporting with different EINs.

[Place Figure 1 about here]

3.4.2 General Firm Characteristics

Panel A of Table 6 contains basic summary statistics for all variables used in our primary panel analyses. The average firm in the sample is quite large, with annual sales of $1,784 million (median of $229 million), with a return on assets of 5%, and an earnings volatility of approximately 8% (three year standard deviation of the change in EBITDA scaled by three year mean assets). On average, tangible assets and capital expenditures make up 23% and 5% of total assets, respectively. As expected, research and development intensity exhibits high positive skewness with a mean of 37% contrasting a median of just 1%. Similarly, Tobin's Q also exhibits positive skewness with a mean value of 1.50 versus a median value of 0.96. Finally, mean book leverage (financial debt to book assets) is 0.17 and mean long-term book leverage (long-term financial debt to book assets) is 0.15.

Panel B of Table 6 presents univariate tests of equivalence between sampled unionized and non-unionized firms. These tests confirm that unionized firms are significantly different to non-unionized firms with respect to all variables at the 1% level. Moreover, at the mean, economically significant differences exist for size, Tobin's Q (and market-to-book), tangibility, ROA (return on assets), earnings volatility, bankruptcy risk (Zprob), research and development intensity, and four alternative measures of leverage.

In our sample, unions tend to operate in much larger, more stable, more profitable firms with lower value, proxied by Tobin's Q, and in firms that maintain a higher proportion of fixed assets. Finally, unionized firms exhibit considerably higher financial debt ratios than their non-unionized counterparts but are significantly further from bankruptcy.

Crucially, from a research design perspective, the pervasive finding of non-equivalence between the two groups motivates the use of a firm fixed effects approach, since such non-equivalence very likely extends to include unobserved, time-invariant, firm-specific variables.[27]

[Place Table 6 about here]

[27] Ignoring this issue would render our analysis unreliable due to non-trivial endogeneity threats (Parsons and Titman, 2009).

3.5 Panel Regression Specification

To test our hypothesis, we follow the advice of Gormley and Matsa (2014) and estimate a panel regression with high-dimensional fixed effects. Heteroskedastic-consistent standard errors, clustered at the firm-level are used for inference (Matsa, 2010). Specifically, the regression specification is:

$$Flev_{i,t} = \alpha_0 + \beta_1 LPBU_{i,t-1} + \beta_k X_{k,i,t-1} + \gamma_{j,t} + \eta_i + \epsilon_{i,t} \tag{4}$$

where i, j, and t index firms, industries, and years; the parameter $Flev_{i,t}$ represents a given measure of financial leverage and our main variable of interest (LPBU) is measured as the ratio of the number of active staff covered by the largest collectively bargained retirement pension to the total number of employees (i.e., equation (3)). The vector, $X_{k,i,t-1}$ contains the same control variables used in our quasi-experiment. The measurement, predicted signs, and a brief motivation of all of these variables is summarized in Panel B2 of Appendix B. The parameter $\gamma_{j,t}$ represents industry-by-year fixed effects, while η_i represents firm fixed effects.[28] Since increases in LPBU indicate increases in union power at the firm-level, our hypotheses are as follows:

H1b (Bargaining Hypothesis): there is a positive relation between union power and financial leverage (i.e., $\beta_1 > 0$*)*

H2b (Crowding-out Hypothesis): there is a negative relation between union power and financial leverage (i.e., $\beta_1 < 0$*)*

3.6 Baseline Analysis

Table 7 presents the estimation of equation (4). Consistent with our quasi-experiment, we find evidence of a negative relation between union power and financial leverage for the average firm in our sample. We gauge economic significance by comparing the predicted effect of an increase in LPBU from 0.00% to 34.41% (conditional mean) to the non-unionized sample's mean leverage ratio.

[28]To discriminate between a fixed effects model and a random effects model, we apply the Hausman Specification Test. These results indicate that the fixed effects estimator is preferred to the random effects estimator at the 1% significance level. These results are not reported for brevity. Further, we do not use limited dependent variable models such as the Tobit model because the fixed effects estimator is biased in nonlinear models (Greene, 2002).

[Place Table 7 about here]

To this end, based on the results presented in Column 1 of Table 7, an average firm with 34.45% of its' workforce in a single bargaining unit is associated with a leverage ratio (FDAT) that is 62 basis points lower than an average non-unionized firm. This represents a reduction in financial leverage of 3.9% relative to the mean leverage ratio of non-unionized firms. In the case of long-term financial debt to book assets as an alternative proxy for the dependent variable, this same change is associated with a 4.7% reduction.

These findings hold regardless of the measure of leverage used. Panel B reports the results of identical regressions on the ratio of financial debt to market assets, financial debt to market capital, total liabilities to market assets, and total liabilities to book assets (defined in Appendix B). As an additional robustness check (reported in Column 5 of Panel B), we re-estimate our model using the subset of firm-years with non-zero financial debt, non-zero LPBU, and a Cook's Distance of less than 4 divided by the number of observations. The purpose of this test is to ensure that our results are not being driven by zero-leverage firms (see, e.g., Strebulaev and Yang (2013)), zero-LPBU firms, or by influential outliers (Cook's Distance). Our results remain robust.[29, 30] Overall, our baseline results strongly support the view that higher labor bargaining power crowds out financial leverage for the average firm in our sample.

3.7 Exploring the Underlying Mechanism

One possible, alternative explanation for the negative coefficient on LPBU is that it reflects the strategic reduction of corporate cash holdings (Klasa, Maxwell, and Ortiz-Molina, 2009). In other words, are these firms actually deliberately responding to an increase in the bargaining power of labor by reducing their cash holdings by retiring debt? Following this argument, such a reduction in cash shores-up the firm's bargaining position by (allegedly) credibly signaling liquidity constraints. Thus, should we observe our sample firms systematically opting to retire debt to reduce cash holdings, an alternative mechanism is likely at play.

[29]Because our algorithm identifies pension plans using codes submitted by the direct filing entity, there remains a small chance that our results are being driven by filer error. To control for any potential miscoding, we take advantage of the declaration of the attachment of retirement schedules as an alternative method of identifying retirement plans. The Internet Appendix documents a robust negative relation using this alternative analysis (refer to Table IA.2).

[30]Table IA.3 in our Internet Appendix also shows that our panel regression results are robust to the inclusion of additional control variables.

To rule out this alternative scenario, we run a cash holdings regression on LPBU with firm and industry-by-year fixed effects.[31] If this explanation has merit in our sample, we would expect to find that the LPBU coefficient is negative and statistically significant. However, in results reported in the Internet Appendix (see Table IA.4), each and every cash holdings regression fails to find any evidence of a relation between LPBU and corporate cash holdings, suggesting that our main results are not driven by the strategic reduction in cash holdings.

However, ruling out this alternative explanation does not guarantee that the negative relation in our sample is, indeed, being driven by excess costs associated with labor unions (e.g., wage markups). To directly test the crowding-out effect, we ideally need to observe the wage-effects of labor unions at the firm-level. Unfortunately, comprehensive 'micro' wages data needed for such an analysis are not readily available. An obvious alternative strategy would be to test for a positive relation between the relative size of the largest pension bargaining unit (LPBU) and the firm's reported wage bill. Again however, this strategy is met with significant data issues. For example, Cronqvist, Heyman, Nilsson, Svaleryd, and Vlachos (2009) find that Compustat only reports "Labor and Related Expenses" for 18% of firm-years between 1995 and 2005. Further, Cronqvist, Heyman, Nilsson, Svaleryd, and Vlachos (2009) additionally find that firms non-randomly choose to report labor expenses as a separate line item.[32] This acute self-selection bias could easily lead to spurious inferences regarding the relation between LPBU and wages.

More pragmatically, in light of the above issues, we conduct an informal investigation of the relation between LPBU and reported "Pension and Retirement Expenses" from Compustat. If increases in LPBU are associated with excess remuneration in the form of more valuable pensions, we would expect to observe a positive relation between LPBU and pension and retirement expenses reported on the income statement.

For defined contribution plans, employer-contributions are directly expensed on the income statement. For defined benefit plans, expenses reflect changes in the pension's liability relative to the pension's return. Thus, increases in the defined benefit promised to employees results in

[31] The dependent variable is defined as the natural log of the ratio of cash to net assets. We also follow Opler, Pinkowitz, Stulz, and Williamson (1999) to include the following control variables: market-to-book ratio, log of deflated net assets, cash flow/net assets, net working capital capital/net assets, industry sigma, R&D/sales, leverage, and a dividend dummy. For more details on these control variable definitions, refer to Table IV of Opler, Pinkowitz, Stulz, and Williamson (1999).

[32] The sample of firms who report "Labor and Related Expenses" are also dominated by financial firms, which are restricted from our analysis.

an increased expense, ceteris paribus. To this end, our chosen specification is:

$$PRE_{it} = \alpha_0 + \beta_1 LPBU_{i,t-1} + \eta_i + \epsilon_{i,t} \tag{5}$$

where i and t index firms and years. The dependent variable PRE_{it} is reported pension and retirement expenses (\$) scaled by the number of employees. The variable LPBU is measured as the ratio of the number of active staff covered by the largest collectively bargained retirement pension to the total number of employees (i.e., equation (3)). Because the dependent variable is highly sensitive to the unique characteristics of the firm's pension offerings, we include firm-fixed effects, denote by η to sweep out any unobserved, time-invariant, firm-level heterogeneity. Finally, standard errors are corrected for heteroskedasticity and are clustered by firm.

The results of this informal analysis are presented in Equation (6). Standard errors are reported in parentheses.

$$PRE_{it} = \alpha_0 + \underset{(186.64)}{511.46} LPBU_{i,t-1} + \eta_i + \epsilon_{i,t} \tag{6}$$

Observations = 24,992; Adj R^2=75.1%; R^2 Within=1.4%

Consistent with the crowding-out mechanism, we find evidence of a strong positive association between LPBU and reported pension and retirement expenses at the 1% level of significance. Indeed, our results suggest that an average unionized firm (34.45% of employees in a single bargaining unit) is associated with an incremental increase in retirement and pension expenses of approximately \$176 *per employee.* For the average unionized firm in our sample, this represents an economically significant total expense of around \$3.58 million per year.[33] Accordingly, we interpret this as *suggestive* evidence consistent with the view that increased labor costs is at least one of the mechanisms driving the crowding-out effect.

This is in contrast to a stricter interpretation of the crowding-out effect which suggests that the sole mechanism is operating leverage caused by increased hiring and firing costs (see, e.g., Kuzmina (2013)). On an intuitive level, this would appear to present an immediate threat to our

[33]These results are not confounded by the introduction of SFAS 87, *Employers Accounting for Pensions* since it occurred 14 years prior to the beginning of our sample. Additionally, our findings are unlikely to be confounded by the introduction of SFAS 158, *Employers Accounting for Defined Benefit Pensions and Other Postretirement Plans* in 2006 as well as the amendment of SFAS 132 in 2003 because they did not change existing rules regarding measurement or recognition.

crowding-out interpretation—don't unions simultaneously increase job security by increasing the cost of firing unionized workers?[34] If this is the case, then we need to offer some assurance that our findings do not solely reflect labor-rigidity. We note two objections here. First, the link between RTW's affect on *all* firms and the cost of firing appears tenuous. In fact, ex-ante, we are unable to come up with a compelling argument that our design can be considered a test of operating flexibility. This natural insulation supports the relevance of excess labor costs.

Second, and rather curiously, it is also worth noting that in spite of the intuition linking unionization to increased firing costs, prior evidence suggests that unionized manufacturing firms experience *more* layoffs (particularly temporary) than their non-unionized counterparts during business cycle fluctuations (Freeman and Medoff, 1984). Aidt and Tzannatos (2002) argue that this paradox might be explained by self-serving senior union officials who barter deals to layoff junior workers in return for the preservation of senior workers positions and wages. Thus, the misalignment of incentives between union stewards and junior members may distort the actual relation between unionization and operating flexibility. In sum, while we do not take issue with the literature regarding firing costs and leverage, we do argue that our tests are more revealing of the importance of excess labor costs.

4 Conclusion

Despite the decline in trade unionism in the United States, organized labor still has plausibly important impacts on financial policies. Our paper provides new evidence on the relation between financial leverage and union power as well as the *threat* of unionization. Using a robust research design involving both quasi-experimental and regression evidence, we document the existence of a negative relation between union power (and the threat of unionization) and financial leverage in the contemporary United States. Specifically, we find that firms in both Oklahoma (2001) and Indiana (2012), chosen because of their ideal settings, respond to an exogenous weakening in union power and the threat of unionization by *increasing* financial leverage. We also document heterogeneous treatment effects. Specifically, in our quasi-experiments, we find that the passage of RTW has a significantly stronger effect in firms who operate in labor-

[34]Although unions are interested in both wage gains and job security, private sector unions appear to be much more interested in the former (Aidt and Tzannatos, 2002).

intensive industries and industries which are traditionally union-intensive. In other words, the overall evidence supports a "crowding-out" effect.

Further, and to guard against the possibility that our findings are not being driven by unobserved, correlated legislative changes, we also exploit panel regression analysis. In these regressions, we develop a novel treatment variable derived from mandatory disclosures on union pension coverage over the period 1999 to 2013. Our regression results are consistent with the quasi-experimental design. Increases in the relative size of the largest union bargaining unit are associated with reductions in financial leverage. These results are strongly robust in favor of the "substitution" effect between labor power and financial leverage, captured by the crowding-out hypothesis.

5 Tables and Figures

Table 1
Summary Statistics—Quasi Experiment Analysis

Our sample consists of 22,519 firm-year observations for all non-financial and non-utility U.S. companies between 1998 and 2004. 1998 (2004) is the three years prior to (after) the adoption of the 2001 Right-to-Work law in Oklahoma. This table tabulates summary statistics for our dependent variables in Panel A and the controls in Panel B, respectively. All variables are defined in Appendix B.

Panel A: Dependent Variable Measures

Variable	Mean	StDev	25th	Median	75th	Min	Max
Financial Debt to Assets	0.20	0.19	0.01	0.15	0.33	0.00	0.72
Financial Debt to Book Capital	0.26	0.25	0.01	0.21	0.45	0.00	0.91
LT Financial Debt to Assets	0.15	0.18	0.00	0.07	0.26	0.00	0.67
LT Financial Debt to Book Capital	0.20	0.23	0.00	0.10	0.35	0.00	0.85

Panel B: Control Variable Measures

Variable	Mean	StDev	25th	Median	75th	Min	Max
Size (real $mil)	1,045	3,331	36	140	562	0.33	26,676
Size (natural log of real net sales)	4.94	2.10	3.59	4.94	6.33	-1.10	10.19
Tangibility	0.25	0.22	0.08	0.18	0.36	0.01	0.90
Market-to-Book	2.00	2.09	0.80	1.25	2.27	0.31	11.25
ROA	0.04	0.23	-0.01	0.10	0.16	-0.89	0.40
Default Spread (in %)	0.89	0.26	0.64	0.81	1.06	0.55	1.41
GSP Growth Rate (in %)	3.78	2.43	2.20	3.70	5.90	-4.20	12.70
State Unemployment Rate (in %)	4.87	1.13	4.12	4.90	5.65	2.30	8.42

Table 2
Oklahoma's Adoption of Right-to-Work Legislation
Difference-in-Differences Analysis

This table presents the results of a difference-in-differences regression with additional covariates using an unbalanced panel of firms. In all regressions, the dependent variable is the ratio of financial debt to book assets and the key variable of interest is the "Right-to-Work (Oklahoma) × Post" coefficient, i.e., δ_1. H1a (Bargaining Hypothesis) predicts a negative δ_1, while H2a (Crowding-out Hypothesis) predicts a positive δ_1. Oklahoma's neighboring states include Arkansas, Colorado, Kansas, Missouri, New Mexico, and Texas. Headquarters locations are verified as described in Section 2.2.4. Compustat variables are winsorized at the 1% and 99% tails. All regressions in Panel A and C include an intercept term, a post dummy, an RTW (Oklahoma) dummy as well as both industry-by-year and firm fixed effects. In addition, Columns (5) and (6) in Panel C include all remaining interaction terms required for a triple difference analysis. Firm-level financial controls include size, market-to-book ratio, ROA, tangibility, and modified Z-score. These variables are defined in Appendix B. The dynamic analysis regression in Panel B includes an intercept term as well as firm, industry-by-year, and year fixed effects. For ease of difference-in-differences interpretation, we drop RTW (Oklahoma) × Year=1998. Therefore, in the dynamic analysis in Panel B, each coefficient of RTW (Oklahoma) × Year (t=1999 to 2004) can be interpreted as the difference-in-differences for the respective year relative to 1998. Heteroskedastic-consistent standard errors, clustered at the state-level are provided in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively.

Panel A: Difference-in-Differences (Oklahoma)

Variable	Predicted Sign	(1)	(2)	(3)	(4)
		All states		Neighboring states	
		All firm-years	Non-internet firm-years	Non-internet firm-years	Non-internet firm-years
Right-to-Work (Oklahoma) × Post (δ_1)	H1a: $\delta_1 < 0$	0.015*	0.015*	0.024*	0.021*
	H2a: $\delta_1 > 0$	(0.01)	(0.01)	(0.01)	(0.01)
Size		0.017***	0.019***	0.016**	0.016**
		(0.00)	(0.00)	(0.01)	(0.01)
Market to Book Ratio		−0.004***	−0.005***	−0.006**	−0.006**
		(0.00)	(0.00)	(0.00)	(0.00)
Profitability		−0.029**	−0.033**	−0.089***	−0.088***
		(0.01)	(0.01)	(0.03)	(0.03)
Tangibility		0.111***	0.127***	0.120	0.120
		(0.02)	(0.02)	(0.08)	(0.08)
Zprob		−0.004***	−0.004***	−0.006	−0.006
		(0.00)	(0.00)	(0.00)	(0.00)
State Union Membership					−0.001
					(0.01)
Drop Internet Firms (3-digit SIC 737)		NO	YES	YES	YES
Neighboring States Only		NO	NO	YES	YES
Firm and Industry-by-year Fixed Effects		Yes	YES	YES	YES
Treated OK Firms (firm-year observations)		49	48	48	48
		(182)	(179)	(179)	(179)
Control Firms (firm-year observations)		5,361	4,536	620	620
		(22,337)	(19,313)	(2,583)	(2,583)
Total Firms (Observations)		5,410	4,584	668	668
		(22,519)	(19,492)	(2,762)	(2,762)
Adjusted R-squared		84%	84%	85%	85%

Panel B: Dynamic Analysis (Test for Pre-treatment Trends and Reversals)

		(1)
		Neighboring states (non-internet)
Variable	Predicted Sign	
Right-to-Work (Oklahoma) × 1999 (δ_{t-2})	H1a: $\delta_{t-2} = 0$	−0.020
	H2a: $\delta_{t-2} = 0$	(0.01)
Right-to-Work (Oklahoma) × 2000 (δ_{t-1})	H1a: $\delta_{t-2} = 0$	0.027
	H2a: $\delta_{t-2} = 0$	(0.02)
Right-to-Work (Oklahoma) × 2001 (δ_t)	H1a: $\delta_t < 0$	0.037**
	H2a: $\delta_t > 0$	(0.02)
Right-to-Work (Oklahoma) × 2002 (δ_{t+1})	H1a: $\delta_{t+1} < 0$	0.031**
	H2a: $\delta_{t+1} > 0$	(0.01)
Right-to-Work (Oklahoma) × 2003 (δ_{t+2})	H1a: $\delta_{t+2} < 0$	0.024**
	H2a: $\delta_{t+2} > 0$	(0.01)
Right-to-Work (Oklahoma) × 2004 (δ_{t+3})	H1a: $\delta_{t+3} < 0$	0.045***
	H2a: $\delta_{t+3} > 0$	(0.00)
Control Variables		YES
Year Dummies		YES
Drop Internet Firms (3-digit SIC 737)		YES
Neighboring States Only		YES
Firm and Industry-by-year Fixed Effects		YES
Treated OK Firms (firm-year observations)		48 (179)
Control Firms (firm-year observations)		620 (2,583)
Total Firms (Observations)		668 (2,762)
Adjusted R-squared		85%

Panel C: Difference-in-Differences (Industry Sub-samples)

	(1)	(2)	(3)	(4)	(5) = (1)vs.(3)	(6) = (2)vs.(4)
	Neighboring states (non-internet)					
Sample	Labor-intensive	Union-intensive	Less Labor-intensive	Less Union-intensive	Labor vs. Less Labor-Intensive	Union-vs. Less Union-intensive
Right-to-Work (Oklahoma) × Post (δ_1)	0.075*	0.104*	0.004	−0.001		
	(0.04)	(0.05)	(0.01)	(0.01)		
RTW × Post × LaborIntensiveIndustryDummy					0.050*	
					(0.03)	
RTW × Post × UnionIntensiveIndustryDummy						0.110**
						(0.05)
Control Variables	YES	YES	YES	YES	YES	YES
Drop Internet Firms (3-digit SIC 737)	YES	YES	YES	YES	YES	YES
Neighboring States Only	YES	YES	YES	YES	YES	YES
Firm and Industry-by-year Fixed Effects	YES	YES	YES	YES	YES	YES
Treated OK Firms	20	13	33	35	48	48
Treated OK (firm-year observation)	(75)	(48)	(104)	(131)	(179)	(179)
Control Firms	299	194	444	426	620	620
Control (firm-year observations)	(998)	(778)	(1,585)	(1,805)	(2,583)	(2,583)
Adjusted R-squared	90%	84%	85%	85%	85%	85%

Table 3

Abadie-Imbens Matching Estimator of the Average Treatment Effect for the Treated

This table presents the results of the Abadie-Imbens matching estimator. This technique estimates the average treatment effect, i.e., δ, by comparing the treatment group to a set of counterfactuals. Here, the counterfactuals are chosen from the universe of potential controls by minimizing the Mahalanobis distance between size, tangibility, market-to-book ratio, ROA, and ZProb. H1a (Bargaining Hypothesis) predicts a negative δ_1, while H2a (Crowding-out Hypothesis) predicts a positive δ_1. Column 1 presents the results of the main test using the adoption of the Right-to-work law in Oklahoma in 2001. Column 2 presents the results of the placebo test. For this placebo test, all design aspects remain identical with one exception: the "treatment" period is arbitrarily reassigned to 2000, 1999 and 1998, respectively. Standard errors are heteroskedastic-consistent and estimates are corrected for matching bias. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively. Robust standard errors are reported in parentheses.

Panel A: Main Matching Tests

		(1)
		Neighboring states (non-internet)
		Year 2001
Variable	Prediction	
Sample Average Treatment Effect for the Treated (δ_1)	H1a: $\delta_1 < 0$	0.088*
- Financial Debt to Book Assets (three-year mean)	H2a: $\delta_1 > 0$	(0.05)
Treated firms		13
Non-treated firms		193
Control firms		13

Panel B: Placebo Matching Tests

		(1)	(2)	(3)
		Neighboring states (non-internet)		
		Year 2000	Year 1999	Year 1998
Variable	Prediction			
Sample Average Treatment Effect for the Treated (δ_1)	Null	0.062	0.038	-0.029
- Financial Debt to Book Assets (three-year mean)		(0.07)	(0.06)	(0.05)
Treated firms		14	16	17
Non-treated firms		199	184	177
Control firms		14	16	17

Table 4
Indiana's Adoption of Right-to-Work Legislation
Falsification Experiment

This table presents the results of a difference-in-differences regression with additional covariates using an unbalanced panel of firms. In all regressions, the dependent variable is the ratio of financial debt to book assets and the key variable of interest is the "Right-to-Work (Indiana) × Post" coefficient, i.e., δ_1. H1a (Bargaining Hypothesis) predicts a negative δ_1, while H2a (Crowding-out Hypothesis) predicts a positive δ_1. Panel A presents the results from the sample period 2009 to 2013 (one year post) whilst Panel B includes 2009 to 2014 (two years post). If the main findings from our cross-sectional time-series regressions as well as our quasi-experiment in Oklahoma are not spurious, we expect to find evidence of a positive leverage response for treated firms in Indiana, particularly in the shorter estimation window. Headquarters locations are verified as described in Section 2.2.4. Compustat variables are winsorized at the 1% and 99% tails. All regressions include an intercept term, a post dummy, an RTW (indiana) dummy, as well as both industry-by-year and firm fixed effects. Firm-level financial controls include size, market-to-book ratio, ROA, tangibility, and modified Z-score. These variables are defined in Panel B of Appendix B. Heteroskedastic-consistent standard errors, clustered at the state-level are provided in parentheses. ***, ** and * denote statistical significance at the 1%, 5%, and 10% level respectively.

Panel A: Difference-in-Differences (Indiana)—2009 to 2013 (1 year post)

		(1)	(2)
		All states	Neighboring states
	Predicted Sign		
Right-to-Work (Indiana) × Post (δ_1)	H1a: $\delta_1 < 0$	0.009***	0.011*
	H2a: $\delta_1 > 0$	(0.00)	(0.01)
Control Variables		YES	YES
Neighboring States Only		NO	YES
Treated IN firms (firm-year observations)		23 (151)	23 (151)
Control Firms (firm-year observations)		3,417 (12,469)	230 (862)
Total Firms (Observations)		3,440 (12,620)	253 (1,013)
Adjusted R-squared		88%	92%

Panel B: Difference-in-Differences (Indiana)—2009 to 2014 (2 years post)

	(1)	(2)
	All states	Neighboring states
Right-to-Work (Indiana) × Post (δ_1)	0.005	0.006
	(0.00)	(0.01)
Control Variables	YES	YES
Neighboring States Only	NO	YES
Treated IN firms (firm-year observations)	23 (174)	23 (174)
Control Firms (firm-year observations)	3,550 (14,373)	237 (1,011)
Total Firms (Observations)	3,573 (14,547)	260 (1,185)
Adjusted R-squared	88%	92%

Table 5
Largest Pension Bargaining Unit (LPBU) Summary Statistics

This table presents summary statistics for our firm-level bargaining power construct, LPBU. Panel A presents means, standard deviations, and percentiles for LPBU. Panel B presents means and standard deviations for each of eight industry classifications (according to the Department of Labor) as well as for all firms in our sample. Firms are classified as unionized if there is evidence of collective bargaining from the EBSA Form 5500 database. LPBU is calculated using the EBSA Form 5500 database as described in Section 3.3. It is important to note that this is not a measure of total unionization but is instead the ratio of the largest collective bargaining unit (with respect to retirement pensions) to total employees.

Panel A: Sample Classified by Union Status

Group	Mean	StDev.	25^{th} Percentile	Median	75^{th} Percentile	Obs
Unionized Firms	34.49%	31.42%	6.59%	25.00%	57.83%	3,393

Panel B: Sample Classified by Industry

Division	Mean	StDev.	Obs
Agriculture, Forestry, and Fishing	1.23%	6.80%	93
Mining	3.61%	16.11%	1,143
Construction	> 0.00%	0.05%	213
Manufacturing	5.18%	16.68%	15,849
Transportation and Communications	10.09%	26.14%	1,484
Wholesale Trade	5.55%	19.49%	1,045
Retail Trade	1.20%	8.00%	2,248
Services	1.10%	8.67%	6,499
All Firms	4.10%	15.55%	28,574

Table 6
Summary Statistics and Univariate Tests of Equivalence - Panel Regression Analysis

This table tabulates summary statistics for all firms in our sample (Panel A) and for all firms by union status (Panel B). It also presents results of relevant univariate tests with a null hypothesis H_0: difference = 0. Firms are classified as unionized if there is evidence of collective bargaining from the EBSA Form 5500 database. All variables are defined in Appendix B. In order to assess differences in means (medians) we apply the t-test (Wilcoxon rank-sum test). ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Summary Statistics—All Firms

Variable	Mean	StDev	25th	Median	75th	Min	Max	Obs
Financial Debt to Book Assets	0.17	0.18	0.00	0.12	0.29	0.00	0.69	28,574
Financial Debt to Book Capital	0.24	0.24	0.00	0.17	0.4	0.00	0.91	28,574
Long-term Financial Debt to Book Assets	0.15	0.17	0.00	0.09	0.26	0.00	0.68	28,574
Long-term Financial Debt to Book Capital	0.21	0.24	0.00	0.13	0.37	0.00	0.89	28,574
LPBU (Largest Pension Bargaining Unit)	0.04	0.16	0.00	0.00	0.00	0.00	1.00	28,574
Size (logarithm)	5.40	2.17	3.95	5.44	6.87	-0.37	10.58	28,574
Size (real $mil)	1,784	5,407	52	229	959	0.69	39,510	28,574
Tangibility	0.23	0.21	0.07	0.16	0.32	0.01	0.89	28,574
Market-to-Book	1.82	1.78	0.80	1.22	2.09	0.28	11.06	28,574
ROA	0.05	0.21	0.01	0.10	0.16	-0.89	0.40	28,574
Zprob	0.75	3.52	0.41	1.63	2.56	-17.53	5.16	28,574
R&D Intensity	0.37	1.75	0.00	0.01	0.12	0.00	14.75	28,574
Advertising Intensity	0.01	0.03	0.00	0.00	0.01	0.00	0.18	28,574
Tobin's Q	1.50	1.74	0.53	0.96	1.77	-0.22	10.63	28,574
Earnings Vol.	0.08	0.11	0.02	0.04	0.09	0.00	3.32	19,728
Capital Expenditures	0.05	0.05	0.02	0.03	0.06	0.00	0.31	28,458

Table 6 cont.

Panel B: Summary Statistics by Union Status & Tests of Equivalence

Variable	Non-Unionized Firms Mean	Non-Unionized Firms Median	Non-Unionized Firms StDev	Unionized Firms Mean	Unionized Firms Median	Unionized Firms StDev
Financial Debt to Book Assets	0.16***	0.10***	0.18	0.25	0.24	0.16
Financial Debt to Book Capital	0.22***	0.14***	0.24	0.37	0.37	0.23
Long-term Financial Debt to Book Assets	0.14***	0.07***	0.17	0.23	0.23	0.16
Long-term Financial Debt to Book Capital	0.20***	0.10***	0.23	0.35	0.34	0.23
LPBU (Largest Pension Bargaining Unit)	0.00***	0.00***	0.00	0.34	0.25	0.31
Size (logarithm)	5.14***	5.17***	2.09	7.33	7.33	1.72
Size (real $mil)	1,275***	176***	4,267	5,567	1,526	9,742
Tangibility	0.22***	0.14***	0.22	0.32	0.29	0.19
Market-to-Book	1.91***	1.27***	1.86	1.16	0.96	0.76
ROA	0.04***	0.10***	0.22	0.13	0.13	0.08
Zprob	0.58***	1.55***	3.69	2.02	1.98	1.20
R&D Intensity	0.42***	0.01***	1.86	0.02	0.00	0.26
Advertising Intensity	0.01***	0.00***	0.03	0.01	0.00	0.02
Tobin's Q	1.57***	1.00***	1.82	0.97	0.81	0.76
Earnings Vol.	0.08***	0.05***	0.12	0.04	0.02	0.05
Capital Expenditures	0.05***	0.03***	0.06	0.05	0.04	0.03

Table 7
High-Dimensional Fixed Effects Regressions of Bargaining Power and Financial Leverage

Panel A of this table presents the results from panel regressions regressions using two alternative measures of book leverage on Largest Pension Bargaining Unit (LPBU) and a vector of controls. Columns 1, 2, 3, and 4 of Panel B present the results of the same regressions as Panel A for four additional measures of leverage. Column 5 of Panel B presents the results of a regression on the subsample of firms with positive-leverage, positive-LPBU, and a Cook's Distance of less than 4 divided by the number of observations. In all regressions, the key variable of interest is the LPBU coefficient, i.e., β_1. H1b (Bargaining Hypothesis) predicts a positive $\beta_1 > 0$, while H2b (Crowding-out Hypothesis) predicts a negative $\beta_1 < 0$. All variables are defined in Appendix B. Compustat variables are winsorized at the 1% and 99% tails. All regressions include an intercept term as well as both industry-by-year and firm fixed effects. Heteroskedastic-consistent standard errors, clustered at the firm-level are provided in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level respectively. [a] The restrictions placed on the observations included in this specification result in a significant reduction in sample size.

Panel A: Financial Book Leverage

		(1)	(2)	(3)	(4)
		Dependent Variable			
Variables	Predicted Sign	Financial Debt to Book Assets	Financial Debt to Book Capital	LT Financial Debt to Book Assets	LT Financial Debt to Book Capital
LPBU (β_1)	H1b: $\beta_1 > 0$	-0.018*	-0.025**	-0.019**	-0.027**
	H2b: $\beta_1 < 0$	(0.01)	(0.01)	(0.01)	(0.01)
Size		0.017***	0.025***	0.015***	0.022***
		(0.00)	(0.00)	(0.00)	(0.00)
Market to Book Ratio		-0.005***	-0.007***	-0.005***	-0.007***
		(0.00)	(0.00)	(0.00)	(0.00)
Profitability		-0.050***	-0.092***	-0.038***	-0.070***
		(0.01)	(0.02)	(0.01)	(0.01)
Tangibility		0.104***	0.163***	0.098***	0.154***
		(0.02)	(0.03)	(0.02)	(0.03)
Zprob		-0.005***	-0.008***	-0.004***	-0.006***
		(0.00)	(0.00)	(0.00)	(0.00)
Observations		28,574	28,574	28,574	28,574
Firm FE		YES	YES	YES	YES
Industry-by-year FE		YES	YES	YES	YES
Adjusted R-squared		78%	78%	78%	78%
R-squared within		10%	10%	10%	10%

Panel B: Financial Market Leverage and Total Leverage

		(1)	(2)	(3)	(4)	(5)
		Dependent Variable				
Variable	Predicted Sign	Financial Debt to Market Assets	Financial Debt to Market Capital	Total Liabilities to Market Assets	Total Liabilities to Book Assets	Financial Debt to Book Assets
LPBU (β_1)	H1b: $\beta_1 > 0$	-0.020**	-0.029**	-0.029**	-0.018*	-0.048*
	H2b: $\beta_1 < 0$	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
All Control Variables		YES	YES	YES	YES	YES
Firm FE		YES	YES	YES	YES	YES
Industry-by-Year FE		YES	YES	YES	YES	YES
Observations		28,530	28,530	28,530	28,574	1,594[a]
Adjusted R-squared		78%	79%	79%	79%	84%
R-squared within		10%	18%	18%	18%	41%

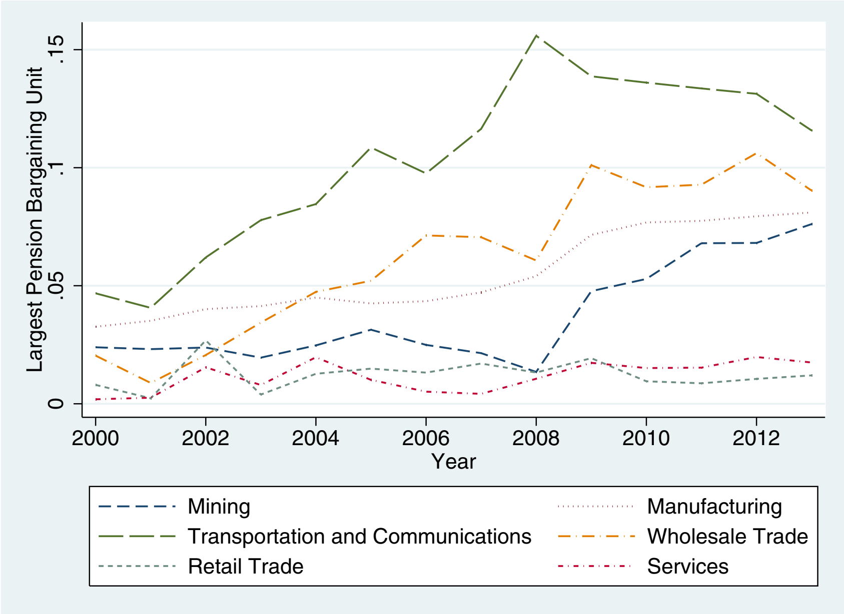

Figure 1: Bargaining Power of Labor (LPBU) Time Series

Figure 1 tracks the conditional density of the largest firm-level bargaining unit (with respect to retirement pensions) by industry division (for divisions with more than 1,000 firm-year observations), as measured by equation (3).

Appendix A: Summary and Predictions

Panels A1 and A2 of Appendix A provide a summary of extant predictions and previous empirical tests.

Panel A1: Summary of Extant Predictions

Paper	Treatments	Predicted Signs to Leverage	Interactions	Actual Signs	Sample Period
Bronars and Deere (1991)	Threat of unionization	+	No	n/a	1973
Dasgupta and Sengupta (1993)	Bargaining power	+	No	n/a	Not conducted
Perotti and Spier (1993)	Bargaining power	+ [1]	No	n/a	Not conducted
Cavanaugh and Garen (1997)	Bargaining power	+	Bargaining power × asset specificity	+	1973 to 1982
Sarig (1998)	Bargaining power	− [2]	No	n/a	Not conducted
Hennessy and Livdan (2009)	Bargaining power	+ [3]	No	n/a	Not conducted
Matsa (2010)	Bargaining power	+	Bargaining power × earnings volatility	+	Cross-section: 1977, 1987 and 1999 (concatenated) and exogenous shocks: 1950s, 1960s and early 1970s
Simintzi, Vig, and Volpin (2015)	Bargaining power	−	Bargaining power × staff turnover	−	Exogenous shocks: 1985 to 2007

Panel A2: Overview of Extant Treatment Variables and Empirical Tests

Paper	Bargaining Power Construct	Level	Market(s)	Sample Period
Bronars and Deere (1991)	Industry unionization	3 digit SIC using data from 1968 to 1972	U.S.	1973
Cavanaugh and Garen (1997)	Retrospective survey	Firm-level using unionization estimate from 1977	U.S.	1973 to 1982
Matsa (2010)	Retrospective survey and 10-K	Firm-level using unionization estimates from 1977, 1987, 1999 (10-K)	U.S.	Cross-section: 1977, 1987 and 1999 (concatenated) and exogenous shocks: 1950s, 1960s and early 1970s
Simintzi, Vig, and Volpin (2015)	Employee protection indicators	Firm-level / country-level	International	Exogenous shocks: 1985 to 2007
Cheng (2011)	10-K	Firm-level	U.S.	1999 to 2009

[1] Firms with low current profits and strong future investment prospects. [2] In the presence of firm-specific inputs. [3] In the presence of a significant level of self-enforcing, discretionary bonus contracts.

Appendix B: Variable Measurement and Motivation

Appendix B summarizes the motivations and measurement of dependent, independent and control variables used in our main financial leverage regressions and their predicted signs. Panel B1 summarizes all financial leverage proxies and the key independent variable, LPBU. Panel B2 summarizes control variables used in the final leverage regressions. Compustat mnemonics are provided in parentheses for ease of exposition.

Panel B1: Key Dependent and Independent Variables

Variable	Predicted Sign	Proxy Measurement
FDBCP	n/a	The ratio of financial debt to the book value of capital. $(dltt + dlc)/(dltt + dlc + seq + mib)$
FDAT	n/a	The ratio of financial debt to book assets. $(dltt + dlc)/at$
LFDBCP	n/a	The ratio of long-term financial debt to book capital. $(dltt + dd1)/(dltt + dlc + seq + mib)$
LFDAT	n/a	The ratio of long-term financial debt to book assets. $(dltt + dd1)/at$
FDMCP	n/a	The ratio of financial debt to market capital. $(dltt + dlc)/(dltt + dlc + prcc_f \times csho)$
FDMAT	n/a	The ratio of financial debt to market assets. $(dltt + dlc)/(at - (seq + mib) + prcc_f \times csho)$
LTMAT	n/a	The ratio of total liabilities to market assets. $lt/(at - (seq + mib) + prcc_f \times csho)$
LTAT	n/a	The ratio of total liabilities to book assets. lt/at
LPBU	+/−	The bargaining power of labor is the relative size of the largest collective bargaining unit (with respect to retirement pensions) at the firm-level. This is defined as the ratio of the number of active staff covered by the largest collectively bargained pension plan to the total number of employees.

Panel B2: Regression Controls

Variable	Predicted Sign	Proxy Measurement
Size	+ Rajan and Zingales (1995)	Firm size is the natural logarithm of sales (sale) in year 2009 real dollars
Tangibility	+ Rajan and Zingales (1995)	$ppent/at$
Market-to-book	− Rajan and Zingales (1995)	$(prcc_f \times csho + pstkl + dltt + dlc - txditc)/at$
ROA (return on assets)	− Rajan and Zingales (1995)	$ebitda/at$
Bankruptcy risk (ZProb)	Rajan and Zingales (1995)	ZProb is calculated following Mackie-Mason (1990) as $3.3 \times ebit/at + sale/at + 1.4 \times re/at + 1.2 \times wcap/at$

Panel B3: Additional Univariate Statistics

Variable	Proxy Measurement
Research and development intensity	Research and development intensity is defined as the ratio of research and development expense to sales. $xrd/sale$
Advertising intensity	Advertising intensity is defined as advertising expenses to net sales. $xad/sale$
Tobin's Q	Tobin's Q is defined as the ratio of the market value of equity plus the liquidating value of preference stock plus net debt to total assets. $(csho \times prcc_f + pstk + dltt + (lct - act))/at$
Earnings volatility	Earnings volatility is defined as the three year standard deviation in the change in EBITDA scaled by the three-year mean of total assets. $3\,year\,STDEV(\Delta ebitda)/3\,year\,lagged\,at$
Capital expenditures	Capital expenditure is defined as capital expenditures scaled by total assets. $capx/at$
Default spread	Default spread is employed to control for credit market conditions. Default spread is the difference between the yield on Baa and Aaa rated corporate bonds, measured as of the firm's fiscal-year month end
State GDP growth rate	State GDP growth rate is employed to control for state-level economic conditions. GDP growth rate is the real annual growth rate in gross state product using data obtained from the U.S. Bureau of Economic Analysis
State unemployment rate	State unemployment rate is employed to control for state-level economic conditions. State unemployment rate is the state unemployment rate, obtained from the U.S. Bureau of Labor Statistics

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