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Abstract

Using a regression discontinuity design, we provide evidence that incentive conflicts between firms and their creditors have a large impact on employees. There are sharp and substantial employment cuts following loan covenant violations, when creditors exercise their ex post control rights. The negative impact of violations on employment is stronger for firms that face more severe agency and financing frictions and those whose employees have weaker bargaining power. Employment cuts following violations are much larger during industry and macroeconomic downturns, when employees have fewer alternative job opportunities and reduced bargaining power. Union elections that create new labor bargaining units lead to higher loan spreads, consistent with creditors requiring compensation for their reduced control rights when labor is stronger. Overall, these findings enrich our understanding of the consequences of the state contingent transfer of control rights by identifying a risk-shifting channel from creditors to employees. Our analysis establishes an endogeneity-free link between financing frictions and employment and offers direct evidence that binding financial covenants are an important amplification mechanism of economic downturns.

1 Introduction

One fundamental contribution of modern corporate finance is the insight by Jensen and Meckling (1976) that firms are a complex nexus of contractual relations.[1] Important aspects of this original insight have been developed. In particular, the literature has extensively studied conflicts of interest between shareholders and managers and between shareholders and debtholders (see Stein (2003) for a survey). It is now well understood that these conflicts of interest can potentially be mitigated by contractual features such as, for example, financial covenants that protect lenders before non-payment or default by defining a state-contingent transfer of control rights. A growing recent empirical literature (Chava and Roberts (2008), Roberts and Sufi (2009), and Nini, Smith and Sufi (2009, 2012)) shows that loan covenants are indeed effective at protecting creditors' rights, and that the transfer of control rights that accompanies covenant violations has important consequences for firm investment and financial policies.

Another important insight of the nexus of contracts view has received relatively little attention. There can also be a fundamental conflict of interest between creditors and other stakeholders stemming from the fact that each of these groups has a priority claim on firm revenues. In Jensen and Meckling (1976), "firms incur obligations daily to suppliers, to employees, to different classes of investors, etc. So long as the firm is prospering, the adjudication of claims is seldom a problem. When the firm has difficulty meeting some of its obligations, however, the issue of the priority of those claims can pose serious problems." Since a complex web of multiple contracts ultimately determine the adjudication of claims, the allocation of rights between shareholders and creditors likely has an impact on contractual relations between the firm and nonfinancial stakeholders. However, we have virtually no empirical evidence on whether and how creditor rights actually impact nonfinancial stakeholders.

[1]"There is in a very real sense only a multitude of complex relationships (i.e., contracts) between the legal fiction (the firm) and the owners of labor, material and capital inputs and the consumers of output. The firm [. . .] is a legal fiction, which serves as a focus for a complex process in which the conflicting objectives of individuals (some of whom may "represent" other organizations) are brought into equilibrium within a framework of contractual relations. In this sense the "behavior" of the firm is [...] the outcome of a complex equilibrium process." (Jensen and Meckling (1976))

In an attempt to fill the gap in the literature, this paper examines conflicts of interest between creditors and an important class of nonfinancial stakeholders. Specifically, we assess the impact of loan covenant violations on employees. Why would covenant violations affect employees? Loan covenants which are tied to performance indicators protect creditors by defining a transfer of control rights when a covenant is violated (e.g., financial contracting theory of Aghion and Bolton (1992) and Dewatripont and Tirole (1994)). Such violations provide creditors with the same rights as would payment defaults, including the ability to accelerate any outstanding principal and to terminate any unused revolving credit facility, and lead to renegotiation of loans on less favorable terms to borrowers. In order to avoid acceleration and ensure continued access to credit, management may decide to reduce operating costs by cutting jobs after a covenant violation in an attempt to reassure creditors about the firm's ability to generate cash flows. Employees may also be affected directly by creditors' interventions in the form of "advising" management to reduce headcount and operating expenses, as exemplified by the following excerpt from the first quarter 10-Q filing of Interpharm Holdings in 2008:[2]

> Subsequently, on January 28, 2008, Wells Fargo informed the Company that it would consider providing the Company with credit availability on the condition that the Company (i) develops and implements a new operating plan focused on increasing the amount of eligible collateral and reducing costs and (ii) develop an alternative financing arrangement. Further, on February 5, 2008, the Company and Wells Fargo entered into the Forbearance Agreement [...] In connection with its negotiation of the Forbearance Agreement, the Company completed a restructuring of its operations on January 25, 2008 and submitted a new operating plan to Wells Fargo, which the Company believes will result in positive cash flow and net profits, and includes [...] reducing payroll and headcount by approximately 20%.

Consistent with this reasoning, we document large-sample evidence that loan covenant violations and the associated transfer of control rights to creditors lead to less job security for workers.

[2]Using keyword searches of SEC filings (with keywords such as employee or headcount or overhead reduction) we found several cases of a direct link between violations and employement in management discussion of violations. For instance, the annual 10-K filing of Meade Instruments Corp in 2008 reads as follows: "We are working with our lender on a potential amendment to our agreement to cure this technical default. Our restructuring plans include implementation of headcount reductions, corporate overhead and manufacturing costs." From the second quarter 10-Q filing of Advanced Materials in 2004: "The Company is in the process of attempting to cure its line of credit and term loan violations. Management has implemented a plan to reduce expenses and improve sales. Selling, general and administrative expenses for the first quarter of fiscal 2004 and 2003 were $397,000 and $499,000, respectively, a decrease of $102,000 or 20%. This decrease was due primarily to a reduction in the number of employees as the Company continues to improve individual productivity."

Ours is the first endogeneity-free evidence that financing frictions have a sizable adverse impact on employment and that binding contractual covenants are an amplification mechanism of downturns. This evidence contributes to the classical academic and policy debate on the influence of corporate financing on macroeconomic and financial stability (e.g., Bernanke and Gertler (1989); Sharpe (1994), Hanka (1998), Benmelech, Bergman, and Seru (2011), and Pagano and Pica (2011) focus on employment), a debate which has been recently revived in the aftermath of the financial crisis and the ensuing Great Recession of 2008 and 2009.

Specifically, we use a regression discontinuity design pioneered by Chava and Roberts (2008) to achieve identification and document sizable job cuts following a loan covenant violation, which are concentrated among firms with agency problems and whose employees have weak bargaining power.[3] Our sample consists of 11,536 firm-year observations for 2,265 unique US firms that have information on loan covenants in Dealscan and on employment and firm balance-sheet in Compustat between 1994 and 2010, which we complement with 3,129 hand-collected layoff announcements from major news sources to construct measures of employment that do not reflect workers' voluntary separations.[4] Our baseline estimates indicate that employment falls sharply in response to a covenant violation by about 18% per year, a reduction which is roughly three times as large as the median employment drop in the sample. The estimates are robust to restricting the sample to include only observations that are "close" to the covenant threshold, where violations can be plausibly considered a "quasi-random" treatment.[5]

Employment cuts subsequent to violations become as deep as 30% per year for firms with more severe agency problems, and as much as 27% for firms whose employees have weaker bargaining power. Investment cuts are instead bigger when employees have stronger bargaining power,

[3] A firm is classified to be in violation in any given year when the value of either its current ratio or its net worth falls below the corresponding contractual threshold. We examine broader covenants in our robustness analysis (Table 6).

[4] Layoff announcement are collected from the Wall Street Journal and other major news sources using Factiva and Lexis Nexis keyword searches.

[5] Our baseline results also pass several robustness checks, which include using a specification in changes, rather than levels; probit regression specifications of layoffs; alternative samples and definitions of covenant violations; and adding control variables to address omitted variable concerns.

which is consistent with firms substituting between their labor and capital margins of adjustment. These results are robust across several firm- and industry-level proxies of agency problems and labor bargaining.[6] Variation by agency proxies suggests that the state-contingent allocation of control rights to creditors is effective as a mechanism to mitigate underlying "quiet life" type frictions, which make managers reluctant to fire employees (see Bertrand and Mullainathan (2003), Cronqvist et al (2008), and Atanassov and Kim (2009) for evidence). Since labor rights drive a wedge between the impact of creditor rights on employment vs. investment, there is an interplay between creditor and labor rights. Overall, variation by labor bargaining power suggests that there is a "rivalry effect" between creditor and labor rights which is broadly consistent with the nexus of contracts perspective of Jensen and Meckling (1976).

The impact of violations on employment is outsized in bad times and relatively muted in good times, a finding that is robust across different proxies based on industry and macroeconomic activity.[7] For example, violations lead to employee cuts of about 29% in NBER recession years, and their impact was truly outsized at about 42% in the Great Recession of 2008 and 2009. There is even stronger evidence of time-series variation in the employment impact among firms with weaker labor bargaining power and among non-rated firms, which likely have less access to alternative sources of credit. The combination of a higher likelihood of violations and bigger job cuts when violations occur leads to an estimated expected impact on employment which is about 5 times larger in recession than in non-recession times, which is consistent with a fundamental tenet of much macroeconomics and finance that financing frictions exacerbate the real impact of downturns. Since employment cuts are concentrated in exactly those times when creditors have arguably the most bargaining power and labor has the least, time-series evidence further corroborates our interpretation that the impact of violations reflects the relative strength of creditor and

[6]The firm-level proxies of agengy and financing frictions include: cash holdings, Gompers, Ishii, and Metrick (2003) GIM-Index, book leverage, and the fraction of total debt with short maturity, as well as a dummy for rated status. The industry-level labor bargaining proxies include: degree of union representation, labor intensity and flexibility, domestic and foreign product market competition.

[7]We thank Michael Roberts for suggesting these tests of time-series variation.

labor rights.

Our evidence so far suggests that the ability of creditors to exercise their control rights is limited by labor rights. In our final set of tests, we ask whether loan terms impound labor rights and price in a premium for creditors' expected loss of effective control when labor is stronger. Our identification is a regression discontinuity design that exploits the requirement by U.S. labor laws that in order to create a new labor bargaining unit an election is held in which workers vote by majority rule for or against union representation. We match administrative data from the National Labor Relations Board (NLRB) on all union elections that took place in the US between 1985 and 2010 with loan pricing information from Dealscan and accounting data from Compustat, which results in a sample of 3,814 loans for 1,756 unique election events.

Union wins are reliably associated with higher spreads on loans originated within two years from the election,[8] and the effect of unionization is particularly large for low rated and non-rated borrowers which have most scope for state contingent transfer of control rights to creditors. This result continues to hold when we use a matched sample methodology and when we consider only "close" elections to address potential concerns about anticipation of election outcomes and omitted variables issues. This evidence suggests that creditors demand compensation when employees have more bargaining power, further corroborating our interpretation that labor rights limit the impact of creditor rights on employees. The analysis also contributes to the classical literature on the economic effects of unions (e.g., DiNardo and Lee (2004), Lee and Mas (2012)), which has traditionally abstracted from corporate control issues.

Overall, our analysis indicates that creditor rights increase employment risk. To the best of our knowledge, this is the first direct evidence consistent with the important implication of Jensen and Meckling (1976) that there are conflicts of interest between creditors and nonfinancial stakeholders with priority claims in asmuch as credit contracts that mitigate conflicts between debtholders and

[8] For example, in the overall sample, the mean loan spread for unions wins is 189.7 basis points, which is about 20 basis points higher than the mean loan spread for union losses. This 20 basis points differential is highly statistically and economically significant at about 10 percent of the (unconditional) sample mean of loan spreads.

shareholders have spillover effects on employees. We make two main additional contributions to the literature. First, our findings contribute to the literature on the real effects of the state contingent transfer of control rights (Chava and Roberts (2008), Roberts and Sufi (2009), and Nini, Smith, and Sufi (2009; 2012)) by identifying a risk-shifting channel from creditors to employees. Our results indicate that the state contingent allocation of control rights to creditors is effective as a mechanism to mitigate labor-related "quiet life" type agency frictions, which make managers reluctant to fire employees (see Bertrand and Mullainathan (2003), Cronqvist et al (2008), and Atanassov and Kim (2009)). By highlighting the interplay between labor rights and creditor rights and by exploring the link between labor bargaining rights and loan spreads, our results also fill the gap in the small but fast growing recent literature on labor and finance, which has so far mostly focused on capital structure decisions (e.g., Matsa (2010) and Agrawal and Matsa (2013)).

Second, we contribute to the literature on financing and employment by providing identification of financing effects.[9] Existing evidence is relatively scant, since the literature has mostly focused on financing and investment, and its interpretation is complicated by identification issues, since financing variables are likely correlated with future growth prospects and firm's demand for labor. In addition to establishing an endogeneity-free link between financing frictions and employment, we also offer direct evidence that binding financial covenants are an important amplification mechanism of economic downturns,[10] which supports theories such as Bernanke and Gertler (1989) where a deterioration of firm net worth amplifies the effect of economic downturns by exacerbating financing frictions. Our evidence highlights a specific channel for the credit restriction, namely covenant violations, and indicates that what makes bad times really bad for workers is that creditors are more likely to exercise their control rights at the same time when

[9]Previous research has focused on the effects of finance and investment (Fazzari, Hubbard, and Petersen (1988); Whited (1992)); the effect of finance on employment (Ofek (1993), Hanka (1998), Kaplan (1989), Muscarella and Vetsuypens (1990), Davis, Haltiwanger, Jarmin, Lerner, and Miranda (2008), and Benmelech, Bergman, and Seru (2011)). In addition, previous studies have documented real costs of bankruptcy, such as lost customers and employee relationships (Titman and Opler (1994)).

[10]The influence of corporate financing on macroeconomic and financial stability (e.g., Kyotaki and Moore (1997), Bernanke and Gertler (1989)) and, specifically, on employment fluctuations (Sharpe (1994)) is a classic topic in macroeconomics and finance.

employees have weaker bargaining power. While there is recent evidence that bankruptcies entail costs for workers (Graham et al (2013)), our evidence shows that the real effects of finance on employment are operative well before bankruptcy, which can help to explain why economic downturns lead to large job losses even when they do not trigger a large wave of bankruptcies, as in the case of the Great Recession of 2008 and 2009.

2 Analysis of Loan Covenant Violations and Employment

If the transfer of control rights to creditors has adverse consequences for employees, then there should be a negative effect of loan covenant violations on employment. In this section, we examine this hypothesis using the regression discontinuity design approach pioneered in this literature by Chava and Roberts (2008). After formally testing for the impact of violations on employees, we examine which factors are driving the impact and show that the impact of violations varies predictably in the cross-section and in the time-series, and is concentrated among firms with greater agency problems, those with greater bargaining power of creditors relative to employees, and in bad times when labor markets have less slack, which corroborates our interpretation of the baseline estimates.

2.1 Data and Sample Selection

Our sample consists of all Compustat firms incorporated in the United States that have relevant loan covenant information from Loan Pricing Corporation's (LPC) Dealscan database for the period 1994 to 2010 which, after applying standard data filters, results in a final set of 11,536 firm-year observations for 2,265 unique firms.

Our loan information comes from a 2011 extract of Loan Pricing Corporation's (LPC) Dealscan database. The data consist of dollar-denominated private loans made by bank (e.g., commercial and investment) and nonbank (e.g., insurance companies and pension funds) lenders to U.S. corporations during the period 1981 to 2010. Our sample construction strategy follows closely Chava and Roberts (2008) and Dichev and Skinner (2002). Thus, in this section we summarize the main parts of our sample construction strategy, detail the few parts where it differs from these papers, and refer to Chava and Roberts (2008) for further details. We start with the annual merged CRSP-Compustat database, excluding financial firms (SIC codes 6000-6999). While Chava and Roberts (2008) primarily use quarterly data, we use annual data because firms do not report employment at the quarterly frequency. We acknowledge that this data limitation is likely to make our assessment of when the covenant violation occurs more noisy, although Chava and Roberts document that their results also hold with annual data. All variables are defined in Appendix A.

Data from Compustat are merged with loan information from Dealscan by matching company names and loan origination dates from Dealscan to company names and corresponding active dates in the CRSP historical header file. The basic unit of observation in Dealscan is a loan, also referred to as a facility or a tranche. Loans are often grouped together into deals or packages. Most of the loans used in this study are senior secured claims, features common to commercial loans. Because information on covenants is limited prior to 1994, we focus our attention on the sample of loans with start dates between 1994 and 2010. Additionally, we require that each loan contains a covenant restricting the current ratio, or the net worth or tangible net worth (which we group together as net worth loans) to lie above a certain threshold.[11]

Since covenants generally apply to all loans in a package, we define the time period over which the firm is bound by the covenant as starting with the earliest loan start date in the package and ending with the latest maturity date. In effect, we assume that the firm is bound by the covenant for the longest possible life of all loans in the package. A firm is in violation of a covenant if the value of its accounting variable breaches the covenant threshold - i.e., when either the current ratio or the net worth falls below the corresponding threshold.[12] We focus on net worth and current

[11] We also require the covenant's corresponding accounting measure to be non-missing. We also manually recover some missing covenant information by looking at the package notes provided by Dealscan (package_comments).

[12] While conceptually straightforward, the measurement of the covenant threshold, and consequently the covenant

ratio covenants for two reasons, as elaborated by Chava and Roberts (2008) and Dichev and Skinner (2002). First, they appear relatively frequently in the Dealscan database.[13] Second, and most importantly, the accounting measures used for these two covenants are standardized and unambiguous. As the earlier papers documented, while other restrictions with debt or leverage may often be used, definitions can vary across contracts where debt can refer to long-term, short-term, secured, or other debt, making it difficult to define violations. In robustness analysis, we consider the impact of broadening the set of covenants. Since our focus does not discriminate between the two covenants, for the purpose of our regression analysis there is a violation if either of the two covenants is breached in any given firm-year.

We use two different measures of employment. One is the number of employees from Compustat. The second is a dummy variable which is equal to one for firm-years when there is either one of the 3,129 layoff announcements involving Compustat firms in the press, which we hand-collected from the Wall Street Journal and other major news sources obtained from Factiva and Lexis-Nexis news searches, or a reduction in the number of employees from Compustat (see Ofek (1993) for a similar variable). In addition to results reported for this basic layoff variable, we also report results for a medium-sized layoff dummy which corresponds to labor cuts of more than 5 percent of the workforce (which are those larger than the sample median of the distribution of job cuts), and a large-sized layoff dummy for cuts of more than 10 percent of the workforce (top quartile of the distribution of job cuts).

Table 1 provides summary statistics for the incidence of loan covenant violations as well as means and medians of our main dependent variable, employment, and standard firm and industry characteristics in the resulting sample of 11,536 firm-year observations for 2,265 unique firms that are bound by either a current ratio or a net worth covenant during the period 1994 to

violation, poses several challenges, such as the possibility of multiple overlapping deals, and, importantly, the fact that covenant thresholds can change over the life of the contract. We deal with these measurement issues following Chava and Roberts (2008) (see their Appendix B for details).

[13]Table I in Chava and Roberts (2008) shows that covenants restricting the current ratio or net worth are found in 9,294 loans (6,386 packages) with a combined face value of over a trillion dollars.

2010. By way of comparison, we also report summary statistics of these variables for other non-financial firms in Compustat. Appendix A provides sources and detailed definitions for each of these variables. Overall, our sample is comparable to those used in previous studies (Chava and Roberts (2008), Dichev and Skinner (2002)). As in these studies, our sample of Compustat firms with available loan covenant information contains firms that are somewhat larger, both in terms of assets and number of employees, and have higher cash flow, profits, and leverage relative to other firms in Compustat. The frequency of firm-year observations that are classified to be in violation is 20%, which is in line with the 15% frequency reported in Table 3 of Chava and Roberts (2008), considering that there is some time-aggregation due to the fact that our sample frequency is annual while theirs is quarterly and our longer sample period includes the Great Recession. An advantage of having a longer time-series than previous studies is that we can document some stylized time-series features of violations. In particular, the frequency of violations is markedly higher in NBER recession years, 27%, than in non-recession years, 18%.

2.2 Empirical Framework and Estimation Approach

Our empirical specification follows the approach of Chava and Roberts (2008) and exploits their insight that the "tightness" of loan covenants - i.e., the distance between the covenant threshold and the actual accounting measure - can be used to estimate the causal effect of financing. In particular, we consider covenant violations as the treatment and non-violations as the control, and adopt a regression discontinuity design approach. We can do so since the treatment effect is a discontinuous function of the distance between the underlying accounting variable and the covenant threshold. Specifically, our treatment variable, $Bind_{it}$, is defined as a dummy which equals one if $z_{it} - z_{it}^0 < 0$, where i and t index firm and year observations, z_{it} is the observed current ratio (or net worth), and z_{it}^0 is the corresponding threshold specified by the covenant.

Our baseline empirical model is

$$Emp_{i,t} = \alpha + \beta \times Bind_{i,t-1} + \gamma \times X_{i,t-1} + \eta_i + \lambda_t + \nu_{i,t} \quad (1)$$

where $Emp_{i,t}$ is (log) employment, $Bind_{it-1} = 1$ if $z_{it-1} - z^0_{it-1} < 0$ and zero otherwise is the covenant violation dummy, $X_{i,t-1}$ is a vector of control variables measured at the fiscal year-end prior to the year in which employment is measured, η_i is a firm fixed effect, λ_t is a year fixed effect, and $\nu_{i,t}$ is a random error term assumed to be correlated within firm and potentially heteroskedastic (Petersen (2006)). Controls include variables that have been previously employed in the loan covenants literature, such as firm size, profitability, and operating performance, as well as in employment regressions (Nickell (1984), Nickell and Wadhwani (1991)), such as total labor costs. In robustness analysis, we include additional controls such as market-to-book asset ratio, leverage, Altman's Z-score, and discretionary accruals.

The parameter of interest is β, which represents the impact of a covenant violation on employment (i.e., the treatment effect). Because of the inclusion of a firm-specific effect, identification of β comes only from within-firm time-series variation for those firms that experience a covenant violation. As noted in Chava and Roberts (2008), the nonlinear relation in equation (1) provides for identification of the treatment effect under very mild conditions. In fact, in order for the treatment effect β to not be identified, it must be the case that the unobserved component of employment ($\nu_{j,t}$) exhibits an identical discontinuity as that defined in equation (1), relating the violation status to the underlying accounting variable. That is, even if $\nu_{j,t}$ is correlated with the difference, $z_{it-1} - z^0_{it-1}$, our estimate of β is unbiased as long as $\nu_{j,t}$ does not exhibit precisely the same discontinuity as $Bind_{it-1}$.

Because the discontinuity is the source of identifying information, we also include smooth functions of the distance from the technical default boundary in our baseline specification.[14] In-

[14] More precisely, Default Distance (CR) and Default Distance (NW) are defined as Default Distance (CR) = I(Current Ratio$_{it}$)$\times$(Current Ratio$_{it}$ - Current Ratio$^0_{it}$), Default Distance (NW) = I(Net Worth$_{it}$)$\times$(Net Worth$_{it}$ - NetWorth$^0_{it}$), where I(Current Ratio$_{it}$) and I(Net Worth$_{it}$) are indicator variables equal to one if the firm-year observation is bound by a current ratio or net worth covenant, respectively. The Current Ratio$^0_{it}$ and Net Worth$^0_{it}$ variables correspond to the covenant thresholds.

cluding these variables helps to isolate the treatment effect to the point of discontinuity and addresses the concern that the distance to the covenant threshold may contain information about future investment opportunities not captured by the other controls. In addition, we report estimates of equation (1) using the subsample of firm-year observations that are close to the point of discontinuity. We follow Chava and Roberts (2008) and formally define the "Discontinuity Sample" as comprising firm-year observations for which the absolute value of the relative distance between the accounting variable and the corresponding covenant threshold is less than 0.20. This restriction reduces sample size to 4,469 firm-year observations,which is about 40% of the overall sample.

2.3 The Response of Employment to Covenant Violations: Baseline Results

Table 2 reports results of estimating equation (1) in the entire sample (Panel A) and in the discontinuity sample (Panel B), respectively. All the specifications include year and firm fixed effects, except for Column 4 which refers to a specification in changes with year and industry fixed effects. First, we replicate the results of Chava and Roberts (2008) on investment in our sample even with the addition of several recent years of data (Column 0). In the next sections we will use investment responses as a benchmark to assess alternative explanations for our results.

Moving to employment, covenant violations are associated with a sharp decline in employment of about 19% per year (Column 1). The economic magnitude of the job cuts is substantial, at about three times the 6% median yearly employment drop in the entire sample. In the discontinuity sample, violations lead to employment drops of roughly the same magnitude (Column 1, Panel B). Nonparametric analysis of average percentage annual changes in the number of employees in event time leading to and after the year when a violation occurs confirms that there is a sharp break in average employment in the year of violation ($t = 0$) and in the one immediately after ($t = 1$), which is of roughly the same magnitude as the regression estimates (Figure 1).[15]

[15]In the years prior to violation ($t = -4, -1$), employment changes are close to zero on average. Employment

Our estimates of the impact of violations on employment are robust to using alternative specifications. Column 2 addresses omitted variable concerns by incorporating standard control variables (firm size, total wages, cash flows, and ROA), and Column 3 adds smooth functions of the distance from the default boundary to further isolate the discontinuity corresponding to the covenant violation. In this full specification, covenant violations remain associated with a sharp decline in employment, which is about 18% per year. Signs of the coefficient estimates are as expected, and the inclusion of the controls has little effect on the estimated impact of violations. Column 4 reports estimates from the first difference analog to the fixed effects specification in equation (1), which examines the change in the number of employees for a given firm in a given year as a function of covenant violations, after controlling for changes in the control variables. Fixed effects and first differences estimators are both consistent under standard exogeneity assumptions (Wooldridge (2002)), thus making the comparison of the two specifications useful to assess whether our baseline equation is properly specified. The first difference specification yields estimates that are similar to the specification with fixed effects.

Finally, Columns 5 to 7 address the concern that the results may be driven by frictional voluntary separations rather than the firm decision to fire employees. We report estimates from a probit analog to the baseline OLS regression analysis, which examine the probability that layoffs occur for a given firm in a given year as a function of covenant violations and the full set of controls.[16] The impact on layoffs is both qualitatively and quantitatively in line with the results of the impact on the number of employees. Violations lead on average to a 19% higher likelihood of layoff in a given year (Column 5). The impact is nearly identical when we consider only more discrete medium-sized layoff events involving more than 5% of the workforce (Column 6), suggesting that frictional separations are unlikely to be driving our results. The coefficient estimate is smaller for

continues to shrink somewhat in the subsequent years ($t = 2, 3$), with the annual change in number of employees remaining negative and below its pre-violation average at about -8% per year on average.

[16]Layoffs have been considered in previous papers on financing and employment (see, for example, Hanka (1998)) and are a common focus in the empirical labor literature.

layoffs that involve more than 10% of the workforce but the impact remains economically large at about 10%, which is on the same order of magnitude as the unconditional likelihood of occurrence of such large layoffs in our sample (Column 7).

2.4 Cross-sectional Variation in the Employment Response: Evidence on Agency and Labor Bargaining Power

Based on our motivating theory and direct evidence from management discussion of covenant violations, we expect that there should be cross-sectional variation in the impact of violations on employment. First, the impact of violations should be larger for firms with more severe agency frictions, since covenants are designed to mitigate agency and financing problems and there is evidence supporting the "quiet life" hypothesis that agency problems make managers more reluctant to fire employees (see Bertrand and Mullainathan (2003), Cronqvist et al (2008), and Atanassov and Kim (2009)). Second, the impact should also be larger whenever employees are in a relatively weaker bargaining position with respect to creditors, since financial contracting theory suggests that employment cuts are brought about by a strengthening of creditor rights relative to labor rights. Next, we test these two hypotheses in turn.

Table 3 shows evidence of variation by several firm-level proxies of agency and financing frictions. In each year of the sample period, we rank firms based on the empirical distribution of these proxies, which include cash holdings (Column 1), Gompers, Ishii, and Metrick (2003) GIM-Index of antitakeover provisions (Column 2), book leverage (Column 3), and the fraction of total debt with short maturity (Column 4), as well as a dummy for rated vs. nonrated status (Column 5). Free cash flows (Jensen (1986)) and protection from disciplinary takeovers (Manne (1965)) are well-known to exacerbate agency problems. Leverage, especially when mostly short-term and costly to refinance, and lack of credit ratings increase financial constraints risk, thus potentially exacerbating risk-shifting (Jensen and Meckling (1976)). An alternative interpretation is that firms with high leverage, shorter debt maturities, and no credit ratings have less financial slack and fewer alternative borrowing opportunities, which increases their existing lenders' bargaining power and ability to exert influence upon violation.

We estimate the specification of equation (1) with the full set of controls and splines (Column 3 of Table 2) separately for the two groups of firms in the bottom and top quartiles of the (year-prior) distribution of each of the four continuous proxies in turn, and for rated vs. nonrated firms. We report results for the entire sample in Panel A and for the discontinuity sample in Panel B. For both samples and both outcome variables (number of employees and the layoff dummy), the negative impact of violations on employment is concentrated among firms that have higher cash-to-asset ratios and antitakeover protection, those that are highly leveraged and have more short maturity debt, and those with no credit rating (Rows 2, 4, 8, and 10). We also replicate the results in Chava and Roberts (2008) for investment across the different proxies (Rows 6 and 12). Overall, these results suggest that the state-contingent allocation of control rights to creditors helps to mitigate "quiet life" employment distortions, and especially so for firms whose creditors are in a stronger bargaining position at the time of violation.

Table 4 examines variation by various industry-level proxies of employees' bargaining power (Columns 1 to 4) and product market competition (Columns 5 and 6).[17] In each year of the sample period, we rank firms based on the empirical distribution of these proxies, which include measures of union representation (Columns 1 and 2), labor intensity and flexibility (Columns 3 and 4), and domestic and foreign product market competition (Columns 5 and 6). Organized representation through unions is well-recognized to increase labor bargaining power (Clark (1984), Hirsch (2008)). High labor to capital ratios reflect technological differences across industries in their mix of productive factors, but may also indicate greater labor overhang and weaker bargaining power. Bargaining power is also effectively higher when labor flexibility is lower, reflecting (sunk) costs of

[17]Using industry-level variables reduces the potential for simultaneity and for the case of labor intensity and flexibility is motivated by the intuition that, due to technological differences, the extent to which firms face different costs of adjusting labor varies across industries.

adjusting labor, which include hiring, training, and firing costs due to, for example, loss of human and organizational capital as well as firm-specific employees skills (Oi (1962), Hamermesh (1989), Eisfeldt and Papanikolaou (2011)). There is a classical theory literature and recent evidence that product market competition mitigates agency frictions (Hart (1983), Giroud and Mueller (2011)). Thus, we expect that the impact of violations on employment should be strongest for firms in industries with higher domestic product market concentration and lower import penetration.

We estimate the full specification with controls and splines (Column 3 of Table 2) of equation (1) separately for the two groups of firms in the bottom and top quartiles of the (year-prior) distribution of each of the industry-level proxies in turn, and report results for the entire sample in Panel A and for the discontinuity sample in Panel B. The employment impact of violations is concentrated among those firms that are in industries with lower union representation, higher labor intensity and flexibility, and those in less competitive industries (Rows 1-4, and 7-10). These results are robust for both samples and both outcomes variables (number of employees and the layoff dummy). By contrast, the impact of violations on investment is less negative in industries with lower union representation and those with higher labor intensity and flexibility (Rows 5-6, and 11-12). Thus, labor bargaining power drives a wedge between employment and investment responses, since the impact of violations on employment is smaller while the impact on investment is larger when labor has more bargaining power.

Overall, the cross-sectional variation by firm-level agency proxies suggests that the state contingent allocation of control rights to creditors is effective as a mechanism to mitigate underlying "quiet life" type frictions, which make managers reluctant to fire employees (see Bertrand and Mullainathan (2003), Cronqvist et al (2008), and Atanassov and Kim (2009) for evidence). Variation by labor bargaining suggests that there is an important interplay between creditor and labor rights, and that this interplay matters to understand how stronger creditor rights impact not only employment, but also investment. Labor rights drive a wedge between the impact of creditor rights on employment vs. investment, suggesting that there is a "rivalry effect" between creditor and labor rights which is broadly consistent with the nexus of contracts perspective of Jensen and Meckling (1976). Our finding of differential variation between investment and employment also offers an additional test of our identification strategy. Mechanical explanations of our employment effect as being simply driven by declining assets would predict that employment cuts should be concentrated among the same set of firms that cut investment, which counters the evidence.

2.5 Time-series Variation in the Employment Response: Evidence on Industry and Business Cycle Conditions

The influence of corporate financing on macroeconomic and financial stability (e.g., Kyotaki and Moore (1997), Bernanke and Gertler (1989)) and, specifically, on employment fluctuations (Ofek (1993), Sharpe (1994), Hanka (1998), and Davis, Haltiwanger, Jarmin, Lerner, and Miranda (2008)) is a classic topic in macroeconomics and finance. The recent financial crisis and the "great recession" in 2008 and 2009 with unemployment rates that peaked at 10 percent and 41 consecutive months of rates above 8 percent have revived the academic and policy interest in understanding the impact of financial frictions in the propagation of the business cycle shocks to employment. However, interpretation of existing evidence based on the relation between measures of financing such as leverage ratios or cash flows and employment is complicated by identification issues, since financing variables are likely correlated with future growth prospects and firm's demand for labor. Thus, we still do not have endogeneity-free evidence on whether financing frictions exacerbate the impact of downturns on employees. In addition, since labor markets have less slack in bad times, by exploiting time-series variation in employee bargaining power we can test whether the employment impact of violations is concentrated in bad times, when employees have less bargaining power and fewer outside job opportunities.

Table 5 reports results of this time-series tests, where we examine variation by various proxies of bad times (Columns 1 to 3) vs. good times (Columns 4 to 6). For each year of the sample period, we group firms into two bins based on whether or not (denoted by "Yes" or "No") in that year there is an industry downturn (Column 1), a recession based on the NBER dates (Column 2), the "great recession" (Column 3), an industry expansion (Column 4), the high-tech boom (Column 5), and the "great moderation" (Column 6). We estimate the full specification[18] of equation (1) separately for the two bins and report results for the entire sample in Panel A.[19]

The employment impact of violations is outsized in bad times and relatively muted in good times (Rows 1 and 3), a result that is robust across the different proxies of good and bad times and our two main outcomes variables (number of employees and the layoff dummy). For example, estimated responses imply that violations lead to employee cuts of about 29% in NBER recession years (Column 2, Row 1), and to even bigger cuts of about 42% in the Great Recession (Column 3, Row 1). Evidence of time-series variation in the investment impact is weaker. For example, the investment response in NBER recession periods is -0.9% (Column 2), which is about the same as the average investment impact in Table 2. The relatively less pronounced time-series variation in the investment impact with respect to the employment one is consistent with time-series variation in employees' bargaining power being an important driver of the employment response.

In Panel B we report results of the same set of tests when we further stratify the sample based on firm and industry characteristics that were used in the analysis of Tables 3-4 and the number of employees is the outcome. The firm-level characteristic we consider is firm credit rating status (Rows 7 and 8). There is solid evidence that firms that have access to bond markets tend to substitute bonds for loans in bad times (Kashyap, Stein, and Wilcox (1993), Ivashina and Becker (2011)). Based on this evidence as well as our results in Table 3, we expect the time-series effects to be concentrated among nonrated firms, which have less access to public debt markets. We also present results for one of our industry-level proxies for union representation, union membership

[18] With the full set of controls and splines (Column 3 of Table 2).

[19] Results for the discontinuity sample are qualitatively similar to those in Panel A and are omitted for brevity.

(Rows 9 and 10). Indeed, time-series variation in the employment impact is more pronounced for firms that do not have a credit rating (Row 7) and for those in industries with lower union membership (Row 9), which suggests that the interplay between creditors and labor bargaining power is an important factor behind the propagation effect of violations in downturns.

2.5.1 The employment response in recessions: a calibration

Theory suggests that financial contracting may exacerbate the effect of economic downturns on employment and our analysis in Table 5 offers direct evidence in support of this notion. But how large is the overall amplification effect of loan covenants? In order to facilitate a quantitative assessment of the economic magnitude of the employment impact of violations in bad times, we provide a simple calibration of the additional job cuts in recessions associated with a covenant violation. There are two related but distinct sources of amplification. First, as shown in Table 1, covenant violations are more frequent in bad times as measured by NBER recession years. Second, our estimates in Table 5 imply that employee cuts are bigger in response to any given violation. Again, based on the NBER definition, in non-recession periods violation leads to 8.9% cut in employees (Column 2, Row 2 of Table 5) and the frequency of bind is 18 percent (Table 1); in recessions, violation leads to 29% cut (Column 2, Row 1 of Table 5) and the frequency of bind is 27 percent. Putting these effects together, the implied expected impact of covenant violations on employment in recession times is given by $-0.078=0.27\times-0.29$, which is about 5 times larger than the impact in non-recession periods, $-0.016=0.18\times-0.089$. A similar calculation for investment suggests that there is also amplification, but much less than for employment: violations lead to an expected cut in investment in non-recession times of $0.001=0.006\times0.18$, vs. a cut in a recession of $0.0024=0.009\times0.27$, which is only twice as large. Thus, the interplay of labor and creditor rights leads to larger amplification effects of violations on employment than on investment.

2.6 Robustness

In Table 6 we examine the robustness of the employment impact of violations to four batteries of tests, which comprise using alternative specifications to address outliers and timing issues (Panel A), using alternative samples and definitions of covenant violations to address alternative explanations and potential measurement error issues (Panel B), and including additional control variables to address potential omitted variables concerns (Panels C and D). In all the tests, we take the full specification[20] of equation (1) as our starting point and report results for both the entire sample (Columns 1 and 3) and the discontinuity sample (Columns 2 and 4). Starting with Panel A, estimates from a median (quantile) regression specification are somewhat larger than OLS estimates (Row [1]), suggesting that outliers are unlikely to be driving our results. Adding a lagged dependent variable (Row [2]),[21] one more lag and two leads of Bind (Rows [3] and [4]) also leaves the estimated impact little changed, suggesting that sluggish employment dynamics and related timing issues are also not driving the results.

Moving to Panel B, estimates derived using an alternative definition of Bind based on the violation dummies hand-collected from SEC filings by Nini, Smith, and Sufi (2012), which are shown in Row [5], remain large and are of the same order of magnitude as our baseline estimates, suggesting that potential measurement error from not using actual violations is not an important concern. A broader definition of Bind that includes the full set of covenants with threshold information available in Dealscan (Row [6]) also leads to strongly statistically and economically significant estimates, though notably lower than our baseline, consistent with the reasoning in Chava and Roberts (2008) that net worth and current ratio covenants have most bite and are defined most unambiguously, thus giving rise to the least potential attenuation bias from measurement error. Excluding firm-years when there is a divestiture of assets (Row [7]) or the financial crisis (Row [8])

[20] With the full set of controls and splines (Column 3 of Table 2).

[21] We are aware of the issue that OLS estimates may be biased in small-T unbalanced panels with firm fixed effects and a lagged dependent variable. In additional robustness tests we have experimented with an IV-GMM estimation approach (Bond and Van Reenen (2007)), which yields similar coefficient estimates for the emplyment impact of violations.

has also little impact on our main estimates, which does not support alternative mechanical explanations based on indirect effects from simply shrinking firms or one-time financial circumstances. Panels C and D verify that our results are robust to controlling for several additional factors that might affect employment,[22] suggesting that omitted variables are not likely to be an important concern.

2.7 Summary of Results

In sum, the results so far show that covenant violations and the associated transfer of control rights to creditors lead to sizable employment cuts. In the cross-section, the employment cuts are concentrated among firms with more severe agency and financing frictions, as well as those whose creditors have more bargaining power and whose employees have less bargaining power, suggesting that the state contingent allocation of creditor control rights is an effective mechanism to mitigate underlying "quiet life" type agency frictions. In addition, our evidence suggests that there is an interplay between creditors and labor rights, since labor rights affect the overall effectiveness of the transfer of control rights to creditors, which is broadly consistent with the nexus of contracts perspective of Jensen and Meckling (1976).

The evidence of differential variation of the employment and the investment impact by labor power corroborates our interpretation that violations have a direct effect on employees because they lead to a transfer of control rights to creditors. A mechanical explanation of the effect of violations would predict cuts in both employment and investment which would not vary with labor bargaining power. Finally, our results on time-series variation of the employment impact of violations further corroborates our interpretation that the fundamental rivalry between creditor and labor rights is driving our results, since employment cuts are concentrated in exactly those times when creditors have the most and labor has the least bargaining power. The time-series analysis

[22] In particular, we include investment (Row [9]); a dummy for whether the firm undergoes a divestiture of assets in any given year (Row [10]); 2nd- and 5th-order non-liner splines of the distance from the covenant threshold (Rows [11] and [12]); book leverage (Row [13]); Tobin's Q (Row [14]); Altman's Z-score (Row [15]); and discretionary accruals (Row [16]). The estimated impact of covenant violations on employment is stable across all these different controls.

also offers the first endogeneity-free evidence that, consistent with a fundamental tenet of much macroeconomics and finance literature, financing frictions exacerbate the impact of downturns on employees.

3 Analysis of Union Elections and Loan Pricing

Our main evidence so far is that covenant violations lead to substantial employment cuts, but less so when labor has bargaining power, which suggests that labor rights limit creditors' control rights. In this section's additional analysis of loan pricing terms, we ask whether creditors anticipate that labor rights may limit their control rights upon violation of a covenant. If this is the case, then we expect that loan terms should impound the strength of labor bargaining rights and price in a premium for creditors' expected reduced effective control. These tests offer subsidiary evidence of an interplay between labor rights and creditor rights, which further corroborates our interpretation of the employment impact of violations. The analysis also contributes to the classical literature on the economic effects of unions (e.g., DiNardo and Lee (2004)), which has traditionally abstracted from corporate control issues and focused on the impact of unions on labor market outcomes, such as wages (see Lee and Mas (2012) for a recent study on unions and equity prices).

While there is solid evidence that unionized workers have stronger bargaining rights (Clark (1984), Hirsch (2008)),[23] empirical tests based on labor union representation face a classical endogeneity challenge: cross-sectional comparison of loan pricing between unionized and non-unionized firms is complicated by potential omitted variable bias if the two groups of firms differ along other characteristics that may affect loan prices. To overcome this challenge, we assemble a new dataset that combines information on elections to establish union representation with loan and firm information from Dealscan and Compustat. Our main identification is a regression dis-

[23] Which include bargaining over wages, pensions, and a variatey of work-related issues with the employer.

continuity design that exploits the requirement by US labor laws that in order to create a new labor bargaining unit, an election is held in which workers vote for or against union representation and a simple majority rule is followed to determine whether or not they become unionized. We look at yield spreads of loans issued in the two years following elections and ask whether there is a significant spread differential depending on whether the result is a win or a loss for unions. Results within a close range around the 50 percent majority threshold are our "discontinuity sample," within which election outcomes are plausibly a "quasi-random" experiment. We also complement these local estimates with a matched-sample analysis for the overall sample.

In the remainder of this section, after describing our sample selection and construction criteria, we summarize the results on the impact of unionization on loan pricing.

3.1 Data

We match administrative data from the National Labor Relations Board (NLRB) on all union elections that took place in the US between 1985 and 2010 with loan data from our 2011 extract of Dealscan for firms that have balance sheet variables available in Compustat.[24] This is a labor intensive task since it involves matching company names and union election dates for a very large number of events from NLRB to company names and corresponding active dates in the CRSP-Compustat historical header file. Since the bulk of our sample construction strategy follows closely the literature on the economic impact of unionization events (DiNardo and Lee (2004), and especially Lee and Mas (2012), whose Data Appendix we refer to for details), we only highlight our main innovations.

Availability of loan pricing information from Dealscan restricts our usable NLRB data with respect to previous studies, which generally rely on a longer time series (1961-) and the entire Compustat universe. Due to this constraint and in order to insure that our hypothesis testing

[24] Since we are not using loan covenant information for this part of the analysis, we are not constrained by covenant data availability and, hence, we can use the entire Dealscan sample.

has enough power even for the "discontinuity sample" defined within a narrow band around the 50 percent majority threshold, we take several steps to increase the sample size. The main step involves implementing a second-pass name match for NLRB firms that were not matched in the CRSP-Compustat historical header file, for which we used: (i) a list of historical company names retrieved from Capital IQ; and (ii) a list of historical links between CRSP-Compustat firms and the company names of their operating segments and subsidiaries also from Capital IQ.

Summary statistics for the final sample of 3,814 loan observations for 1,756 unique election events that have information on the percentage vote for unionization during the period 1985 to 2010 are tabulated in Table 7. Panel A reports means (and medians) for union election variables: a union win dummy, which is equal to one for any given election that results in a win for the union, and two important election characteristics, size, which is the number of employees involved, and percentage share of votes that were cast in favor of unionization. Overall, these statistics are broadly in line with those in previous studies (e.g., Lee and Mas (2012)), indicating that loan information availability from Dealscan does not lead to issues with selection from the NLRB universe. Firms in our sample are larger, more likely to be highly rated, and have somewhat lower spreads than other firms in Dealscan-Compustat, another feature we share with previous studies.

Finally, a simple diagnostic comparison of pre-event firm characteristics and loan spreads between firms where elections resulted in a win and those that resulted in a loss for the union is tabulated in Panel B for the overall sample, and in Panel C for the "discontinuity sample" of "close" elections, defined as a narrow range (a vote share range of $\pm5\%$) around the majority (50%) threshold needed for the union to win representation. While there are some residual differences in the overall sample, especially in terms of prior spreads, these differences go away in the discontinuity sample, which validates our key identifying assumption.

3.2 The Impact of Unionization on Loan Spreads: Results

Table 8 reports results of simple t-tests of differences between mean loan spreads in the first and in the second year after union elections (Columns (1) to (4) and Columns (5) to (8), respectively) depending on whether the election resulted in a win or a loss for the union. In Panel A, we report results for the entire sample (Columns (1), (5)) and for various sub-samples that exclude elections involving, in turn, operating subsidiaries (Columns (2), (6)), fewer than 150 employees (Columns (3), (7)), and those involving both fewer than 150 employees and investment grade-rated firms (Columns (4), (8)).

Union wins are associated with significantly higher loan spreads, a result which is robust to the different sub-samples and both time windows. In the overall sample, the mean loan spread for borrowers where unions win the election is 189.7 basis points, which is about 20 basis points higher than the mean loan spread for borrowers where unions lose (Column 1). This spread differential is not only statistically significant, but also economically significant at about 10 percent of the sample mean loan spread. The differential triples in magnitude when we consider relatively larger elections involving low rated and nonrated borrowers and it about doubles for loans issued two years after the election. Combined, these results suggest that the effect of unionization on loan spreads is long lasting and is concentrated among the firms that have most scope for state contingent transfer of control rights to creditors.[25]

In Panel B, we repeat these t-tests of differences but now sharpen our identification by exploiting the unique feature of the NLRB data that we can observe the percentage vote for unionization in any given election. We use the percentage vote variable to restrict the sample and include only "close" elections, which are defined as a narrow range (a vote share range of $\pm 5\%$) around the majority (50%) threshold needed for the union to win representation ("Discontinuity sample").[26]

[25]Using equity prices, Lee and Mas (2012) also find long lasting effects of unionization events.

[26]This regression discontinuity design is standard in the literature and relies on plausibly exogenous "local" variation in unionization around the 50% threshold, which is due to the fact that unions cannot control the assignment variable (votes) near the threshold.

The impact of unionization on loan spreads is larger than in the overall sample, with a premium for union wins now ranging between about 70 and 150 basis points. Graphical analysis in Figure 2, which plots mean loan spreads for each of ten bins of the data sorted on deciles of the union vote share variable, offer additional nonparametric evidence that there is a sharp break in average loan spreads around the 50% threshold.

Finally, Panel C shows that, when we further stratify the sample based on the number of employees involved in each election, the loan spread differential between union wins and losses increases monotonically with the size of the election. Since larger elections extend the reach of labor rights, they likely impose greater limitations on the state contingent transfer of control rights to creditors. Stronger results for the "discontinuity sample" and for larger elections suggest that anticipation of the election outcomes and other potential omitted variable issues lead, if anything, to downward biased estimates of the effect of unionization. Thus, the impact of unionization of loan spreads is unlikely to be an artifact of endogeneity issues.

3.2.1 Matched-Sample Analysis and Additional Robustness

In our last set of tests, we use a matched sample methodology analogous to long-run event-studies (e.g., Barber and Lyons (1997)) and construct a "benchmark" spread for a portfolio of loans matched on year, industry, and a variety of firm and loan characteristics. We use this approach to check whether the baseline results in Panel A of Table 8 are robust to controlling for common shocks occurring by chance that affect firms with similar characteristics. Results are reported in Table 9, which shows t-tests for the means of excess loan spreads in the two years after union elections, which are defined as the difference between loan spreads and the average loan spread for a portfolio of loans matched based on year, industry, and, in turn, (deciles of) firm size in Columns (1) to (4) of Panel A; growth opportunities (Market to book ratio) in Columns (5) to (8) of Panel B; credit ratings in Columns (1) to (4) of Panel C; and year-prior loan spreads in Columns (5) to (8) of Panel D. Robustly across these four benchmarks, union wins remain reliably associated with higher loan spreads. The differential in excess spreads between unions wins and union losses remains economically significant. For example, in the overall sample, the mean loan spread in excess of the benchmark based on year-prior spread is about 17 basis points, which is still about 10 percent of the sample mean loan spread (Panel D, Column 5). The spread differential is again much higher and ranging between about 60 and 90 basis points for relatively larger elections involving high-yield and nonrated borrowers. Thus, controlling for common shocks leaves our baseline results little changed.

We implemented a battery of additional robustness tests, which are not tabulated to conserve space.[27] In particular, we confirmed that the results in Table 8 are robust to the following: (i) addressing potential outliers by either repeating the t-test analysis on the logarithm of spreads or by using Mann-Whitney (z-statistic) tests; (ii) estimating a full-fledged polynomial regression of loan spreads on union win dummy, while controlling for smooth and higher order polynomials of the union vote share as well as standard firm and loan characteristics, and year and industry fixed effects. An additional advantage of this robustness check is that we have verified that the coefficient of the union win dummy remains statistically significant when we cluster standard errors at the firm level, which addresses the potential concern that multiple election events for the same borrower firm-year may affect our assessment of statistical significance in Table 8;[28] (iii) a series of placebo or falsification tests in which we take arbitrary thresholds for the union share vote and an associated "discontinuity" band and examine if unionization around these artificial thresholds is related to borrowers' post-election loan spreads. We found no statistical significance around the two placebo thresholds we considered (40% and 60%), suggesting that our baseline results are not spurious.

Overall, the analysis in this section indicates that union wins in elections to set up new la-

[27] Tabulations of the results are available upon request.

[28] We have also verified that the results are robust to retaining the outcome of the largest election only in the cases when there are multiple elections for any given firm-year.

bor bargaining units are associated with higher loans spreads, and especially so for firms that are rated below investment grade where creditors have more scope for mitigating risk-shifting issues through the state contingent transfer of control rights. This evidence suggests that creditors demand compensation when employees gain bargaining power. This evidence further corroborates our interpretation that there is a transfer of control rights to creditors in response to covenant violations and stronger labor rights mitigate the impact of creditor rights on employees.

4 Conclusion

Stronger creditor rights increase employment risk. We have provided robust evidence that loan covenant violations and the associated transfer of control rights to creditors have significant adverse effects on employment. In response to a loan covenant violation, employment drops by about 18% per year, with even deeper cuts for firms with more severe agency problems, those whose employees have relatively weaker bargaining power, and in times when industry and macroeconomic conditions are weak. Labor rights not only mitigate the employment impact of creditor rights, but also affect creditors' loan pricing decisions. Ours is the first direct evidence that even away from bankruptcy states there are conflicts of interest between creditors and other stakeholders with priority claims. Thus, our evidence suggests that credit contracts between debtholders and shareholders have spillover effects on nonfinancial stakeholders. In addition, our evidence shows that there are real effects of financial contracting on employment and that these effects are operative before debt default or bankruptcy, which we have argued can contribute to explain why economic downturns lead to large job losses even when they do not trigger a large wave of bankruptcies, as in the case of the Great Recession of 2008 and 2009.

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Appendix A: Variable Definitions

The variables used in this paper are extracted from four major data sources: Loan Pricing Corporation's (LPC) Dealscan database, COMPUSTAT, CRSP, and the National Labor Relations Board (NLRB). For each data item, we indicate the relevant source in square brackets. The variables are defined as follows:

Loan Covenants [Dealscan]:
Bind is a dummy that takes value of one if either net worth or current ratio fall below their respective loan covenant thresholds in any given firm-year.
NW is the net worth covenant threshold.
CR is the current ratio covenant threshold.

Outcome Measures:
Log(Employment) is the natural logarithm of the total number of employees (item 29). [Compustat]
Layoff (All) is a dummy that takes value of one if there is a decline in employment in any given year from the previous year. We complement Compustat data with information on 3,129 hand-collected layoff announcements from Wall Street Journal and other major news sources [Compustat, Factiva and Lexis Nexis news searches].
Layoff (Medium) is a dummy that takes value of one if there is a larger than average (5%) decline in employment in any given year from the previous year. We complement Compustat data with information on 3,129 hand-collected layoff announcements from Wall Street Journal and other major news sources [Compustat, Factiva and Lexis Nexis news searches].
Layoff (Big) is a dummy that takes value of one if there is a larger than 10% decline in employment in any given year from the previous year. We complement Compustat data with information on 3,129 hand-collected layoff announcements from Wall Street Journal and other major news sources [Compustat, Factiva and Lexis Nexis news searches].
Investment is capital expenditures (item 128) over net property, plant and equipment at the beginning of the fiscal year (item 8). [Compustat]
Loan spread is the all in spread on loans, including fees [Dealscan].

Union Elections [NLRB]:
Union Win is a dummy that takes value of one for any given election that results in a win for the union.
Union Election Size is the number of employees that are eligible to vote in any given election.
Union Vote Share is the percentage of votes cast in favor of the union in any given election.

Firm and Industry Variables:

Baseline Controls:
Log(Assets) is the natural logarithm of the book value of assets (item 6), deflated by CPI in 1990. [Compustat]
Total Wages is the natural logarithm of total labor expenses (item 42), deflated by CPI in 1990. [Compustat]
Cash Flow is the ratio of income before extraordinary items plus depreciation and amortization over the ratio of net property, plant and equipment at the beginning of the fiscal year to total assets. [Compustat]
Return on assets (ROA) is the ratio of operating income after depreciation (item 178) over lagged total assets (item 6). [Compustat]
Default Distance (NW) is the difference between the net worth covenant threshold and total assets minus total liabilities. [Compustat]

Default Distance (CR) is the difference between the current ration covenant threshold and the ratio of current assets to current liabilities. [Compustat]

Sample-Split Variables:

Cash Holdings is the ratio of cash holdings (item 1) to total assets (item 6). [Compustat]

GIM Index is the index of antitakeover provisions by Gompers, Ishii, and Metrick (2003).

Leverage is long term debt (item 9) plus debt in current liabilities (item 34) over the sum of long term debt (item 9) plus debt in current liabilities (item 34) plus market value of equity (item 25*item199). [Compustat]

Short Term Debt is the fraction of a firm's total debt that matures in three years or less. [Compustat]

Rating is a dummy variable that takes the value of one if the firm has an S&P Long-Term Domestic Issuer Credit Rating [Compustat]

Industry Unionization is the share of employees in any given industry-year that are members of a union (*Membership*) or covered by a collective bargaining agreement (*Coverage*).

Industry Labor Intensity & Labor Adjustment Costs are measured as the average ratio of number of employees to total assets (*Labor-Capital Ratio*) and the average ratio of capitalized selling, general, and administrative (SG&A) expenses to total assets (*Adjustment Costs*) [Compustat]

Industry Product Market Competition is measured by the Herfindahl-Hirschman Index (*HHI*) and the share of imports to the total value of shipments (*Import Penetration*) [Compustat]

Bad Times are measured as industry-years in the bottom quartile of sales growth (*Industry Downturn*), a dummy that takes value of one in NBER recession years (*NBER Recession*), and a dummy that takes value of one in 2008 and 2009 (*The Great Recession*) [Compustat & NBER]

Good Times are measured as industry-years in the top quartile of sales growth (*Industry Expansion*), and a dummy that takes value of one for firms in the semiconductors, computer manufacturing, and telecommunications sectors for the 1993-2000 period (*High Tech Boom*) and a dummy that takes value of one in the 1993-2000 period (*The Great Moderation*) [Compustat & NBER]

Additional Controls:

Tobin's Q (*M/B*) is the market value of assets divided by the book value of assets (item 6), where the market value of assets equals the book value of assets plus the market value of common equity less the sum of the book value of common equity (item 60) and balance sheet deferred taxes (item 74). [Compustat]

R&D is the ratio of R&D expenditures (item 46, or 0 is missing) over lagged sales (item 12). [Compustat]

Advertising is the ratio of advertising expenditures (item 45, or 0 if missing) over lagged total sales (item 12). [Compustat]

Free Cashflow is the ratio to total assets (item 6) of operating income before depreciation (item 13) less interest expense (item 15) and income taxes (item 16) and capital expenditures (item 128). [Compustat]

Altman's Z-Score is the sum of 3.3 times pre-tax income, sales, 1.4 times retained earnings, and 1.2 times net working capital all divided by total assets. [Compustat]

Accruals TWW and *Accruals DD* are as defined in Chava and Roberts (2008). [Compustat]

Table 1: Loan Covenant Sample: Summary Statistics

This table presents summary statistics (means and medians) for our merged Dealscan-Compustat sample, which consists of 11,536 firm-year observations for nonfinancial firms between 1994 and 2010 corresponding to firms that have at least one private loan found in Dealscan with a covenant that restricts current ratio or net worth to lie above a certain threshold (Columns 1 and 2). For the sake of comparison, Columns 3 and 4 report summary statistics (means and medians) for the Other Compustat sample, which consists of 110,058 firm-year observations for nonfinancial firms in the same period that have no matching information in Dealscan. Definitions for all variables are in Appendix A.

	Dealscan-Compustat		Other Compustat	
	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)
Loan Covenant Violations:				
Bind	0.20	0	n.a.	n.a.
Excluding NBER Recession Years	0.18	0	n.a.	n.a.
Only in NBER Recession Years	0.27	0	n.a.	n.a.
Employment Outcome Variables:				
Employees (000)	7.75	2.16	7.45	0.75
Layoff Dummy:				
All	0.38	0	0.38	0
Medium	0.22	0	0.20	0
Large	0.09	0	0.10	0
Capex	0.07	0.04	0.07	0.04
Firm Characteristics:				
Assets (Log)	5.9	6.0	5.1	4.9
Total Wages (Log)	4.7	4.5	4.3	4.5
Cash Flow	0.08	0.08	0.04	0.08
ROA	0.10	0.11	0.06	0.10
Net Worth	476	149	471	43
Tangible Net Worth	179.8	60.3	123.8	29.3
Current Ratio	2.10	1.76	2.30	1.79
Cash Holdings	0.10	0.05	0.20	0.10
GIM Index	9.30	9.00	9.20	9.00
Leverage	0.29	0.28	0.25	0.20
Short Term Debt	0.13	0.05	0.12	0.05
Rated Dummy	0.32	0	0.18	0
Tobin's Q	1.94	1.36	1.89	1.41
Industry Characteristics:				
Unionization	0.10	0.08	0.09	0.06
Labor Intensity	0.01	0.01	0.01	0.01
Competition (HHI)	0.23	0.18	0.22	0.17
Competition (Import Penetration)	0.24	0.21	0.24	0.20
Firm-Year Obs	11,536		110,058	
Firms	2,265		15,186	

Table 2: Loan Covenant Violations and Employment: Baseline Regression Analysis

This table presents regression results of employment on a covenant violation measure ("Bind") and controls. The dependent variable is the ratio of capital expenditures to assets at the start of the period in Column (0), to ensure comparability of samples to Chava and Roberts (2008); log employment in Columns (1) to (3), employment growth (the ratio of number of employees to lagged number of employees) in Column (4), and a dummy that takes value of one in any given firm-year when there is a layoff in Columns (5) to (7). All variable definitions are in Appendix A. All independent variables, except cash flow, are lagged one year. Panel A presents the results for the entire sample. Panel B only uses firm-year observations in which firms are close to violating the covenant, defined as a narrow range (±20%) around the covenant threshold ("Discontinuity sample"). Specifications in Columns (0) to (3) include both firm and year fixed effects, while Columns (4) to (7) include industry and year fixed effects. Standard errors robust to heteroskedasticity and within-firm serial correlation appear below point estimates. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	Panel A: Entire Sample							
Dependent Variable:	Investment	log(Employment)			Emp Growth	Layoffs		
	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model Specification:	FE No Controls	FE No Controls	FE Controls	FE Controls & Splines	OLS Controls & Splines	Probit All	Probit Medium	Probit Large
Bind	-0.010***	-0.194***	-0.174***	-0.176***	-0.158***	0.188***	0.192***	0.109***
	(0.003)	(0.025)	(0.022)	(0.022)	(0.027)	(0.014)	(0.015)	(0.010)
Log(Assets)			0.261***	0.258***	0.136**	-0.027***	-0.026***	-0.001
			(0.024)	(0.025)	(0.058)	(0.004)	(0.004)	(0.002)
Total Wages			0.042	0.041	0.089	-0.002	-0.001	-0.001
			(0.029)	(0.029)	(0.057)	(0.005)	(0.004)	(0.002)
Cash Flow			0.059	0.068	0.065**	-0.186***	-0.086**	-0.026*
			(0.115)	(0.115)	(0.032)	(0.048)	(0.033)	(0.016)
ROA			0.418***	0.415***	0.145***	-0.436***	-0.411***	-0.218***
			(0.122)	(0.122)	(0.035)	(0.074)	(0.059)	(0.032)
Default Distance (NW)				0.001	-0.004	-0.001	0.001	-0.000
				(0.008)	(0.005)	(0.003)	(0.002)	(0.001)
Default Distance (CR)				-0.019**	0.034	0.001	0.011	0.002
				(0.008)	(0.030)	(0.004)	(0.009)	(0.006)
Intercept	0.088***	0.331***	-0.867***	-0.846***	0.218***			
	(0.005)	(0.083)	(0.138)	(0.139)	(0.048)			
Firm Fixed Effects	Yes	Yes	Yes	Yes	No	No	No	No
Industry Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,536	11,426	11,065	11,065	10,965	10,865	10,951	10,951
	Panel B: Discontinuity Sample							
Bind	-0.008***	-0.162***	-0.140***	-0.134***	-0.161***	0.152***	0.111***	0.070***
	(0.003)	(0.031)	(0.030)	(0.030)	(0.040)	(0.026)	(0.026)	(0.018)
Observations	4,469	4,403	4,193	4,193	4,179	4,055	4,091	4,091

Table 3: Loan Covenant Violations and Employment: By Proxies of Agency and Financing

This table presents regression results of employment on a covenant violation measure ("Bind") and controls for different sub-sample splits of the data based on ex-ante proxies for the severity of agency (Columns (1) and (2)) and financing (Columns (3) to (5)) frictions faced by firms. The model specification is the one with firm fixed effects, controls, and splines as in Column (3) of Table 2 and the dependent variable is log employment in Rows [1]-[2] and [7]-[8], a dummy that takes value of one in any given firm-yearn when there is a layoff in Rows [3]-[4] and [9]-[10], and the ratio of capital expenditures to assets at the start of the period in Rows [5]-[6] and [11]-[12]. All variable definitions are in Appendix A. We only report estimates of the Bind coefficient and omit estimates of firm controls from the table for brevity (available upon request). Panel A presents the results for the entire sample. Panel B only uses firm-year observations in which firms are close to violating the covenant, defined as a narrow range ($\pm$20%) around the covenant threshold ("Discontinuity sample"). These samples are split between bottom and top quartiles of (year-prior) values of the following ex-ante proxies of agency frictions: cash holdings (Column (1)), and Gompers, Ishii, and Metrick (2003) GIM index of antitakeover provisions (Column (2)); and between bottom and top quartiles of (year-prior) values of the following ex-ante proxies of financing frictions leverage (Column (3)) and the fraction of total debt with short maturity (Column (4)), as well as credit rating status (Column (5)). Standard errors robust to heteroskedasticity and within-firm serial correlation appear below point estimates. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

		Agency		Financing			
		(1)	(2)	(3)	(4)	(5)	
		Cash Holdings	GIM Index	Leverage	Short Term Debt	Credit Rating	
Panel A: Entire Sample							
Log(Employment)							
[1]	Q1	-0.049	-0.065	-0.052	-0.086	Yes	-0.095**
		(0.042)	(0.076)	(0.050)	(0.068)		(0.039)
[2]	Q4	-0.228***	-0.303***	-0.200***	-0.243***	No	-0.185***
		(0.077)	(0.072)	(0.044)	(0.079)		(0.028)
Layoffs, Probit							
[3]	Q1	0.053***	0.023	0.058**	0.032	Yes	0.012
		(0.017)	(0.012)	(0.030)	(0.024)		(0.012)
[4]	Q4	0.156***	0.128***	0.154***	0.187***	No	0.112***
		(0.034)	(0.060)	(0.017)	(0.028)		(0.012)
Investment							
[5]	Q1	-0.003	-0.003	-0.002	-0.001	Yes	-0.006
		(0.006)	(0.006)	(0.007)	(0.006)		(0.004)
[6]	Q4	-0.020***	-0.011**	-0.013**	-0.011**	No	-0.014***
		(0.006)	(0.006)	(0.006)	(0.004)		(0.003)
Panel B: Discontinuity Sample							
Log(Employment)							
[7]	Q1	-0.035	-0.038	-0.037	-0.064	Yes	-0.052
		(0.058)	(0.140)	(0.078)	(0.074)		(0.054)
[8]	Q4	-0.233***	-0.228***	-0.217***	-0.267**	No	-0.182***
		(0.078)	(0.083)	(0.067)	(0.117)		(0.036)
Layoffs, Probit							
[9]	Q1	0.041**	0.028	0.021	0.024	Yes	0.013
		(0.021)	(0.019)	(0.028)	(0.025)		(0.024)
[10]	Q4	0.148***	0.178**	0.189***	0.196***	No	0.124***
		(0.036)	(0.116)	(0.023)	(0.035)		(0.016)
Investment							
[11]	Q1	-0.002	-0.006	-0.002	-0.006	Yes	-0.006
		(0.005)	(0.007)	(0.008)	(0.006)		(0.004)
[12]	Q4	-0.019**	-0.022**	-0.014***	-0.016**	No	-0.013***
		(0.008)	(0.011)	(0.006)	(0.007)		(0.004)

Table 4: Loan Covenant Violations and Employment: By Proxies of Labor Bargaining Power

This table presents regression results of employment on a covenant violation measure ("Bind") and controls for different sub-sample splits of the data based on ex-ante proxies for industry unionization (Columns (1) and (2)), labor intensity and flexibility (Columns (3) and (4)) and product market competition (Columns (5) and (6)). The model specification is the one with firm fixed effects, controls, and splines as in Column (3) of Table 2 and the dependent variable is log employment in Rows [1]-[2] and [7]-[8], a dummy that takes value of one in any given firm-yearn when there is a layoff in Rows [3]-[4] and [9]-[10], and the ratio of capital expenditures to assets at the start of the period in Rows [5]-[6] and [11]-[12]. All variable definitions are in Appendix A. We only report estimates of the Bind coefficient and omit estimates of firm controls from the table for brevity (available upon request). Panel A presents the results for the entire sample. Panel B only uses firm-year observations in which firms are close to violating the covenant, defined as a narrow range (±20%) around the covenant threshold ("Discontinuity sample"). These samples are split between bottom and top quartiles of (year-prior) values of the following ex-ante industry-level proxies union membership (Column (1)) and coverage (Column (2)); the average industry ratio of employees to assets (Column (3)) and SG&A to assets (Column (4)), the Herfindahl-Hirschman Index (HHI) (Column (5)) and the degree of import penetration (Column (6)). Standard errors robust to heteroskedasticity and within-firm serial correlation appear below point estimates. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

		Unionization		Labor Intensity & Flexibility		Competition	
		(1)	(2)	(3)	(4)	(5)	(6)
		Membership	Coverage	Labor-Capital Ratio	Labor Adj Costs	HHI	Import Penetration
Panel A: Entire Sample							
Log(Employment)							
[1]	Q1	-0.223***	-0.270***	-0.066*	-0.216***	-0.106*	-0.212***
		(0.082)	(0.094)	(0.037)	(0.043)	(0.063)	(0.065)
[2]	Q4	-0.074	-0.075	-0.269***	-0.061	-0.257***	-0.141**
		(0.047)	(0.047)	(0.044)	(0.049)	(0.055)	(0.065)
Layoffs, Probit							
[3]	Q1	0.134***	0.166***	0.029**	0.131***	0.035**	0.098***
		(0.033)	(0.038)	(0.016)	(0.017)	(0.017)	(0.036)
[4]	Q4	0.025	0.012	0.160***	0.030*	0.138***	0.033
		(0.018)	(0.014)	(0.016)	(0.016)	(0.014)	(0.024)
Investment							
[5]	Q1	-0.002	-0.003	-0.013**	-0.003	-0.002	-0.014**
		(0.004)	(0.004)	(0.006)	(0.003)	(0.007)	(0.006)
[6]	Q4	-0.010**	-0.009***	-0.006**	-0.017***	-0.013***	-0.002
		(0.004)	(0.004)	(0.003)	(0.007)	(0.004)	(0.004)
Panel B: Discontinuity Sample							
Log(Employment)							
[7]	Q1	-0.213***	-0.265***	-0.056	-0.244***	-0.098	-0.213**
		(0.078)	(0.095)	(0.058)	(0.059)	(0.131)	(0.105)
[8]	Q4	-0.031	-0.056	-0.265***	-0.062	-0.223***	-0.131
		(0.069)	(0.058)	(0.095)	(0.080)	(0.071)	(0.095)
Layoffs, Probit							
[9]	Q1	0.139***	0.180***	0.001	0.140***	0.016	0.112***
		(0.037)	(0.041)	(0.029)	(0.021)	(0.023)	(0.051)
[10]	Q4	0.014	0.001	0.180***	0.045	0.137***	0.047
		(0.024)	(0.029)	(0.041)	(0.035)	(0.019)	(0.037)
Investment							
[11]	Q1	-0.002	-0.003	-0.012**	-0.001	-0.005	-0.015**
		(0.004)	(0.004)	(0.005)	(0.005)	(0.007)	(0.006)
[12]	Q4	-0.011**	-0.012**	-0.003	-0.017**	-0.013***	-0.001
		(0.005)	(0.005)	(0.004)	(0.008)	(0.005)	(0.006)

Table 5: Loan Covenant Violations and Employment in Good and Bad Times

This table presents regression results of employment on a covenant violation measure ("Bind") and controls for different sub-sample splits of the data based on proxies for macroeconomic conditions. The model specification is the one with firm fixed effects, controls, and splines as in Column (3) of Table 2 and the dependent variable is log employment in Rows [1]-[2] and [7]-[12], a dummy that takes value of one in any given firm-yearn when there is a layoff in Rows [3]-[4], and the ratio of capital expenditures to assets at the start of the period in Rows [5]-[6]. All variable definitions are in Appendix A. We only report estimates of the Bind coefficient and omit estimates of firm controls from the table for brevity (available upon request). Panel A presents the results for the entire sample, which is split based on several proxies between good (Columns (1) to (3) and bad (Columns (4) to (6)) times. Panel B further stratifies the sample based on credit rating status (Rows [7] and [8]), and industry union membership (Rows [9] and [10]). Standard errors robust to heteroskedasticity and within-firm serial correlation appear below point estimates. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

		Bad Times			Good Times		
		(1) Industry Downturn	(2) NBER Recession	(3) The Great Recession	(4) Industry Expansion	(5) High Tech Boom	(6) The Great Moderation
Panel A: Entire Sample							
Log(Employment)							
[1]	Yes	-0.246***	-0.290***	-0.424***	-0.057	-0.023	-0.088***
		(0.037)	(0.057)	(0.099)	(0.041)	(0.068)	(0.028)
[2]	No	-0.109***	-0.089***	-0.089***	-0.239***	-0.230***	-0.194***
		(0.024)	(0.020)	(0.020)	(0.025)	(0.067)	(0.030)
Layoffs, Probit							
[3]	Yes	0.198***	0.157***	0.198***	0.019	0.053	0.064***
		(0.024)	(0.028)	(0.059)	(0.024)	(0.060)	(0.015)
[4]	No	0.053***	0.058***	0.095***	0.146***	0.167***	0.132***
		(0.012)	(0.010)	(0.009)	(0.012)	(0.045)	(0.014)
Investment							
[5]	Yes	-0.009***	-0.009**	-0.012**	-0.006	-0.009**	-0.008***
		(0.003)	(0.004)	(0.005)	(0.005)	(0.004)	(0.002)
[6]	No	-0.007***	-0.006***	-0.005***	-0.010***	-0.008**	-0.007***
		(0.002)	(0.002)	(0.001)	(0.002)	(0.004)	(0.002)
Panel B: Row 1 By Firm and Industry Characteristics							
By Firm Credit Rating Status							
[7]	Rated	-0.112**	-0.159***	-0.083	-0.046	-0.002	-0.022
		(0.048)	(0.061)	(0.200)	(0.093)	(0.102)	(0.040)
[8]	Not Rated	-0.315***	-0.354***	-0.709***	-0.066	-0.036	-0.086***
		(0.086)	(0.077)	(0.201)	(0.048)	(0.066)	(0.032)
By Industry Unionization (Union Membership)							
[9]	Q1	-0.340***	-0.367***	-0.612***	-0.091	-0.105	-0.102**
		(0.094)	(0.093)	(0.153)	(0.064)	(0.083)	(0.044)
[10]	Q4	-0.148*	-0.188**	0.203	-0.046	-0.001	-0.023
		(0.078)	(0.078)	(0.123)	(0.059)	(0.091)	(0.031)

Table 6: Loan Covenant Violations and Employment: Robustness Analysis

In this table, we check for robustness of the impact of violations on employment presented in Table 2 to using alternative specifications (Panel A), alternative samples and definitions of covenant violations (Panel B), and to including additional controls (Panels C and D). In all robustness checks, the starting model specification is the one with firm fixed effects, controls, and splines as in Column (3) of Table 2 and the dependent variable is log employment. We only report estimates of the covenant violation coefficient and omit estimates of firm controls from the table for brevity (available upon request). Columns 1 and 3 present the results for the entire sample. Columns 2 and 4 only use firm-year observations in which firms are close to violating the covenant, defined as a narrow range ($\pm$20%) around the covenant threshold ("Discontinuity sample"). Panel A reports results from the following specifications: a median (quantile) regression specification in Row [1]; a specification that adds a lagged dependent variable in Row [2]; and specifications that add one more lag and two leads of Bind in Rows [3] and [4], respectively. Panel B shows results for: using an alternative definition of Bind based on the violation dummies hand-collected from SEC filings by Nini, Smith, and Sufi (2012) in Row [5]; using a broader definition of Bind that includes the full set of covenants that have threshold information available in Dealscan in Row [6]; using a smaller sample that excludes observations when there is a divestiture of assets in Row [7]; and using a smaller sample that excludes the years of the financial crisis in Row [8]. Panels C and D present results for specifications that include the following additional controls: investment in Row [9]; a dummy for whether the firm undergoes a divestiture of assets in any given year in Row [10]; 2nd- and 5th-order non-liner splines of the distance from the covenant threshold in Rows [11] and [12], respectively; book leverage in Row [13]; Tobin's Q in Row [14]; Altman's Z-score in Row [15]; and discretionary accruals in Row [16]. All variable definitions are in Appendix A. Standard errors robust to heteroskedasticity and within-firm serial correlation appear below point estimates. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

Robustness Test	Estimated Coeff, log(Employment)		Robustness Test	Estimated Coeff, log(Employment)	
	Entire Sample	Disconti-nuity		Entire Sample	Disconti-nuity
	(1)	(2)		(3)	(4)
Panel A: Alternative Specifications			Panel C: Additional Controls		
			Controlling for:		
[1] Median (quantile) regression	-0.206***	-0.218***	[9] Investment	-0.175***	-0.126***
	(0.030)	(0.048)		(0.021)	(0.032)
[2] Include lagged dependent	-0.133***	-0.106***	[10] Divestitures	-0.176***	-0.131***
	(0.014)	(0.022)		(0.021)	(0.032)
[3] Include two lags of Bind	-0.152***	-0.116***	[11] 2-nd Splines	-0.171***	-0.126***
	(0.039)	(0.041)		(0.021)	(0.030)
[4] Include two leads of Bind	-0.151***	-0.115***	[12] 5-th Splines	-0.169***	-0.117***
	(0.028)	(0.036)		(0.021)	(0.031)
Panel B: Alternative Covenants and Samples			Panel D: Other Additional Controls		
			Controlling for:		
[5] Use violations dummy from	-0.120***	n.a.	[13] Leverage	-0.172***	-0.142***
Nini, Smith, and Sufi (2012)	(0.024)	n.a.		(0.022)	(0.030)
[6] Include all other covenants	-0.070***	-0.092***	[14] Tobin's Q	-0.175***	-0.132***
	(0.011)	(0.027)		(0.023)	(0.032)
[7] Exclude divesting firm-years	-0.162***	-0.113***	[15] Z-Score	-0.163***	-0.102***
	(0.022)	(0.028)		(0.022)	(0.031)
[8] Exclude financial crisis years	-0.163***	-0.116***	[16] Accruals	-0.164***	-0.128***
	(0.023)	(0.033)		(0.023)	(0.032)

Table 7: Union Election Sample: Summary Statistics

This table presents summary statistics (means and medians) for our merged Union (NLRB)-Dealscan (DS)-Compustat sample, which consists of 3,814 observations for nonfinancial firms between 1985 and 2010 corresponding to loans that have union representation election information in NLRB for firms in Compustat and loan pricing information in Dealscan one year after the union election event (Column 1 and 2, Panel A). For the sake of comparison, Columns 3 and 4 (Panel A) report summary statistics (means and medians) for the Other Dealscan-Compustat sample, which consists of the remaining observations in the merged Dealscan-Compustat sample for the same period that have no matching information in NLRB. Panel B reports summary statistics (means and medians) for two sub-samples of the Union (NLRB)-Dealscan (DS)-Compustat sample, based on whether the representation election results in a win or a loss for the union. Panel C reports summary statistics (means and medians) for two more sub-samples of the Union (NLRB)-Dealscan (DS)-Compustat sample, based on whether the representation election results in a "close" win or a "close" loss for the union and with "closeness" defined as a narrow range (a vote share range of $\pm 5\%$) around the majority (50%) threshold needed for the union to win representation ("Discontinuity sample"). All variable definitions are in Appendix A.

Panel A: Union (NLRB)-Dealscan (DS)-Compustat Sample				
	NLRB-DS-Compustat		Other DS-Compustat	
	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)
Union Election Results and Other Election Characteristics:				
Union Win	0.36	0	n.a.	n.a.
Union Election Size	268.5	141.0	n.a.	n.a.
Union Vote Share	0.46	0.43	n.a.	n.a.
Loan Spreads After Union Elections:				
Loan Spread, Year=+1 (bps)	180	150	223	212
Loan Spread, Year=+2 (bps)	181	150	223	212
Loan Spread, Year=+1,+2 (bps)	181	150	223	212
Loan Spreads & Firm Characteristics Before Union Elections:				
Log(Assets)	9.0	9.2	6.6	6.5
M/B	1.6	1.3	1.6	1.3
Rated Dummy	0.58	1	0.55	0
Loan Spread, Year=-1 (bps)	179	125	223	212
Panel B: Difference in Pre-Election Characteristics between All Union Wins vs. Losses				
	Union Wins		Union Losses	
	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)
Loan Spreads & Firm Characteristics Before Union Elections:				
Log(Assets)	8.9	9.2	9.0	9.3
M/B	1.5	1.3	1.6	1.4
Rated Dummy	0.57	1	0.59	1
Loan Spread, Year=-1 (bps)	184	150	176	125
Panel C: Difference in Pre-Election Characteristics between Close Union Wins vs. Losses				
	Close Union Wins		Close Union Losses	
	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)
Loan Spreads & Firm Characteristics Before Union Elections:				
Log(Assets)	9.1	9.2	9.1	9.2
M/B	1.6	1.3	1.6	1.3
Rated Dummy	0.58	1	0.58	1
Loan Spread, Year=-1 (bps)	178	125	175	125

Table 8: Union Elections and Loan Pricing: Baseline Analysis

This table presents results for tests of differences in means of loan spreads after union election events depending on whether the election outcome was a win or a loss for the union. Columns (1) to (4) refer to loan spreads one year after the election and Columns (5) to (8) are for spreads two years after the election. For each spread, Panels A reports results for the entire sample (Columns (1) and (5)) and various sub-samples that exclude elections involving, in turn, operating subsidiaries (Columns (2) and (6)), fewer than 150 employees (Columns (3) and (7)), and those involving both fewer than 150 employees and investment grade-rated firms (Columns (4) and (8)). Panel B only uses observations involving "close" elections, defined as a narrow range (a vote share range of ±5%) around the majority (50%) threshold needed for the union to win representation ("Discontinuity sample"). Panel C stratifies the sample based on the number of employees involved in each election. All variable definitions are in Appendix A. Standard deviations appear in square brackets below mean loan spreads and p-values are below the difference in mean loan spreads. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

Panel A: Entire Sample								
	Loan Spread, Year=+1				Loan Spread, Year=+2			
	(1) All	(2) Exclude Subs	(3) Exclude Small	(4) = (3)+ Exclude Inv Grade	(5) All	(6) Exclude Subs	(7) Exclude Small	(8) = (7)+ Exclude Inv Grade
Union Win	189.7	204.1	214.4	250.1	188.6	200.7	207.9	264.1
	[144.0]	[145.5]	[155.4]	[155.5]	[159.3]	[138.2]	[187.3]	[194.9]
Union Loss	168.9	163.0	161.1	191.1	153.5	156.0	149.5	198.4
	[128.8]	[121.7]	[139.1]	[150.5]	[129.9]	[129.5]	[139.1]	[153.9]
Difference	20.8***	41.1***	53.4***	59.0***	35.1***	44.7***	58.4***	65.6***
p-value	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	3,814	2,382	1,652	734	3,811	2,366	1,635	720
Panel B: Discontinuity Sample								
Union Win	236.5	244.4	265.7	295.1	226.7	252.9	213.9	259.5
	[164.9]	[186.3]	[199.9]	[204.6]	[149.5]	[149.3]	[142.0]	[133.3]
Union Loss	167.2	135.7	125.9	141.8	141.6	151.0	119.5	140.1
	[146.5]	[114.9]	[101.7]	[110.8]	[106.9]	[120.1]	[102.7]	[140.1]
Difference	69.2***	108.7***	139.7***	153.3***	85.1***	101.9***	94.4***	119.4***
p-value	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	470	276	232	102	511	301	227	100
Panel C: Loan Spread, Year=+1 by Union Election Size								
	(1) N≥100	(2) N≥150	(3) N≥200	(4) N≥250	(5) N≥300	(6) N≥350	(7) N≥400	(8) N≥450
Union Win	190.7	214.4	215.9	226.4	232.1	240.5	240.8	253.0
	[146.2]	[155.4]	[171.4]	[182.2]	[190.2]	[200.2]	[201.1]	[213.5]
Union Loss	167.9	161.1	162.2	151.5	149.5	155.4	153.7	142.1
	[138.9]	[139.1]	[138.1]	[143.6]	[147.1]	[152.4]	[159.8]	[119.4]
Difference	22.8***	53.4***	53.7***	74.9***	82.6***	85.0***	87.1***	110.9***
p-value	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2,466	1,652	1,204	913	736	601	509	447

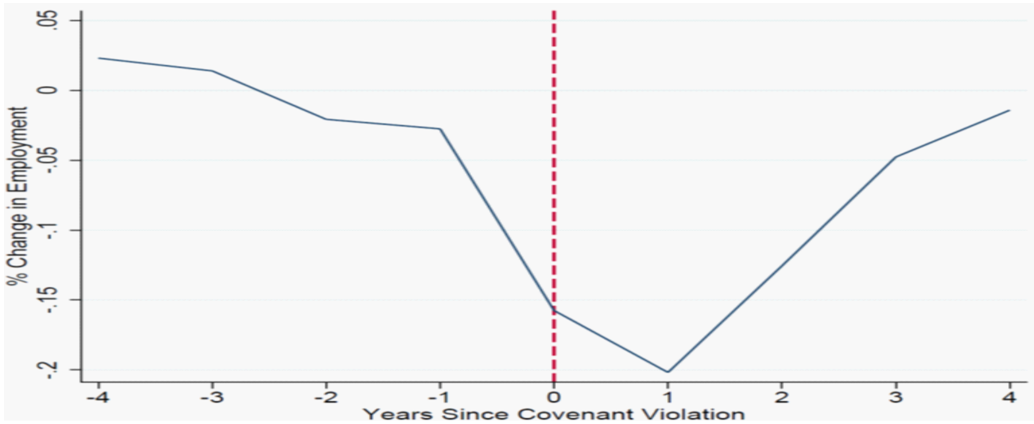
Table 9: Union Elections and Loan Pricing: Matched-Sample Analysis

This table presents results for t-tests of differences in means of average excess loan spreads in the two years after union election events depending on whether the election outcome was a win or a loss of for the union. Average excess loan spreads are defined as the difference between loan spreads and the average loan spread for a portfolio of matching loans. Columns (1) to (4) of Panel A refer to average excess loan spreads over a portfolio of loans matched based on year, industry, and firm size (deciles), while Columns (5) to (8) of Panel B are for average excess loan spreads over a portfolio of loans matched based on year, industry, and firm growth opportunities (Market to book ratio deciles). For each spread, we report results for the entire sample (Columns (1) and (5)) and various sub-samples that exclude elections involving, in turn, subsidiaries (Columns (2) and (6)), fewer than 150 employees (Columns (3) and (7)), and those involving both fewer than 150 employees and investment grade-rated firms (Columns (4) and (8)). Columns (1) to (4) of Panel C refer to average excess loan spreads over a portfolio of loans matched based on year, industry, and firm credit ratings, while Columns (5) to (8) of Panel D are for average excess loan spreads over a portfolio of loans matched based on year, industry, and loan spreads (deciles) one year before the election. For each spread, we again report results for the entire sample (Columns (1) and (5)) and various sub-samples that exclude elections involving, in turn, subsidiaries (Columns (2) and (6)), fewer than 150 employees (Columns (3) and (7)), and those involving both fewer than 150 employees and investment grade-rated firms (Columns (4) and (8)). All variable definitions are in Appendix A. Standard deviations appear in square brackets below mean loan spreads and p-values are below the difference in mean loan spreads. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1% respectively.

	A. Year, Industry, & Size Matched				B. Year, Industry, & Growth Opp. Matched			
	(1) All	(2) Exclude Subs	(3) Exclude Small	(4) = (3)+ Exclude Inv Grade	(5) All	(6) Exclude Subs	(7) Exclude Small	(8) = (7)+ Exclude Inv Grade
Union Win	24.0	23.6	38.2	139.4	-26.2	-10.1	-27.1	124.6
	[121.3]	[120.8]	[121.1]	[140.4]	[134.0]	[132.2]	[135.0]	[131.6]
Union Loss	8.7	-2.1	4.7	76.9	-42.7	-51.0	-58.9	34.1
	[111.6]	[108.2]	[115.4]	[155.3]	[121.1]	[115.7]	[126.6]	[165.1]
Difference	15.3***	25.8***	33.4***	62.5***	16.5***	41.0***	31.8***	90.5***
p-value	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Observations	3,122	1,845	1,378	606	3,124	1,844	1,376	605

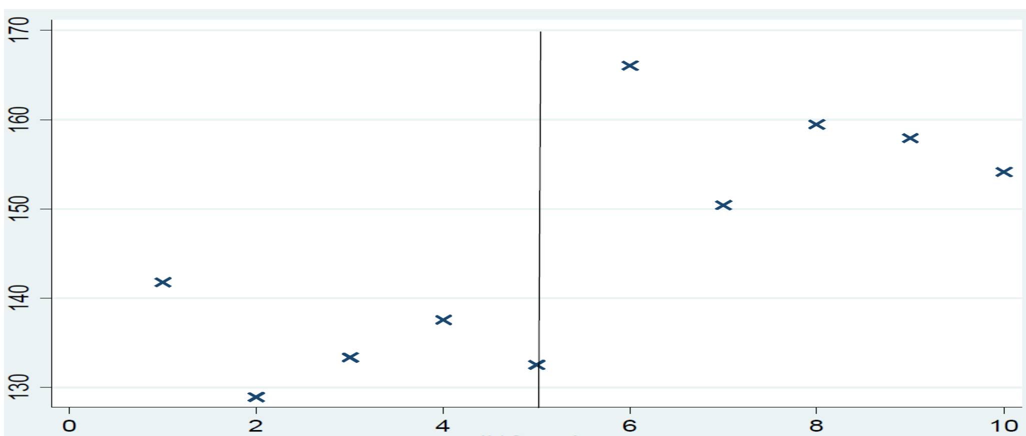
	C. Year, Industry, & Ratings Matched				D. Year, Industry, & Prior Spread Matched			
	(1) All	(2) Exclude Subs	(3) Exclude Small	(4) = (3) + Below Inv Grade	(5) All	(6) Exclude Subs	(7) Exclude Small	(8) = (7) + Below Inv Grade
Union Win	10.9	9.1	26.9	39.0	19.4	12.9	26.3	140.3
	[97.4]	[98.4]	[120.9]	[151.8]	[103.9]	[106.0]	[111.5]	[149.6]
Union Loss	-5.1	-10.7	-2.3	-22.1	2.2	-6.7	-2.9	59.1
	[93.3]	[90.4]	[95.0]	[135.9]	[92.3]	[84.4]	[96.8]	[165.3]
Difference	16.0***	19.9***	29.2***	61.1***	17.2***	19.6***	29.3***	81.2***
p-value	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Observations	3,113	1,840	1,372	604	3,106	1,829	1,376	605

Figure 1: Loan Covenant Violations and Employment



The sample consists of 11,536 firm-year observations for nonfinancial firms between 1994 and 2010 corresponding to firms that have at least one private loan found in Dealscan with a covenant that restricts current ratio or net worth to lie above a certain threshold. This figures shows average percentage annual changes in the number of employees in event time leading to and after the year when a covenant violation occurs.

Figure 2: Unionization Elections and Loan Spreads



The sample consists of 3,814 observations for nonfinancial firms between 1985 and 2010 corresponding to loans that have union representation election information in NLRB for firms in Compustat and loan pricing information in Dealscan one year after the union election event. This figures plots mean loan spreads for each of ten equally-spaced bins of the data sorted on values of the union vote share variable, with the vertical line denoting the 50% vote threshold.