Do Creditor Rights Increase Employment Risk? Evidence from Loan Covenants

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First draft: October 2013

Current revision: October 2014

PRELIMINARY AND INCOMPLETE

[1]Views expressed are those of the authors and do not represent the views of the Board or its staff. Contacts: antonio.falato@frb.gov, jnellie.liang@frb.gov. Special thanks to Mark Carey and Greg Nini for their help with Dealscan and for kindly sharing their Compustat-Dealscan key. We thank Bill Bassett, Sudheer Chava (discussant), Steve Davis (discussant), Edward Morrison (discussant), Greg Nini (discussant), Michael Roberts (discussant), Marco Pagano, Steve Sharpe, Martin Schmalz, Amir Sufi, Till von Wachter; seminar participants at the Federal Reserve Board; and conference participants at the annual meetings of the American Economic Association, American Finance Association, the Conference on Empirical Legal Studies, the UBC Winter Finance Conference, the Financial Intermediation Research Society, and the NBER Capital Markets and the Economy, for helpful comments and discussions. Brandon Nedwek, Nicholas Ryan, Richard Verlander, and especially Suzanne Chang provided excellent research assistance. All remaining errors are ours.

Abstract

Financial contracts that mitigate incentive conflicts between firms and their creditors have a large impact on employees. Using a regression discontinuity design, we provide evidence that there are sharp and substantial employment cuts following loan covenant violations, when creditors gain control rights to accelerate, restructure, or terminate a loan. The employment cuts following violations are larger at firms with higher financing frictions and agency costs and when employees have weaker bargaining power. The cuts are also much larger in industry and macroeconomic downturns, when employees have fewer alternative job opportunities. In addition, union elections that create new labor bargaining units lead to higher spreads on new loans, consistent with creditors requiring greater compensation when employees have more bargaining power. Our analysis establishes a specific link between financing frictions and employment, and offers direct evidence that binding financial contracts are an important amplification mechanism of economic downturns.

1 Introduction

The effect of financing frictions on employment is an important question but, relative to other economic outcomes such as investment, an underexplored one both in corporate finance and macroeconomics. In an attempt to make progress on this question, we examine whether violations of loan covenants lead to reductions in labor force, as firms attempt to appease creditors who can take various actions to protect the value of their claims.[1] We find evidence of substantial economic effects of loan covenant violations on employment. Our evidence has important implications for the classical academic and policy debate on the influence of corporate financing on macroeconomic and financial stability (e.g., Bernanke and Gertler (1989), and Benmelech, Bergman, and Seru (2011) for a 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.

Loan covenants protect the rights of creditors by defining a state-contingent transfer of "control rights." When a borrower violates a loan covenant, creditors gain control rights in that the violation grants rights to creditors to accelerate payments or terminate an unused revolving credit facility, or to renegotiate loans on more favorable terms to themselves. In order to avoid acceleration and ensure continued access to credit, firms may take actions to cut spending and improve net cash flows. Thus, using loan covenant violations, we can clarify which financial frictions impact employees – agency and contractual incompleteness, and assess how these frictions matter for employees – via the state contingent allocation of control rights.[2]

Using a regression discontinuity design, we document sizable job cuts following a loan covenant violation. Our sample includes 9,190 firm-year observations for 2,153 unique US firms that have information on loan covenants in Dealscan and employment and balance sheet information in

[1] 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."

[2] By examining corporate labor policies, we complement previous studies, which show that loan covenants are effective at protecting creditors, and that the gain in control rights that accompanies covenant violations has important implications for firm investment and financial policies (Chava and Roberts, 2008, Roberts and Sufi, 2009; and Nini, Smith, and Sufi, 2009, 2012).

Compustat between 1994 to 2010. Our baseline estimates indicate that covenant violations lead to a sharp drop of about 13 log points or about 10% of the workforce in a given year, a reduction which is roughly equal in magnitude to the unconditional mean of employment growth in the sample. We complement this data with a large sample of layoff announcements for 7,649 firm-year observations in Compustat, which we assemble by combining the results of keyword searches from major news sources and information on layoff announcements from Capital IQ. Our estimates indicate that the probability of a layoff increases by about 3 percent following a loan covenant violation. The estimates are robust to restricting the sample to include only observations that are "close' to the covenant threshold, where violations can plausibly be considered a "quasi-random" treatment, following Chava and Roberts (2008). The findings are also robust to using a variety of measures of employment and covenants, and collectively provide evidence that the state-contingent transfer of rights following a covenant violation has a large adverse impact on employees.

The employment impact of violations varies predictably with various measures that proxy for the relative strength of creditors versus the firm and its employees. Employment cuts subsequent to violations are as deep as 17 log points for highly-levered firms and for those with no credit rating, which have fewer alternative sources of external financing, as well as for firms in industries with relatively high domestic concentration and low import penetration, consistent with 'quiet life" type agency frictions that make managers reluctant to fire employees being higher for these firms (see Bertrand and Mullainathan (2003) and Atanassov and Kim (2009)). Employment cuts are also deeper for firms where labor has less bargaining power, based on industry-level measures of union membership and coverage. We also examine investment cuts following a loan covenant violation, and find that investment cuts also vary predictably with labor bargaining power and are bigger when labor is stronger, suggesting that firms trade-off the employment and investment margins rather than implementing across the board cuts in activity.

The impact of violations on employment also varies predictably in the time-series, with employment cuts following violations being outsized in bad times and in industry downturns, when employees have fewer alternative job opportunities.[3] For example, violations lead to employee cuts that are more than twice as large in NBER recession periods as they are in non-recession periods. 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 3 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. We also derive an in-sample estimate of the aggregate employment effect of violations, following the approach of Chodorow-Reich (2014). The total employment impact of violations is equal to 1% of the in-sample aggregate workforce and 10% of its annual growth. Since employment cuts are concentrated in exactly the times when creditors have the most bargaining power and labor has the least, the time-series evidence corroborates our interpretation that the impact of violations reflects the relative strength of creditor and labor rights. The time-series results and the estimate of the aggregate effect of violations suggest the possibility that financial covenant are an important amplification mechanism as any given deterioration in economic fundamentals has an even larger adverse impact on employment due to the decline in a firm's net worth or other financial ratios.

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 elections that took place in the US between 1985 and 2010 with pricing information from Dealscan and accounting data from Compustat which results in a sample of 3,914 loans for 1,756 unique election results. Union wins are reliably associated with higher spreads

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

on loans originated within two years of the election, and the effect of unionization is particularly large for low-rated and unrated borrowers; these firms have the most scope for state-contingent transfer of control rights to creditors because they likely have few alternative credit providers. This result is robust to a matched sample methodology and when we consider "close" elections to address potential concerns about anticipation of election outcomes and omitted variables issues. This evidence suggests that creditors demand greater ex ante compensation (higher loan spreads) when employees have more bargaining power, further corroborating our interpretation that labor rights limit the impact of creditor rights on employees.

Overall, our analysis indicates that employment risk increases when creditors get the right to accelerate or terminate loans. Thus, financial contracts that mitigate conflicts between creditors and firms have spillover effects on employees. First, we contribute to the literature on covenant violations (Chava and Roberts (2008), Roberts and Sufi (2009), and Nini, Smith, and Sufi (2009; 2012)) by examining firm labor policies. Our findings highlight the substitutability of labor and capital as adjustment margins in response to violations. We also contribute to the classical literature on the economic effects of unions (see e.g., DiNardo and Lee, 2004; Lee and Mas, 2012), which has largely abstracted from corporate control issues and financial frictions, as well as to the small but growing literature on labor and finance, which has so far focused mostly on capital structure decisions (see Matsa, 2010; Agrawal and Matsa, 2013).

Second, we contribute to the literature on the real effects of financing frictions[4] by clarifying which financial frictions impact employees – agency and contractual incompleteness, and how these frictions matter for employees – via the state contingent allocation of control rights. Until recently, this literature has traditionally focused on financing and investment (see Benmelech, Bergman, and Seru (2011) for a recent study of financing and employment). An important by

[4]Previous research has traditionally focused on the effects of finance and investment (Fazzari, Hubbard, and Petersen, 1998; Whited, 1992), while only a handful of studies has examined the relation between financing and employment (Ofek, 1993; Hanka, 1998; Kaplan, 1989; Muscarella and Vetsuypens, 1990; David, Haltiwanger, Jarmin, Lerner and Miranda, 2008).

product of our approach is that we offer direct evidence that binding financial covenants are a mechanism for the credit supply channel of Bernanke and Gertler (1989),[5] as job losses in economic downturns are exacerbated by binding financial covenants. As such, loan covenant violations operate as an amplification mechanism of an initial negative shock to the real economy, since a deterioration of firms' financial conditions has an even larger real effect in bad times.[6]

Finally, while there is evidence that bankruptcies entail costs for workers (Graham, et al, 2013),[7] our evidence shows that the real effects of finance on employment are operative well before bankruptcy occurs. This risk-shifting channel can help explain why economic downturns lead to large job losses even when they do not trigger a similarly large wave of corporate 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. But 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 cut-

[5]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 macro economics and finance.

[6]In fact, there may be additional macroequilibrium effects at play, as a negative feedback loop is created by a macro shock that leads to a deterioration in firms' net worth positions and to job cuts at many firms that could collectively lead to a further decline in aggregate sales and economic growth, which in turn could lead to further declines in the financial conditions of firms.

[7]And for firms, due to lost customers and employee relationships (Titman and Opler, 1994).

ting jobs after a covenant violation in an attempt to reassure creditors about the firm's ability to generate cash flows. 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 employment in management discussion of violations. For example, 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."

Employees may also be affected directly by creditors' interventions in the form of "advising" management to reduce headcount and operating expenses, as illustrated by the following excerpt from the first quarter 10-Q filing of Interpharm Holdings in 2008:

> 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%.

In this section, we examine this hypothesis using a regression discontinuity design approach analogous to 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 less bargaining power relative to creditors, greater agency costs, less bargaining power of employees, and in bad times when labor markets have more 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 an initial set of 11,536 firm-year observations for 2,265 unique firms. We use additional filters to insure that employment changes are not mechanically driven by other corporate decisions that have been previously examined in the literature on covenant violations, such as bankruptcy, divestitures, assets sales, and spinoffs (see, for example, Nini et al (2012)). To that end, we retrieve information on bankruptcy, M&A, spinoffs and divestitures from Capital IQ's Key Development database,[8] which consists of information gathered from over 20,000 public news sources, as well as company press releases, regulatory lings, call transcripts, investor presentations, stock exchanges, regulatory websites, and company websites.[9] We exclude firm-year observations that correspond to any of these corporate events. In addition, following Hanka (1998) we exclude observations for which the (absolute value of) the growth rate of fixed assets is larger than 100% and the number of employees is smaller than 10.[10] These additional filters lead to a final sample of 9,190 firm-year observations for 2,153 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-

[8]For the events that have coverage in Capital IQ starting in 2002, we retrive information for the pre-2002 period from SDC.

[9]CIQ analysts filter this data to eliminate duplicate and extraneous information, identify the company(ies) involved, and then categorize the data based on the type of event involved.

[10]In robustness analysis (Table 6), we consider more conservative versions of these filters.

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]

In our baseline analysis, we focus on net worth and current ratio covenants for two reasons, as elaborated by Chava and Roberts (2008) and Dichev and Skinner (2002). First, they appear

[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 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).

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. In additional analysis, we consider the full set of financial covenants for which information is available in Dealscan, which include those based on interest coverage, EBITDA, and various types of leverage. We also consider robustness of our baseline results to using a covenant violation dummy hand-collected by Nini, Smith, and Sufi (2012) from the filings of all US firms that report to the SEC.

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 a layoff announcements involving Compustat firms either 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 in the Capital IQ's Key Development database, which also tracks layoff announcements. Combining these two sources leads to a final sample of 7,649 firm-year observations in Compustat, for which we also perform additional textual analysis of the news releases to gather information on the size of the layoffs as well as the circumstances surrounding it.

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 final sample of 9,190 firm-year observations for 2,153 unique firms that are bound by either a current ratio or a net worth covenant during the period 1994 to 2010. By way

[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.

of comparison, we also report summary statistics of these variables for other nonfinancial 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, in terms of assets, 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 21%, 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, 19%.

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 as well as in employment regressions (Nickell (1984), Nickell and Wadhwani (1991)), such as firm size, profitability, and operating performance. In robustness analysis, we include an extensive set of alternative definitions of our dependent variable, as well as specifications with 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

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 3,634 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 13 log points per year (Column 2). The economic magnitude of the job cuts is substantial, at about 20% of the unconditional size of firm workforce (mean log employment) in the sample. In the discontinuity sample, violations lead to employment drops of roughly the same magnitude (Column 2, 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 (Figure 1) confirms that there is a sharp break in average employment in the year of violation ($t = 0$)

covenant thresholds.

and in the one immediately after ($t = 1$).[15]

Our estimates of the impact of violations on employment are robust to using alternative specifications. Column 3 addresses omitted variable concerns by incorporating standard control variables (firm size, cash flows, and ROA). In this full specification, covenant violations remain associated with a sharp decline in employment, which is about 13 log points 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 also strongly economically significant, and about as large as the mean employment growth in the sample.

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 about 2-3% higher likelihood of layoff in a given year (Columns 5-7), an economically significant effect which is about 1/3 of the unconditional likelihood of layoff in the sample, suggesting that voluntary separations are not driving our baseline estimates.

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

Table 3 addresses potential measurement issues with our baseline analysis. In Panel A, we address measurement issues with our dependent variable. We report results for our baseline specifications using several different employment outcomes. Columns 1 and 2 show that our baseline results are robust to using measures of employment growth scaled by either fixed or total assets, which show that employment intensity declines after violation, thus addressing the potential concern that employment cuts are simply a manifestation of across the board cut backs in activity. Column 3 shows results for symmetric employment growth, to address the potential concern that asymmetries especially between large employment increases and large cuts may be driving our baseline results. Columns 4 and 5 consider finer information from the announcement releases of the layoffs in our sample. Specifically, results for a tobit specification where the dependent variable is the number of employees that are laid off relative to the size of the firm workforce show that violations also lead to bigger layoffs. In addition, violations increases the likelihood of layoffs that are motivated by cost-cutting reasons, which offers direct evidence supporting our transfer of control rights story. Finally, Columns 6-8 consider additional employment outcomes analogous to those examined in Hanka (1998). Covenant violations lead not only to lower total payroll costs, but also affect the composition of the workforce and the structure of employee compensations, as measured by the likelihood of employing part-time employees and the number of stock options per employee, respectively.

In Panel B, we take on measurement issues with our explanatory variable. For both (log) employment (Columns 1 to 4) and layoffs likelihood (Columns 5 to 8), we consider four alternative definitions of Bind. Specifically, Columns 1 and 5 consider a definition of Bind that includes all covenants for which there is information in Dealscan, which in addition to the net worth and current ratio include those based on interest coverage, EBITDA, and various types of leverage. Columns 2 and 6 consider a covenant violation dummy hand-collected by Nini, Smith, and Sufi (2012) from the filings of all US firms that report to the SEC. Our baseline results are robust to using either of these alternative definitions, which offers a useful external validity check. Columns 3-4 and 7-8 combine the covenant information from Dealscan and from the SEC filings by defining Bind based on the cases when there is a violation based on both Dealscan and the SEC filings. The estimates remain remarkably stable, which addresses potential measurement error concerns with using imputed rather than actual violations in our baseline tests.

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 should be larger whenever the firm and its employees are in a relatively weaker bargaining position with respect to creditors. In each year of the sample period, we rank firms based on the empirical distribution of two proxies for financing frictions, book leverage and a dummy for whether the firm has a credit rating or not. High leverage and lack of a credit rating increase financial constraints risk, thus potentially exacerbating risk-shifting (Jensen and Meckling (1976)). In addition, firms with high leverage 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 also examine variation by various industry-level proxies of product market competition and employees' bargaining power.[17]In each year of the sample period, we rank firms based on the empirical distribution of these proxies, which include measures of domestic and foreign product market competition and union representation. Organized representation through unions is well-recognized to increase labor bargaining power (Clark (1984), Hirsch (2008). 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 em-

[17]Using industry-level variables reduces the potential for simultaneity.

ployment should be strongest for firms in industries with higher domestic product market concentration, as measured by a high Herfindahl index, and lower import penetration. 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)).

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 leverage and for rated vs. nonrated firms (Table 4, Columns 1 and 2). For the entire sample in Panel A and for the discontinuity sample in Panel B, and for both outcome variables (number of employees and the layoff dummy), the negative impact of violations on employment is concentrated among firms that are highly leveraged and have no credit rating. A firm with high leverage or no credit rating cuts two to three times as many employees after violating a covenant as a firm in the lower quartile of leverage or with a credit rating. We also replicate the results in Chava and Roberts (2008) for investment across the different proxies. Overall, these results suggest that the state-contingent allocation of control rights to creditors leads to deeper cuts in employment, and in investment, at firms that are in a weaker bargaining position relative to their creditors at the time of violation.

As shown in Columns 3 and 4, the employment impact of violations is greater at firms in less competitive industries; these results are robust for both samples and both outcomes variables (number of employees and the layoff dummy). In particular, employment cuts are sharper for firms where domestic competition is weakest (highest quartile of the HHI), or where import penetration is in the lowest quartile, consistent with the state-contingent allocation of control rights to creditors helping to mitigate “quiet-life” employment distortions, when managers are reluctant to fire employees.

Employment cuts also are greater for firms in the lowest quartile of union membership or coverage (Columns 5 and 6). These results provide a contrast to those for cuts in investment. Notably, the impact of violations on investment is greatest in industries with high union representation, suggesting that firms use the investment margin more when labor has more bargaining power. 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 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. 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, the estimated coefficients of -.184 in NBER recession years, and -.314 in the Great Recession years, are several multiples of the coefficients in their respective non-bad years. In line with these results, the estimated effects are generally not significant robustly across our proxies for good times. In contrast, there is much less time-series variation in the estimated coefficients for investment. For example, the investment response in NBER recession periods is -.008 (Column 2), which is about the same as the -.009 for NBER non-recession years. 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 Table 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)).

[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.

Based on this evidence as well as our results in Table 4, 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 coverage (Rows 9 and 10). The estimates indicate that 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 further supports a state-contingent transfer of control rights mechanism.

2.5.1 The aggregate effects of violations: a calibration and an in-sample estimate

We offer two complementary assessments of the aggregate employment effects of violations. First, 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 7.3% cut in employees (Column 2, Row 2 of Table 5) and the frequency of bind is 19 percent (Table 1); in recessions, violation leads to about 18% 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 $-0.050 = 0.27 \times -0.184$, which is about 3 times larger than the impact in non-recession periods, $-0.014 = 0.19 \times -0.073$. A similar calculation for investment suggests that there is also amplification, but much less than for employment. Thus, the interplay of labor and creditor rights leads to larger amplification effects of violations on employment than on investment.

Second, we follow the approach of Chodorow-Reich (2014) and derive an estimate of the total effect of financing frictions associated with loan covenant violations in the sample. The estimate is derived under the (partial equilibrium) assumption that the total employment effect equals the sum of the direct employment effects measured at the firm-level and for the counterfactual exercise where every firm has loan covenants that are not binding. Under this exercise, the counterfactual (log) employment of any given firm is given by the fitted value from our baseline regression (Table 2, Column 3) which is adjusted by adding the (absolute value of) the estimated regression coefficient on Bind. The estimate for the total employment impact of violations is constructed by summing across firms the difference between the (inverted log transformations of) the counterfactual and the fitted employment levels. For the overall sample, the implied estimate of the total employment impact of violations is equal to 1% of the in-sample aggregate workforce and 10% of the annual growth of the in-sample aggregate workforce.

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 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 out-

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

liers 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 more conservative filters on the size of the workforce and the growth in fixed assets, which are shown in Rows [5-7], remain large and are little changes relative to our baseline. Excluding the financial crisis (Row [8]) has also little impact on our main estimates. Panels C and D verify that our results are robust to controlling for several additional factors that might affect employment,[22] suggesting that omitting these variables from our baseline is not driving our results.

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 tra-

[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.

[22] In particular, we include higher oder non-linear splines of the distance from the covenant threshold (Rows [9] to [11]); investment (Row [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.

ditionally 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 discontinuity 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

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

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 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.

[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.

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 $\pm 5\%$) 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] 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.

[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.

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

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

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 labor 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

[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.

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]
Employment Growth is the ratio of the total number of employees (item 29_t) minus the lagged total number of employees (item 29_{t-1}) divided by the lagged total number of employees (item 29_{t-1}). [Compustat]
Employment Growth to PPE is the ratio of the total number of employees (item 29_t) minus the lagged total number of employees (item 29_{t-1}) divided by the lagged property, plant, and equipment (item 8_{t-1}). [Compustat]
Employment Growth to Book Assets is the ratio of the total number of employees (item 29_t) minus the lagged total number of employees (item 29_{t-1}) divided by the lagged total book assets (item 6_{t-1}). [Compustat]
Symmetric Employment Growth is the ratio of the total number of employees (item 29_t) minus the lagged total number of employees (item 29_{t-1}) divided by 0.5 times the sum of the total number of employees (item 29_t) plus the lagged total number of employees (item 29_{t-1}) (see, for example, Davis et al. (2008)). [Compustat]
Layoff is a dummy that takes value of one if there is a layoff announcement in any given firm-year. We complement information on 3,129 layoff announcements hand-collected from Wall Street Journal and other major news sources with information on about 9,000 layoff announcements from the Capital IQ's Key Development database. Combining these two sources leads to a final sample of 7,649 firm-year observations in Compustat. [Factiva & Lexis Nexis news searches, and Capital IQ's Key Development database].
Layoff Size is the ratio of the number of employees that are laid off divided by the lagged total number of employees (item 29_{t-1}) [Factiva & Lexis Nexis news searches, and Capital IQ's Key Development database; Compustat]
Cost Cutting Layoff is a dummy that takes value of one if the text of the layoff announcement contains specific language about the motivation of the layoff being related to cost-cutting and restructuring. [textual analysis of the layoff news releases from Factiva & Lexis Nexis news searches, and Capital IQ's Key Development database]
Part-Time Employees is a dummy that takes the value of one if the firm reports having part-time employees in its total workforce (footnote of item 29). [Compustat]
Total Wages is the natural logarithm of total labor expenses (item 42), deflated by CPI in 1990. [Compustat]
Stock Options per Employee is the ratio of the number of new stock options granted to employees (calculated as the total number of new stock options granted minus the total number of new options granted to top executives) to the total number of employees (item 29). [Compustat, ExecuComp]

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]
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 9,190 firm-year observations for 2,153 unqiue nonfinancial US 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 82,324 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	0.21	0	n.a.	n.a.
Excluding NBER Recession Years	0.19	0	n.a.	n.a.
Only in NBER Recession Years	0.27	0	n.a.	n.a.
Employment Outcome Variables:				
Employees (000)	6.27	2.10	6.30	.73
Employment Growth	0.08	0.03	0.09	0.03
Employment Growth to PPE	0.01	0.00	0.01	0.00
Employment Growth to Book Assets	0.001	0.00	0.001	0.00
Symmetric Employement Growth	0.04	0.03	0.04	0.03
Layoff Dummy	0.09	0	0.06	0
Layoff Size (% workforce)	0.05	0.02	0.07	0.02
Cost Cutting Layoff Dummy	0.02	0	0.02	0
Part-Time Employees Dummy	0.21	0	0.17	0
Total Wages (Log)	4.49	4.26	3.91	4.26
Stock Options per Employee	0.56	0.14	1.18	0.19
Capex	0.07	0.04	0.06	0.04
Firm Characteristics:				
Assets (Log)	5.82	5.90	4.92	4.79
Cash Flow	0.07	0.08	0.05	0.07
ROA	0.11	0.12	0.05	0.10
Net Worth	435.94	146.17	493.74	53.99
Tangible Net Worth	168.78	59.47	137.56	40.19
Current Ratio	2.14	1.82	2.47	1.86
Cash Holdings	0.09	0.05	0.20	0.10
GIM Index	9.17	9.00	9.03	9.00
Leverage	0.30	0.26	0.27	0.19
Short Term Debt	0.13	0.05	0.19	0.07
Rated Dummy	0.32	0	0.18	0
Tobin's Q	1.64	1.32	1.79	1.51
Industry Characteristics:				
Unionization (coverage)	0.10	0.07	0.08	0.05
Labor Intensity	0.01	0.01	0.01	0.01
Competition (HHI)	0.21	0.16	0.25	0.21
Competition (Import Penetration)	0.25	0.21	0.26	0.21
Firm-Year Obs	9,190		82,324	
Firms	2,153		13,828	

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 Splines	FE Controls & Splines	OLS Splines	Probit No Controls	Probit Splines	Probit Controls & Splines
Bind	-0.008***	-0.150***	-0.137***	-0.128***	-0.114***	0.023***	0.024***	0.032***
	(0.003)	(0.025)	(0.025)	(0.022)	(0.028)	(0.009)	(0.009)	(0.009)
Log(Assets)				0.316***				-0.042***
				(0.028)				(0.002)
Cash Flow				0.147				-0.071***
				(0.151)				(0.026)
ROA				0.145**				-0.008
				(0.139)				(0.041)
Default Distance (NW)			0.074**	0.047**	0.001		-0.002	-0.001
			(0.031)	(0.021)	(0.001)		(0.002)	(0.001)
Default Distance (CR)			-0.024***	-0.011	0.006		0.007	0.008
			(0.006)	(0.008)	(0.005)		(0.006)	(0.005)
Intercept	0.046***	0.933***	0.908***	-1.205***	1.116***			
	(0.004)	(0.043)	(0.044)	(0.190)	(0.034)			
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	No	No	No
Industry Fixed Effects	No	No	No	No	No	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,190	9,161	9,161	8,945	8,980	6,558	6,558	6,386
	Panel B: Discontinuity Sample							
Bind	-0.007**	-0.111***	-0.107***	-0.092***	-0.095***	0.028***	0.031***	0.032***
	(0.003)	(0.030)	(0.030)	(0.030)	(0.012)	(0.008)	(0.010)	(0.011)
Observations	3,634	3,600	3,600	3,475	3,470	3,606	3,606	3,501

Table 3: Loan Covenant Violations and Employment: Analysis of Additional Labor Outcomes and Covenant Measures

This table presents regression results of several additional labor outcomes on a covenant violation measure ("Bind") in Panel A, and of employment on alternative measures of covenant violations in Panel B. All regressions are for the entire sample and the model specification is the baseline one with full controls and splines as in Column (3) of Table 2 and with industry or firm fixed effects included depending on which dependent variable used. 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). In Panel A, we vary the dependent variable, which is the change in employment scaled by (lagged) PPE (Column (1)); the change in employment scaled by (lagged) total book assets (Column (2)); the (symmetric) change in employment scaled by the sum of current total number of employees plus (lagged) total number of employees (Column (3)); the size of the layoff measured by the ratio of the number of employees that are laid off to the (lagged) total number of employees and zero otherwise in a tobit specification (Column (4)); a dummy that takes value of one for layoff announcements motivated by cost-cutting reasons in a probit specification (Column (5)); a dummy that takes value of one for firms that employ part-time workers (Column (6)); the (log) total labor costs (Column (7)); and the number of new stock options granted per employee (Column (8)). In Panel B, we vary the definition of our main explanatory variable, the covenant violation measure ("Bind") for log employment (Columns (1) to (4)) and the layoff dummy (Columns (5) to (8)), respectively. We consider four alternative definitions of "Bind," based on: the full set of financial covenants in Dealscan, which include interest coverage, EBITDA, and various types of leverage (Columns (1) and (5)); the violation dummy hand-collected by Nini, Smith, and Sufi (2012) from the filings of all US firms that report to the SEC (Columns (2) and (6)); combining the information on the full set of financial covenants in Dealscan and the Nini, Smith, and Sufi (2012) dummy by defining Bind as the case when both sources indicate that there is a violation (Columns (3) and (7)); and combining the information on the NW and CR financial covenants in Dealscan and the Nini, Smith, and Sufi (2012) dummy by defining Bind as the case when both sources indicate that there is a violation (Columns (4) and (8)). 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: Additional Labor Outcomes								
Dependent Variable:	Emp Growth to PPE	Emp Growth to Book Assets	Symmetric Emp Growth	Tobit Layoff Size	Probit Cost Cutting Layoffs	Probit Part-Time Employees	OLS log(Labor Costs)	OLS Stock Options per Employee
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bind	-0.011***	-0.001***	-0.095***	0.021***	0.017***	0.030**	-0.120**	0.435***
	(0.003)	(0.000)	(0.008)	(0.006)	(0.007)	(0.014)	(0.056)	(0.164)
Firm Fixed Effects	No	No	No	No	No	No	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,190	9,190	9,190	6,558	6,558	9,190	1,384	2,532

Panel B: Additional Covenant Measures								
Dependent Variable:	Log(Emp)	Log(Emp)	Log(Emp)	Log(Emp)	Layoffs	Layoffs	Layoffs	Layoffs
Model Specification:	All Covenants	NSS (2012) Dummies	All Covenants & NSS (2012)	NW, CR Covs & NSS (2012)	All Covenants	NSS (2012) Dummies	All Covenants & NSS (2012)	NW, CR Covs & NSS (2012)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bind	-0.067***	-0.085***	-0.101***	-0.116***	0.018***	0.014***	0.032***	0.025***
	(0.011)	(0.012)	(0.018)	(0.031)	(0.005)	(0.003)	(0.008)	(0.011)
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	19,364	42,539	15,203	8,120	18,957	41,136	14,042	7,457

Table 4: Loan Covenant Violations and Employment: By Proxies of Financing and 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 the severity of financing (Columns (1) and (2)) and agency (Columns (3) and (4)) frictions faced by firms, as well as the degree of industry unionization (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 ($\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: leverage (Column (1)), as well as credit rating status (Column (2)); the Herfindahl-Hirschman Index (HHI) (Column (3)), and the degree of import penetration (Column (4)); union membership (Column (5)) and coverage (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.

Panel A: Entire Sample								
		Financing			Competition		Unionization	
		(1)		(2)	(3)	(4)	(5)	(6)
		Leverage		Credit Rating	HHI	Import Penetration	Membership	Coverage
Log(Employment)								
[1]	Q1	-0.074	Yes	-0.088**	-0.046	-0.185***	-0.186***	-0.169***
		(0.049)		(0.042)	(0.067)	(0.069)	(0.065)	(0.061)
[2]	Q4	-0.170***	No	-0.163***	-0.161***	-0.106**	-0.054	-0.053
		(0.046)		(0.029)	(0.038)	(0.053)	(0.036)	(0.035)
Layoffs, Probit								
[3]	Q1	0.003	Yes	0.009	0.003	0.048***	0.038***	0.036***
		(0.012)		(0.019)	(0.008)	(0.022)	(0.016)	(0.011)
[4]	Q4	0.061**	No	0.038***	0.042***	0.023	0.001	0.015
		(0.032)		(0.012)	(0.013)	(0.022)	(0.012)	(0.010)
Investment								
[5]	Q1	-0.005	Yes	-0.007	-0.001	-0.014**	-0.001	-0.003
		(0.006)		(0.005)	(0.007)	(0.006)	(0.004)	(0.004)
[6]	Q4	-0.013***	No	-0.010***	-0.011***	-0.001	-0.009**	-0.010***
		(0.006)		(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Panel B: Discontinuity Sample								
Log(Employment)								
[7]	Q1	-0.056	Yes	-0.012	-0.022	-0.192**	-0.198***	-0.181***
		(0.061)		(0.055)	(0.067)	(0.079)	(0.067)	(0.062)
[8]	Q4	-0.148***	No	-0.153***	-0.185***	-0.013	-0.047	-0.013
		(0.051)		(0.041)	(0.069)	(0.059)	(0.063)	(0.038)
Layoffs, Probit								
[9]	Q1	0.008	Yes	0.017	0.021	0.065**	0.040**	0.043***
		(0.015)		(0.029)	(0.044)	(0.032)	(0.022)	(0.013)
[10]	Q4	0.051**	No	0.035***	0.056***	0.037	0.011	0.010
		(0.025)		(0.012)	(0.018)	(0.037)	(0.028)	(0.017)
Investment								
[11]	Q1	-0.004	Yes	-0.002	-0.005	-0.010**	-0.003	-0.004
		(0.009)		(0.004)	(0.007)	(0.005)	(0.005)	(0.004)
[12]	Q4	-0.013***	No	-0.009**	-0.008**	-0.002	-0.011**	-0.011**
		(0.006)		(0.004)	(0.004)	(0.006)	(0.005)	(0.005)

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 coverage (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.

Panel A: Entire Sample							
		Bad Times			Good Times		
		(1)	(2)	(3)	(4)	(5)	(6)
		Industry Downturn	NBER Recession	The Great Recession	Industry Expansion	High Tech Boom	The Great Moderation
Log(Employment)							
[1]	Yes	-0.189***	-0.184***	-0.314***	-0.113	-0.132	-0.071
		(0.077)	(0.070)	(0.105)	(0.100)	(0.185)	(0.052)
[2]	No	-0.081***	-0.073***	-0.069***	-0.134***	-0.139***	-0.174***
		(0.024)	(0.020)	(0.019)	(0.032)	(0.065)	(0.037)
Layoffs, Probit							
[3]	Yes	0.059***	0.060**	0.098***	0.010	0.008	0.007
		(0.021)	(0.027)	(0.031)	(0.009)	(0.010)	(0.006)
[4]	No	0.014***	0.011**	0.025***	0.038***	0.037***	0.047***
		(0.003)	(0.005)	(0.004)	(0.012)	(0.011)	(0.015)
Investment							
[5]	Yes	-0.009***	-0.008**	-0.009***	-0.009	-0.009	-0.010***
		(0.003)	(0.004)	(0.003)	(0.011)	(0.006)	(0.004)
[6]	No	-0.008***	-0.009***	-0.008***	-0.010***	-0.008**	-0.007***
		(0.002)	(0.003)	(0.002)	(0.003)	(0.004)	(0.002)
Panel B: Row 1 By Firm and Industry Characteristics							
By Firm Credit Rating Status							
[7]	Rated	-0.106	-0.097	-0.097	-0.106	-0.039	-0.032
		(0.099)	(0.129)	(0.125)	(0.113)	(0.197)	(0.083)
[8]	Not Rated	-0.203***	-0.258***	-0.511***	-0.110	-0.149	-0.111
		(0.066)	(0.067)	(0.127)	(0.110)	(0.260)	(0.071)
By Industry Unionization (Union Coverage)							
[9]	Q1	-0.225***	-0.275***	-0.624***	-0.095	-0.155	-0.103
		(0.064)	(0.087)	(0.267)	(0.191)	(0.189)	(0.080)
[10]	Q4	-0.148*	-0.157	-0.172	-0.051	-0.063	-0.019
		(0.078)	(0.174)	(0.169)	(0.267)	(0.232)	(0.062)

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 (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 (±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 more conservative filters on the size of the workforce in Row [5], and on the growth in fixed assets in Rows [6-7]; 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: 2nd- and 5th-order non-linear splines of the distance from the covenant threshold in Rows [9-11]; investment in Row [12]; 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 (1)	Discontinuity (2)		Entire Sample (3)	Discontinuity (4)
<u>Panel A: Alternative Specifications</u>			<u>Panel C: Additional Controls</u>		
			Controlling for:		
[1] Median (quantile) regression	-0.144***	-0.138***	[9] 2-nd Splines	-0.127***	-0.099***
	(0.053)	(0.048)		(0.021)	(0.030)
[2] Include lagged dependent	-0.090***	-0.085***	[10] 5-th Splines	-0.123***	-0.097***
	(0.013)	(0.019)		(0.021)	(0.030)
[3] Include two lags of Bind	-0.117***	-0.105***	[11] 2-nd & 5-th	-0.130***	-0.095***
	(0.033)	(0.037)	Splines	(0.022)	(0.030)
[4] Include two leads of Bind	-0.101***	-0.102***	[12] Investment	-0.120***	-0.095***
	(0.029)	(0.041)		(0.021)	(0.030)
<u>Panel B: Alternative Samples</u>			<u>Panel D: Other Additional Controls</u>		
			Controlling for:		
[5] Exclude firms with less than	-0.133***	-0.095***	[13] Leverage	-0.128***	-0.103***
100 employees	(0.031)	(0.033)		(0.021)	(0.030)
[6] Exclude firm-years with	-0.155***	-0.096***	[14] Tobin's Q	-0.123***	-0.094***
more than 75% PPE growth	(0.030)	(0.030)		(0.022)	(0.032)
[7] Exclude firm-years with	-0.153***	-0.097***	[15] Z-Score	-0.127***	-0.088***
more than 50% PPE growth	(0.031)	(0.033)		(0.022)	(0.031)
[8] Exclude financial crisis year	-0.125***	-0.094***	[16] Accruals	-0.114***	-0.092***
	(0.022)	(0.031)		(0.022)	(0.031)

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

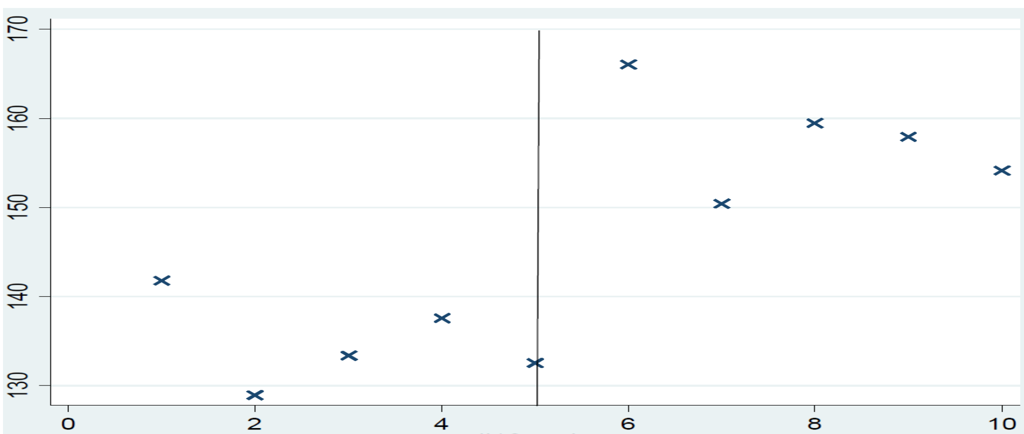
This table presents results for t-tests of differences in means of average excess loan spreads in the two years after union election events depending on whether the election outcome was a win or a loss of for the union. Average excess loan spreads are defined as the difference between loan spreads and 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.