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Does Capital Structure Affect the Behavior of Nonfinancial Stakeholders? An Empirical Investigation into Leverage and Union Strikes

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We use contract negotiation data to study how leverage affects the interaction between firms and an important nonfinancial stakeholder, labor unions. Consistent with the idea that leverage diminishes the bargaining position of labor, we find that unions are less likely to strike when a firm has high leverage or increases leverage prior to a contract negotiation. We also find large leverage increases after a strike, consistent with the idea that firms intentionally use leverage to improve their bargaining position. This poststrike increase in leverage is particularly pronounced when the union wins the strike. Moreover, we do not find any clear indication that such increases in leverage are linked to changes in investments. In addition, firms that experience a strike subsequently invest more internationally and in right-to-work states where union are afforded fewer legal protections, and they increase their disposal of production units that are located in states where strikes have occurred.

Keywords: leverage; nonfinancial stakeholders; contract negotiations; union strikes
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1. Introduction

Firms constantly interact with other economic agents who do not have a direct equity or debt stake in the firm, but who, nevertheless, have a claim to a portion of the firm's cash flow or are otherwise affected by the firm's operations. Such agents are generally referred to as "nonfinancial stakeholders" and include employees, suppliers, customers, governments, regulators, and competitors. A large theoretical literature discusses the ways a firm can use capital structure as a strategic variable to change its negotiation position or competitive dynamic relative to its nonfinancial stakeholders (for surveys, see Harris and Raviv 1991, Parsons and Titman 2008). Empirical tests of the impact of capital structure decisions on nonfinancial stakeholders, however, are comparatively rare, partially because of a lack of data.

We use contract negotiation data to study how leverage affects the interaction of firms with labor unions, an important nonfinancial stakeholder. First and foremost, we test whether the union's decision to strike is correlated with firm leverage. Because strikes are a clearly identifiable action taken against a firm and are unilaterally initiated by labor unions, this test allows us to examine the key premise of the nonfinancial stakeholder literature—that capital structure decisions affect the behavior of nonfinancial stakeholders. Second, strikes reveal information about the relative bargaining power between the firm and the union, which allows the firm to update its information set. This allows us to examine how firms adjust their leverage in reaction to a strike and ahead of the next round of contract negotiations. Taken together, these tests allow us to better understand how dynamic adjustments to capital structure policies affect the interaction between firms and an important nonfinancial stakeholder.

Previous empirical work has documented a correlation between unionization rates and leverage. Bronars and Deere (1991) find that leverage is positively correlated with industry-level unionization rates and argue that this is the result of firms strategically using leverage to improve their bargaining position relative to labor unions. Matsa (2010) uses a precise statistical identification strategy and finds that firms reduce their leverage when the states in which they are located experience legal shocks that reduce union bargaining power. However, the economic identification of the mechanisms by which leverage plays a role in bargaining with unions, and how firms dynamically adjust leverage to affect union behavior, is still not obvious. Our contribution is therefore twofold. First, we focus explicitly on contract negotiations and find evidence that is consistent with leverage deterring unions from initiating a strike, the threat of which provides a critical

element of union bargaining power (Ashenfelter and Johnson 1969). In doing so, we assume that organizing a strike represents a direct expression of a union choosing to negotiate aggressively. We note that although it is possible for aggressive unions to successfully ratify contracts without resorting to a strike, a kind of type two error, this should bias our results against finding any effect. Second, we examine how and when firms dynamically adjust leverage during the time periods surrounding contract negotiations and strikes to better understand the way firms use leverage to influence union behavior.

We find that unions are substantially less likely to strike when firms have high leverage. Moreover, firms that have recently experienced a strike increase their leverage, consistent with the idea that firms consider the effect of leverage on negotiation tactics when designing their financial policies.

We test the hypothesis that unions' bargaining tactics are correlated with firms' leverage by modeling the probability of union strikes during contract negotiations. We argue that leverage should affect the union decision to strike by decreasing the potential payoff of the strike to unions and by increasing the probability of financial distress. We also argue that leverage decreases informational asymmetries between firms and unions by credibly and verifiably constraining future cash flows, thus reducing the attractiveness of a strike as a bargaining tactic.

We find that unions are less likely to strike when their employer has either high leverage relative to other firms in the negotiation process (cross-sectional study) or increases leverage during the period prior to a contract negotiation (time-series study). Economically speaking, the effect is large. In the cross section, we find that a one-standard-deviation increase in either market or book leverage is correlated with a decrease in the probability of a union strike from 11.60% to approximately 3.60% (depending on the specification of the model). Results are similar in the time series: firms that increase leverage are less likely to experience a strike. Consistent with a casual interpretation, high leverage is associated with a lower strike probability for firms with large unions, for firms with lower growth opportunities, for firms closer to financial distress, and for firms with underfunded pension funds.

In the second part of our analysis, we examine firms' capital structure decisions before and after a contract negotiation and strike. We find that the average leverage for firms that do not experience a strike increases prior to a contract negotiation, only to fall back to its initial level two years after the negotiation. This is what we would expect if firms use leverage as a strategic variable in their negotiations with unions. By contrast, firms that eventually experience a strike appear to let their leverage drift downward before a negotiation, only to immediately engage in a large releveraging after a strike. The poststrike increase in leverage is also consistent with firms increasing leverage to improve their bargaining position in the next round of contract negotiations. In fact, poststrike leverage increases are strongest when the union wins the strike, although when firms win the strike, we do not observe any increase in leverage.

We present evidence that supports the idea that firms in our sample are, on average, increasing debt issuance to repurchase equity. These debt-for-equity swaps are more prominent right before a contract negotiation and after a strike, thus leading to leverage increasing in those periods. The financing activity is therefore consistent with the theoretical predictions of Perotti and Spier (1993) that shareholders can improve their bargaining position with senior claimants (such as labor unions) by issuing debt to fund equity repurchases, an action that decreases both future and current liquidity.

Obviously, alternative interpretations of our results are possible. With regard to leverage being negatively correlated with the probability of a union strike, an omitted variable that is associated with both financial leverage and strike activity could explain our results. With regard to our observed poststrike increases in leverage, firms may simply be drawing down lines of credit to raise cash after a costly strike. However, we provide evidence that shows that firms increase leverage in a way that does not produce cash (the increase in leverage is largely driven by an increase in equity repurchases). Another alternative is that managers may be using debt to finance mergers and acquisitions that will shift production to nonunion locales. We do not find any clear indication that increased debt issuances are linked to increased investments, but we find some interesting evidence that after a strike, firms invest more internationally and in states where union bargaining power is reduced (i.e., states with right-to-work laws). We also find evidence that, after a strike, firms tend to dispose of production units that are located in states where strikes have occurred, more often than they dispose of production units in state where labor disputes have not occurred. We therefore confirm that financing is not the only way in which firms respond to conflicts with unions.

Our study is related to three strains of literature. First, this study contributes to the literature on the effect of nonfinancial stakeholders on capital structure. With respect to this literature, our contribution is twofold: first, we show that labor unions, an important nonfinancial stakeholder, appear to respond to the strategic incentives supplied by a firm's capital structure; second, we present evidence consistent with firms responding to union behavior by adjusting their debt levels. This literature includes Titman (1984), who argues that firms whose liquidation would impose significant costs on its

employees, customers, or suppliers have lower debt ratios; Spiegel and Spulber (1994), who study how leverage affects the prices allowed by regulators; Hanka (1998), who examines how leverage affects employment and employee behavior within the firm; Myers (2012), who examines how leverage affects the costs imposed on firms by politicians; and John et al. (2015), who find that firms located in states with strong labor laws are more likely to pursue acquisitions that are not in the best interest of shareholders. This literature also includes studies of the impact of capital structure on output markets, including Phillips (1995), Subramaniam (1998), Campello (2003), and Lyandres (2006).

Second, this study is directly related to the literature that examines the relationship between capital structure and unionized labor, including Bronars and Deere (1991) and Matsa (2010). Our results are also consistent with those of Klasa et al. (2009), who argue that firms use cash holdings as a strategic variable in dealing with labor unions. This study contributes to this literature by taking an additional step toward understanding a key mechanism that drives the relationship between capital structure and unionized labor: the threat of a strike.

Finally, our study is related to the labor economics literature on union bargaining and work stoppages, including Hayes (1984), Tracy (1986, 1987), Cramton and Tracy (1992), Kennan and Wilson (1993), and Bughin (1995). With respect to this portion of the literature, we add the observation that leverage and precontract negotiation changes in leverage are correlated with a union's decision to strike.

2. Main Hypotheses

From an economic point of view, organized labor is costly to firms. Unions monopolize labor supply and can therefore attempt to extract a rent in the form of higher-than-market wages. These rents draw from cash flows that would otherwise be divided among other investment opportunities and financial stakeholders, particularly shareholders. Union bargaining power critically depends on their ability to strike. In a seminal paper, Ashenfelter and Johnson (1969, p. 35) argue that "most union 'power' is derived from the threat of the strike . . . ," and ultimately, strikes are the most direct way in which a union can impose costs on firms.

Previous work confirms the effect of union rents on shareholder wealth. For example, Abowd (1989) finds that a $1.00 increase in union rents results in a $1.00 decrease in equity value. Other studies that examine the impact of unionized labor on firm profitability and stock returns include Clark (1984), Ruback and Zimmerman (1984), and Lee and Mas (2012).

We argue that if leverage is used as a strategic variable to deter union rent seeking, then a key mechanism by which it should do so involves strike deterrence. We therefore focus our analysis on contract negotiations and strikes. We make two predictions. First, we expect leverage to deter strike activity. Second, we expect firms to increase leverage following a strike.

2.1. The Effects of Leverage on the Union's Decision to Strike

The option to strike is exercised by the union, and we wish to examine whether leverage affects this decision. Two theories of strikes are relevant. The first focuses on the importance of expected strike costs and the second focuses on informational asymmetries. In both theories, we argue that unions are less likely to strike when firms have high leverage.

2.1.1. Leverage and Strike Costs. When the expected benefit of a strike is high and the expected cost is low, unions are more likely to strike (Kennan 1980, Reder and Neumann 1980). In this theory, leverage can be understood to reduce the incentive to strike via two mechanisms. First, because the use of debt obligates a portion of present and future flows to creditors, it reduces the rents available to unions, therefore decreasing the expected benefit of a strike. This is consistent, for example, with Bronars and Deere (1991), Perotti and Spier (1993), and Matsa (2010), who show that shareholder wealth is maximized when leverage is used to reduce union rents.

Second, leverage increases the expected cost of a strike by increasing the probability of financial distress. Among other things, financial distress and bankruptcy can result in job loss, wages being revised downward, and the reduction or loss of pension benefits. As a result, if labor markets are not frictionless, as argued by Agrawal and Matsa (2013) and Berk et al. (2010), labor faces large human capital costs of bankruptcy. This is consistent, for example, with Hanka (1998) and Benmelech et al. (2012), who show that firms in or close to financial distress can obtain better concessions from labor, and with Chen et al. (2014), who show that firms with less flexibility to reduce operating costs when they are distressed (e.g., those with unionized labor) are more likely to default.

2.1.2. Leverage and Informational Asymmetries. Another theory of strike activity focuses on informational asymmetries. Hicks (1966) argues that since strikes are costly to both unions and firms, they would be avoided in the absence of informational asymmetries. The labor literature identifies at least two types of informational asymmetries that contribute to the likelihood of a strike. The first type involves informational asymmetries between the union and the firm about their respective reservation prices. In this setting, strikes arise as a rational bargaining tactic (e.g., Hayes 1984, Kennan and Wilson 1993, Cramton and Tracy 1992). During negotiations, firms may claim that they

are financially unable to meet union wage demands. In response, unions may seek to verify the truthfulness of this claim by initiating a strike with a high initial wage demand. If, in fact, the firm is able to meet union demands, the firm has incentive to settle quickly to avoid a disruption of profits; otherwise, the firm may be willing to endure a long strike to secure a reduced wage. Required debt payments credibly and verifiably constrain future cash flows (unlike other demands, such as investments or the claims of other stakeholders). We argue that this reduces the informational asymmetries between the firm and the union about the availability of future cash flows.

The second type, first proposed by Ashenfelter and Johnson (1969), involves informational asymmetries between union leaders and the rank-and-file membership about the firm's reservation price. In this setting, union leaders, when confronted with wage expectations from the rank and file that they know are far above the management's reservation price, may choose to sanction a strike rather than reach a more "realistic" agreement with management. Signing such an agreement, although actually in the rank and file's best interest, might make the leadership vulnerable to charges that they have "sold out" and ultimately jeopardize their leadership positions. As in the previous case, leverage reduces the informational asymmetries by providing credible and verifiable evidence of the firm's future cash flows, which the union leadership can present to the rank and file to temper "unrealistic" wage demands.

Within the finance literature, informational asymmetries between management and investors has been shown to result in a pecking order for financing where debt is preferred over equity (e.g., Myers and Majluf 1984). This has a different set of implications for this study—namely, if informational asymmetries between management and investors is different for firms that experience a strike and those that do not, then our results become more difficult to interpret.

2.2. The Effects of Contract Negotiations and Strikes on Firm Capital Structure Decisions

Our prediction regarding the way we expect firms to use capital structure decisions surrounding contract negotiations and strikes is straightforward. Strikes are costly to firms and, since initiated by the union, likely to occur when unions feel their bargaining position is strong. To the extent that the occurrence of a strike reveals a firm's bargaining position to be weak, and if firms view leverage as contributing to either a union's decision to strike or its outcome, we expect firms to increase their poststrike leverage to improve their bargaining position prior the next round of contract negotiations. We expect this to be particularly true when the union wins a strike, in which case the firm is unlikely to remain satisfied with its bargaining position and likely to seek to improve it. When the firm wins a strike, we expect the firm to remain satisfied with its bargaining position and be unlikely to increase leverage.

3. Data

Similar to the approach in Cramton and Tracy (1994), our analysis of strikes centers around contract negotiations because this is the setting in which strikes generally occur. Indeed, most labor contracts include no-strike clauses that restrict strike activity for the duration of the contract. As a consequence, strikes unrelated to contract negotiations are extremely rare.[1]

Several sources of data for strikes and contract negotiations are used in this study. Strike data are from two sources—the Bureau of National Affairs (BNA) and the U.S. Bureau of Labor Statistics (BLS). These databases are constructed with data from published sources, including newspapers, union publications, BNA labor publications, and government sources. Contract negotiation data are obtained from the BNA Labor PLUS database. The contract negotiation database is constructed from notices filed with the Federal Mediation and Conciliation Service (FMCS). To require an employer to renegotiate a labor contract, and to be in a position to enforce that requirement by striking, unions are required to formally notify their employer of an expiring contract within 60 days of contract expiration and a formal notice to the FMCS within 30 days after notifying their employer.[2] Most contracts are negotiated prior to this 60-day window being reached. Our sample therefore likely consists of contract negotiations that are more likely to be contentious relative to those contract negotiations that occurred without a formal filing with the FMCS. These data sources extend from January 1993 through December 2008. We matched work stoppage and contract negotiation data to firms in the merged Center for Research in Security Prices (CRSP)/COMPUSTAT data set on the basis of firm name.

We consider unions representing at least 1,000 workers in a contract negotiation or strike, and we omit data that fall below this threshold (our results are similar when we use alternative definitions of "large"). The selection of a 1,000-worker threshold is in line with previous studies (e.g., Tracy 1986, Cramton and Tracy 1992, Klasa et al. 2009). Our final sample includes 607 contract negotiations and 140 strikes involving 230 unique firms. Summary statistics for strikes and contract negotiations by industry are reported in Table 1.

[1] Exceptions to no-strike clauses typically include situations that involve "extremely unsafe" working conditions or "unfair labor practices." However, work stoppages motivated by these conditions are uncommon.

[2] See §8(d) of the National Labor Relations Act.

Table 1 Contract Negotiation and Strike Summary Statistics by Industry

Fama and French industry	No. of contract negotiations	No. of strikes	Union win	Firm win
Consumer nondurables	89	11	2	2
Consumer durables	67	24	1	5
Manufacturing	164	46	5	10
Oil, gas, and coal extraction	6	1	0	0
Chemicals and allied products	15	3	0	3
Business equipment	36	8	0	2
Telephone and television transmission	46	6	2	0
Utilities	25	6	1	2
Wholesale, retail, and some services	99	12	0	8
Healthcare, medical equipment, and drugs	10	1	0	0
Finance	1	0	0	0
Other (mines, construction, etc.)	49	22	2	4
Total	607	140	13	36

Notes. This table reports the number of strikes and contract negotiations in our sample subdivided into Fama and French industry classifications. Contract negotiation data are from the BNA Labor PLUS database. Strike data are from the BNA Labor PLUS and BLS Work Stoppages database. The sample extends from January 1993 to December 2008.

Some of our analysis makes use of estimated target leverage ratios. We used the empirical model of Byoun (2008) to estimate these ratios for each fiscal year in our sample.[3]

3.1. Determination of Strike Outcome

A portion of our analysis relies on our determining whether a union or a firm wins a strike. We made this determination by examining contemporary news reports contained in the Factiva database. We declared a win or a loss only if the winner was clear from these news reports; otherwise, we classified the winner of the strike as undetermined. Of the 140 strikes in our sample, we were able to classify 13 as resulting in a clear union victory and 36 as resulting in a clear firm victory.[4]

An example of a strike that was counted as a union win involves SBC Communications. In May 2004, 100,000 unionized workers located in 13 states and representing approximately 60% of SBC's workforce walked off the job after three months of contract negotiations failed to produce a new agreement. After a four-day strike, the firm agreed to a five-year contract that was more generous than it had previously indicated it could afford. The contract included a 2.3% raise each year in addition to cost of living adjustments, SBC agreed to cover 100% of monthly health insurance premiums plus bonuses to cover any copayments ($1,000 for active workers, $2,500 for retirees), and pensions were to be increased by 13%. The contract further guaranteed that unionized employees would not be laid off for the duration of the contract. Employees whose existing jobs were "surplused" would be given another position within the firm, existing workers would have access to jobs in emerging technologies, and several hundred employees who had been laid off earlier in the year were to be rehired. The concessions caused at least one observer to comment, "SBC blinked; this is not a good deal [for SBC]."[5]

An example of a strike counted as a firm win involves the United Auto Workers (UAW) and Peterbilt Motors Company. In May 1998, the UAW called a strike against Peterbilt two days after the previous contract had expired. At issue was base pay, health insurance, holiday pay, and retirement benefits. In July, Peterbilt hired skilled temporary replacement workers to maintain production. By early September, the union had made an unconditional offer to return to work. Peterbilt responded by locking out the union, continuing to use replacement workers, and saying the unionized workforce could return to work only when a contract was ratified. The union ratified a contract in late November, and most workers returned to work in December. News articles noted little difference between the contract that was ratified and the original contract that was offered six months earlier.

[3] We define the "target leverage ratio" as the predicted values from cross-sectional regressions performed each fiscal year on the full sample in COMPUSTAT. Dependent variables were either book leverage or market leverage. We define book leverage as the total debt (sum of debt in current liabilities and long term debt) divided by the book value of assets. Market leverage is defined as the ratio of total debt to the market value of assets, which we define as the firm's market capitalization at the end of the fiscal year plus total debt plus the liquidating value of preferred stock minus deferred taxes. Independent variables include the median industry debt ratio (based on three-digit Standard Industrial Classification (SIC) codes), estimates of the marginal tax rate (the statutory rate if the firm had positive pretax income and 0 otherwise), operating income scaled by total assets, the market-to-book ratio of assets, the log of total assets, depreciation and amortization scaled by total assets, fixed assets scaled by total assets, research and development (R&D) scaled by sales, a dummy equal to 1 if research and development data are missing and 0 otherwise, dividends scaled by total assets, and Altman's *Z*-score.

[4] As noted by Card and Olson (1995), the U.S. Bureau of Labor classified strikes as either a win or a loss to the union and the firm through the early 20th century. Most of the strikes during that time were either won or lost; only a small fraction ending in a compromise or partial success. For example, according to Peterson (1937), only 5%–15% of strikes between 1881 and 1900 ended in compromise. The BLS no longer attempts to classify strikes as wins or losses for either involved party, and modern strikes are less likely than those in the past to yield a clear victor. In this context, our ability to classify only a third of our sample as resulting in a clear union or firm win is not surprising.

[5] Peter Morisi, as quoted in Badger (2004).

4. Empirical Analysis

4.1. The Likelihood of a Union Strike—Probit Models

We test the hypothesis that the union's decision to strike is related to the firm's leverage. Examining this hypothesis in the context of contract negotiations allows us to specify two tests. First, we test whether strikes are less likely to happen in firms with high leverage relative to other firms that are negotiating a contract with a union. Taking some liberty, we refer to this test as the *cross-sectional test*. Second, we test whether strikes are less likely to happen in firms that have increased leverage in the past three years relative to other firms that are negotiating a contract with a union.[6] The three-year period roughly corresponds to the change from the previous contract negotiation; the average contract in our sample is approximately 3.5 years. All results reported here are robust to using a four-year period, and most are robust to the use of a two-year period. Again, taking some liberty, we refer to this test as the *time-series test*.

We pool all firm/year observations that correspond to a contract negotiation and estimate the parameters of probit models of union strikes.[7] For a contract negotiation during year t, the main variables of interest are the level of leverage at $t-1$ and/or the three-year change in leverage from $t-4$ to $t-1$. Book leverage and market leverage are our variables of interest. Summary statistics for all independent variables are reported in Table 2. Marginal effects are estimated at the mean and reported in Table 3 with the t-statistics of the estimated coefficients (adjusted for clustering at the firm level) reported in parentheses. Because probit models are nonlinear, the actual marginal effect of a change in one independent variable is a function of the values of the remaining independent variables. As such, marginal effect estimates such as the ones we report should be interpreted with some caution.[8] All independent variables are standardized with mean 0 and a standard deviation of 1 to facilitate the interpretation of the marginal effects.

Table 2 Summary Statistics

Variable	Mean	Median	SD	Min	Max
Book leverage	0.326	0.318	0.170	0.000	1.272
Market leverage	0.346	0.321	0.217	0.000	1.045
Δ *Book leverage*	−0.005	−0.004	0.110	−0.547	0.554
Δ *Market leverage*	−0.003	−0.001	0.161	−0.635	0.658
Unemployment	0.049	0.048	0.012	0.017	0.096
Union size	7.899	7.550	1.043	6.908	11.926
Equity volatility	0.091	0.082	0.045	0.030	0.348
Equity returns	0.088	0.087	0.252	−0.743	2.275
Δ *Employees/assets*	−0.001	−0.001	0.003	−0.023	0.008
Pension funding ratio	0.889	0.942	0.420	0.000	2.466
Profit/assets	0.140	0.134	0.067	−0.226	0.344
Cash/assets	0.047	0.025	0.059	0.000	0.381
Inventory/sales	0.108	0.098	0.072	0.000	0.843
Dividends/assets	0.016	0.012	0.018	0.000	0.109
Sales	9.055	9.225	1.511	3.141	12.242
Market-to-book	1.198	0.977	0.735	0.174	5.556
Z-score	1.963	1.814	1.104	−1.501	7.614
Δ *Profit/assets*	−0.005	−0.005	0.059	−0.292	0.259
Δ *Cash/assets*	0.001	0.000	0.053	−0.356	0.322
Δ *Inventory/sales*	−0.003	−0.002	0.046	−0.207	0.654
Δ *Dividends/assets*	0.000	0.000	0.009	−0.049	0.067
Δ *Sales*	0.196	0.156	0.326	−1.062	1.750
Δ *Market-to-book*	−0.049	−0.003	0.629	−4.653	2.752
Δ *Z-score*	−0.077	−0.020	0.525	−2.720	2.226

Notes. This table contains summary statistics (mean, median, standard deviation, minimum, and maximum) of the main variables used in Table 3. The sample extends from January 1993 through December 2008.

We report results of our analysis without control variables (panel A) and with control variables (panel B) in Table 3. Each specification includes several fixed effects. To account for macroeconomic and industry-specific conditions, we include time and industry fixed effects (based on two-digit SIC codes). To account for heterogeneity in state labor laws, such as right-to-work laws or work stoppage provisions in unemployment insurance laws, we add state fixed effects. To account for heterogeneity among unions, we include union fixed effects. Finally, we include levels of control variables to account for cross-sectional effects and changes in control variables to identify time-series effects. Levels are measured at $t-1$ relative to a contract negotiation during year t, and changes are measured from $t-4$ to $t-1$.

The inclusion of some independent variables, particularly the fixed effects in panel B, completely determines the outcome of some observations, and these are dropped from the analysis. Missing independent variables also reduce our sample size. We require that an observation be included in the analysis of both

[6] DeAngelo and DeAngelo (1991) argue that not only the *levels* of relevant financial variables that matter in labor negotiations but also the *direction* of recent changes. In particular, they find that in the domestic steel industry, firms decrease dividends, initiate white-collar and chief executive officer pay cuts, and manage earnings downward prior to contract negotiations. They argue that in labor negotiations these changes can be framed as part of a negative economic trend for the firm that calls for greater concessions from the union. Similarly, Klasa et al. (2009) find that recent changes in cash holdings, operating income, net working capital, and market-to-book ratios are statistically significant in explaining the likelihood that a union will strike.

[7] We also run our analysis using both logit and linear probability models and find that the results are qualitatively similar to the probit analysis in terms of the economic magnitudes of the marginal effect estimates as well as their statistical significance.

[8] As a check on our marginal effects estimates, we repeat the probit analysis in this paper using linear probability models which directly measures marginal effects. We find that the economic magnitude of the resulting coefficient estimates are qualitatively similar to the marginal effects we estimate using probit models.

Table 3 Leverage and the Likelihood of a Union Strike

	Book leverage			*Market leverage*		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Panel A: Independent variables include leverage and fixed effects						
Leverage	−9.346**		−7.286**	−8.669**		−6.091*
	(−2.48)		(−2.01)	(−2.36)		(−1.71)
Δ *Leverage*		−8.684**	−7.838**		−11.128**	−9.736**
		(−3.20)	(−2.78)		(−3.19)	(−2.67)
Time fixed effects	X	X	X	X	X	X
Industry fixed effects	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X
Union fixed effects	X	X	X	X	X	X
Pseudo-R^2	0.28	0.29	0.30	0.28	0.30	0.31
Observations	215	215	215	215	215	215
Panel B: Independent variables include leverage, control variables, and fixed effects						
Leverage	−11.392**		−12.076**	−14.860**		−14.943**
	(−3.54)		(−3.63)	(−4.01)		(−3.70)
Δ *Leverage*		−4.544**	0.144		−11.957**	−13.425**
		(−1.98)	(0.08)		(−2.52)	(−2.50)
Unemployment	3.081	0.669	4.793*	4.375	0.324	4.500
	(0.79)	(0.20)	(1.67)	(1.06)	(0.09)	(1.60)
Union size	8.898**	10.807**	5.427**	9.049**	10.776**	6.358**
	(4.67)	(5.07)	(4.07)	(4.75)	(5.16)	(3.72)
Equity volatility	−8.606**	−17.308**	−9.408**	−4.782	−17.632**	−7.333**
	(−2.16)	(−2.94)	(−3.65)	(−1.27)	(−2.81)	(−2.02)
Equity returns	5.016	−1.019	−3.462	1.923	−12.739	−19.187**
	(1.49)	(−0.19)	(−1.00)	(0.56)	(−1.44)	(−2.75)
Δ *Employees/assets*	0.635	−0.928	−3.296	1.468	−1.401	−3.825
	(0.32)	(−0.27)	(−1.21)	(0.69)	(−0.39)	(−1.07)
Pension funding ratio	2.336	5.143	5.792	2.716	5.253	9.837*
	(0.46)	(1.12)	(1.52)	(0.48)	(1.14)	(1.66)
Profit/assets	0.602		−8.838*	1.524		−3.833
	(0.10)		(−1.67)	(0.26)		(−0.62)
Cash/assets	8.488**		5.457**	8.733**		4.146**
	(3.68)		(2.37)	(3.93)		(2.14)
Inventory/sales	−8.063**		−7.877**	−7.759**		−7.971**
	(−2.04)		(−2.65)	(−2.15)		(−2.05)
Dividend/assets	11.263		6.455	10.341		−2.100
	(1.06)		(0.89)	(0.94)		(−0.26)
Sales	7.483*		10.250**	9.204**		12.623**
	(1.69)		(2.79)	(2.04)		(2.26)
Market-to-book	−4.136		5.003	−10.666**		−3.827
	(−0.87)		(1.22)	(−2.00)		(−0.88)
Z-score	−11.649		−5.942	−14.531*		−13.207*
	(−1.36)		(−0.89)	(−1.68)		(−1.65)
Δ *Profit/assets*		0.490	3.514		−0.444	3.349
		(0.11)	(1.09)		(−0.09)	(0.93)
Δ *Cash/assets*		1.0993	−0.151		2.270	0.856
		(0.92)	(−0.09)		(1.03)	(0.59)
Δ *Inventory/sales*		−3.531	−0.280		−3.206	2.387
		(−1.49)	(−0.18)		(−1.30)	(1.20)
Δ *Dividend/assets*		6.320	−0.214		5.800	8.017*
		(0.86)	(−0.03)		(0.79)	(1.66)
Δ *Sales*		−8.887**	−5.032**		−7.681**	−3.976**
		(−3.30)	(−3.76)		(−2.84)	(−3.27)
Δ *Market-to-book*		6.174	8.246**		6.172	9.048**
		(1.17)	(2.09)		(1.10)	(2.04)
Δ *Z-score*		−2.313	3.164		−3.302	1.334
		(−0.48)	(0.77)		(−0.64)	(0.27)

Table 3 (Continued)

	Book leverage			*Market leverage*		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Panel B: Independent variables include leverage, control variables, and fixed effects (continued)						
Time fixed effects	X	X	X	X	X	X
Industry fixed effects	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X
Union fixed effects	X	X	X	X	X	X
Pseudo-R^2	0.54	0.52	0.60	0.55	0.53	0.61
Observations	215	215	215	215	215	215

Notes. This table presents estimation results of probit models of union strikes during a contract negotiation year t. We report results of our analysis without control variables (panel A) and with control variables (panel B). All specifications include time, industry, state, and union fixed effects. Levels of independent variables are measured at the end of fiscal year $t-1$. The three-year change in the independent variable is measured as the difference between the value at the end of fiscal year $t-1$ and $t-4$. Our sample is composed of 607 contract negotiations and 140 strikes involving at least 1,000 workers. The sample extends from January 1993 to December 2008. Independent variables have been standardized with mean 0 and a standard deviation of 1. Estimated marginal effects are reported as percentages. Standard errors are adjusted for clustering at the firm level; t-statistics of the estimated coefficients are in parentheses.

* and ** denote significance levels of 10% and 5%, respectively.

panels to be retained in either. The inclusion of fixed effects and other independent variables reduces the number of observations used in this analysis from 607 to 215. Previous drafts of this paper, and similar to Tracy (1986), excluded fixed effects whose inclusion would completely determine the outcome of the regression, allowing us to retain the observations that would have otherwise been dropped. We have also estimated the parameters of a linear probability model that allowed us to retain all of the observations in the sample that would have otherwise been lost as a result of the inclusion of fixed effects. Results from these alternative specifications are similar to those reported in Table 3.

We start with considering the regression specifications that include leverage without control variables and that are reported in panel A of Table 3. The marginal effect of a one-standard-deviation increase in the level of leverage is correlated with a decrease in the probability of a strike by 9.34 percentage points in the case of book leverage, and 8.67 percentage points in the case of market leverage (Models 1 and 4). The unconditional probability of a strike is 11.60%, and thus a one-standard-deviation increase in book and market leverage reduces this probability to 2.26% and 2.94%, respectively. This correlation is economically significant. Similarly, a one-standard-deviation increase in the three-year change in leverage is related to a decrease in the likelihood of a strike of 8.68 and 11.13 percentage points for book and market leverage, respectively (Models 2 and 5). The correlations of the level and change in leverage with a probability of a strike remain statistically and economically significant when both are included in the same specification (Models 3 and 6).

Control variables are added to all the specifications reported in panel B of Table 3. The estimated marginal effects on leverage remain statistically and economically significant. Within the multivariate specifications, the marginal effect of a one-standard-deviation increase in the level of leverage is associated with a decrease in the probability of a strike by 11.39 and 14.86 percentage points for book and market leverage, respectively (Models 1 and 4). The marginal effects of a one-standard-deviation increase in the three-year change in leverage is associated with a decrease in the strike probability of 4.54 and 11.95 percentage points for book and market leverage (Models 2 and 5). With the exception of the change in book leverage, both effects remain economically and statistically significant when included in the same specification (Models 3 and 6).

We discuss the impact of control variables next. Previous work has focused on the ability of a union to maintain picket lines in a poor labor market. Thus, to control for labor market conditions, we include the unemployment rate by state as measured by the BLS (termed *Unemployment* in Tables 2 and 3). We find that the effects of unemployment are mostly insignificant in our multivariate framework. We also include the natural logarithm of the size of the bargaining unit involved in the contract negotiation as a proxy for union bargaining power and to account for any union size effects (*Union size*). Union size is statistically significant in all specifications, with marginal effects estimated to be as high as 15.10 percentage points.

To control for any stock price effects, as in Tracy (1986), we include the annual equity volatility and average equity returns from $t-4$ to $t-1$ relative to contract year t (*Equity volatility* and *Equity returns*). Table 3 shows that equity volatility is significantly associated with the likelihood of a strike conditional on other control variables, with marginal effects estimated to be between −4.72 and −17.63 percentage points.

A review of contemporary news articles for each of our strikes revealed that concerns over layoffs and pension funding featured prominently in many of the strikes. We thus include the change in a firm's employees scaled by the book value of assets (Δ *Employment/assets*), and following Benmelech et al. (2012), we

include the pension funding ratio, which is the value of a firm's pension assets divided by its estimated liabilities (*Pension funding ratio*). We find that in most of our specifications, neither of these variables is conditionally correlated with the probability of a strike.

DeAngelo and DeAngelo (1991) find that firms manage earnings downward prior to contract negotiations and that firms cut dividends to persuade the union that the firm is in poor financial health. Therefore, we include operating income scaled by the book value of assets (*Profits/assets*) and dividends scaled by the book value of assets (*Dividends/assets*). Overall, neither of these variables appears to be statistically relevant in our multivariate specifications.

Motivated by Klasa et al. (2009), we also include a firm's cash holdings scaled by book value of assets (*Cash/assets*). We find that levels of cash holdings are positively correlated with the likelihood of a strike conditional on the other covariates, with marginal effect estimates ranging from 4.14 to 8.73 percentage points. Results for changes in cash holdings are not statistically significant in our models.

Tracy (1987) argues that firms can accumulate inventory to discourage a strike by reducing the strike costs to firms. The levels of inventories scaled by sales (*Inventory/sales*) are significant and negatively correlated with strike activity in our multivariate models, with marginal effects between −7.75 to −8.06 percentage points. We also include the natural logarithm of sales (*Sales*) as a proxy for firm size. We find that large firms in our sample and firms with negative growth in sales are more likely to experience a strike, conditional on the other control variables.

Because union rents accrue primarily to shareholders, similar to Klasa et al. (2009), we include a firm's market-to-book ratio of equity (*Market-to-book*). We find that the market-to-book ratio is negatively associated with the likelihood of a strike in our multivariate models, though the effects are not always statistically significant. Finally, we include Altman's *Z*-score (*Z-score*) as a proxy for the probability of financial distress. The *Z*-score is only marginally significantly correlated with strike activity conditional on other control variables when market leverage is included in the regressions.

4.1.1. Probit Models with Interaction Effects. The results presented in the previous section indicate a strong negative correlation between strike activity and leverage. In this section we further characterize the negative relationship by considering how it differs across firms that might, ex ante, have incentives to use debt strategically when dealing with unions. The findings reported in this section support (but do not prove) a causal relationship between leverage and strike activity.

Consistent with the theoretical setting of Matsa (2010), we expect debt to deter primarily large unions, whose wage demands would place a greater strain on firm cash flows than the demands of small unions. Consistent with Perotti and Spier (1993), we expect debt to be more effective at discouraging strike activity at firms with low market-to-book ratios, which have depressed stock prices and are expected to have poor profitability and low growth rates. Because financial distress and bankruptcy are bad economic states for labor, and consistent with Bronars and Deere (1991), we expect firms that are in poor financial condition to be better able to use debt effectively to resist union demands. Finally, consistent with the empirical finding of Benmelech et al. (2012), we expect debt to be more effective at discouraging strike activity at firms whose pension plans are not fully funded.

We test these hypotheses by including in our strike probability models an interaction between leverage and union size, market-to-book ratio, a financial distress indicator, and the pension funding ratio, and report the results in Table 4. The financial distress indicator variable (termed *Financial distress*) is set equal to 1 when the interest coverage ratio is below unity. When interacting with the level of leverage we use the probit model specifications of Model 1 (for book leverage) and Model 4 (for market leverage) from panel B of Table 3. For interaction with the change in leverage, we use the probit model specifications of Model 2 (for book leverage) and Model 5 (for market leverage) from the same table. Marginal effects are reported in Table 4. As in Table 3, all right-hand-side variables are standardized to have zero mean and standard deviation equal to 1 to aid in interpreting the results.

Within the multivariate models in Table 4, we find that the estimated marginal effects on the interaction between union size and either the level of leverage or change in leverage are negative and significant (for both book and market leverage). This finding is consistent with the hypothesis that leverage is more effective at deterring the strike activity of large unions than of small unions. In addition, the estimated marginal effects on the interaction between leverage and market-to-book are positive and significant, with the exception of the level of market leverage. Thus, leverage more effectively deters strike activity at firms with low growth options and hence low potential union rents. Moreover, the estimated marginal effects on the interaction between leverage and financial distress are negative and statistically significant, suggesting that leverage discourages unions more effectively when the possibility of bankruptcy is immediate. Finally, the estimated marginal effects on the interaction between leverage and the pension plan funding ratio are positive and statistically significant for changes in leverage (but not levels), consistent with leverage being more effective at deterring union strike activity when worker pension plans are not fully funded.

Table 4 Leverage and the Likelihood of a Union Strike with Interaction Terms

	Model									
	1	2	3	4	5	6	7	8	9	10
					Panel A: Book leverage					
Leverage	−9.800**	−19.409**	−8.187**	−10.141**	1.695					
	(−2.64)	(−3.01)	(−3.09)	(−3.56)	(0.21)					
Δ *Leverage*						−6.287**	−14.581**	−3.816	−21.167**	−24.943**
						(−2.04)	(−2.99)	(−1.46)	(−2.41)	(−2.52)
Leverage × *Union size*	−4.393**				−2.303**					
	(−2.33)				(−2.19)					
Leverage × *Market-to-book*		22.283**			5.083					
		(2.53)			(0.67)					
Leverage × *Financial distress*			−8.329**		−8.751**					
			(−2.61)		(−2.83)					
Leverage × *Pension funding ratio*				−2.423	−10.607					
				(−0.77)	(−1.30)					
Δ *Leverage* × *Union size*						−10.696**				−5.722**
						(−2.97)				(−3.11)
Δ *Leverage* × *Market-to-book*							13.248**			10.903**
							(2.59)			(2.03)
Δ *Leverage* × *Financial distress*								−14.213**		2.378
								(−2.70)		(0.48)
Δ *Leverage* × *Pension funding ratio*									13.859*	10.863*
									(1.87)	(1.64)
Union size	7.768**				4.488**	9.365**				10.155**
	(3.14)				(3.65)	(4.35)				(5.38)
Market-to-book		−25.478**			−12.219*		−8.856**			−6.025**
		(−3.29)			(−1.93)		(−2.14)			(−1.99)
Financial distress			−4.817**		−5.612**			−2.295		12.024
			(−2.13)		(−2.38)			(−0.17)		(1.47)
Pension funding ratio				0.903	9.654*				5.242	3.368
				(0.20)	(1.64)				(1.24)	(0.90)
Pseudo-R^2	0.54	0.54	0.56	0.56	0.57	0.52	0.53	0.51	0.52	0.55
Observations	215	215	215	215	215	215	215	215	215	215
					Panel B: Market leverage					
Leverage	−8.400**	−19.018**	−13.268**	−14.742**	8.603					
	(−2.06)	(−2.34)	(−3.98)	(−3.98)	(−1.03)					
Δ *Leverage*						−11.859**	−15.297**	−11.644**	−29.884**	−33.976**
						(−2.16)	(−2.34)	(−2.27)	(−3.29)	(−3.32)
Leverage × *Union size*	−6.198**				−4.859**					
	(−4.08)				(−3.71)					
Leverage × *Market-to-book*		1.624			1.778					
		(0.23)			(0.25)					
Leverage × *Financial distress*			−4.479**		−3.844**					
			(−3.36)		(−2.54)					
Leverage × *Pension funding ratio*				−1.279	−4.930					
				(−0.32)	(−1.14)					
Δ Leverage × *Union size*						−8.062**				−6.643**
						(−2.47)				(−3.00)
Δ *Leverage* × *Market-to-book*							7.579**			2.560
							(1.97)			(0.88)
Δ *Leverage* × *Financial distress*								−41.777**		−21.973**
								(−3.14)		(−1.99)
Δ *Leverage* × *Pension funding ratio*									15.251**	13.599**
									(2.34)	(2.61)
Union size	9.752**				8.534**	9.057**				13.726**
	(5.04)				(4.89)	(3.10)				(4.73)
Market-to-book		−15.523**			−11.601**		−10.608**			−3.198
		(−3.02)			(−2.03)		(−2.34)			(−1.28)

Table 4 (Continued)

	Model									
	1	2	3	4	5	6	7	8	9	10
					Panel B: Market leverage (continued)					
Financial distress			−6.630**		−6.083**			−9.764		29.287**
			(3.40)		(3.03)			(0.70)		(2.49)
Pension funding ratio				1.953	1.090				8.021	9.372**
				(0.02)	(0.33)				(1.57)	(1.99)
Pseudo-R^2	0.56	0.55	0.55	0.55	0.59	0.51	0.50	0.51	0.52	0.53
Observations	215	215	215	215	215	215	215	215	215	215
					All panels					
Control variables	X	X	X	X	X	X	X	X	X	X
Time fixed effects	X	X	X	X	X	X	X	X	X	X
Industry fixed effects	X	X	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X	X	X
Union fixed effects	X	X	X	X	X	X	X	X	X	X

Notes. This table presents estimation results of probit models of union strikes during a contract negotiation year t. Results for book leverage are reported in panel A. Results for market leverage are reported in panel B. The independent variables of interest are leverage and the interaction effect with union size, the market-to-book ratio, a financial distress indicator equal to 1 when the firm's interest coverage ratio is below 1, and the pension funding ratio. Each specification includes the control variables used in Table 3 and time, industry, state, and union fixed effects. Levels of independent variables are measured at the end of fiscal year $t-1$. The three-year change in independent variables is measured as the difference between the value at the end of fiscal years $t-1$ and $t-4$. Our sample is composed of 607 contract negotiations and 140 strikes involving at least 1,000 workers. Contract negotiation data are from the BNA Labor PLUS database. Strike data are from the BNA Labor PLUS and BLS Work Stoppage databases. Accounting and stock market data are from the COMPUSTAT and CRSP databases, respectively. The sample extends from January 1993 to December 2008. Independent variables have been standardized with mean 0 and a standard deviation of 1. Estimated marginal effects are reported as percentages. Standard errors are adjusted for clustering at the firm level; t-statistics of the estimated coefficients are in parentheses.

* and ** denote significance levels of 10% and 5%, respectively.

In Models 5 and 10 we include all interaction terms within the same specification. We find that most of the interaction terms remain statistically significant when included with the others. This is consistent with their measuring distinct interaction effects.

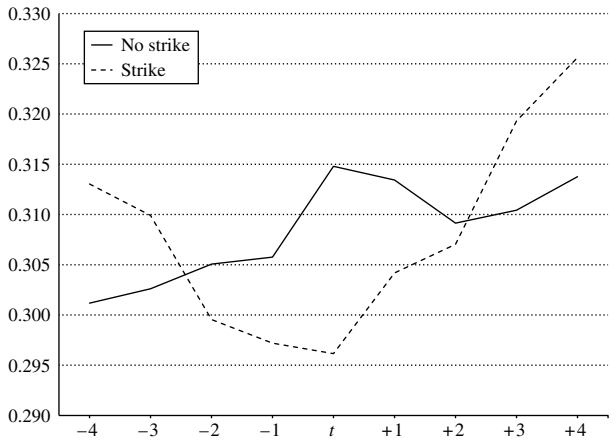
5. Firm Leverage Surrounding Contract Negotiation and Strikes

We examine the changes in leverage surrounding contract negotiations and strikes to better understand the use of leverage as a strategic variable. As a starting point, we plot the average book leverage for the firms in our sample over the years surrounding a contract negotiation in Figure 1, and we divide the sample into firms that experience a strike at that time and those that do not.

A pattern in leverage dynamics is immediately apparent: entering a contract negotiation year, the average leverage for firms that do not experience a strike increases prior to a contract negotiation, only to fall back to its initial level two years after the negotiation. This is what we would expect if firms use leverage as a strategic variable in their negotiations with unions. By contrast, firms that eventually experience a strike appear to let their leverage drift downward before a negotiation, only to immediately engage in a large releveraging after a strike. The poststrike increase in leverage is also consistent with firms increasing leverage to improve their bargaining position in the next round of contract negotiations.

In this section, we examine whether the pattern observed in the figure is consistent with leverage being used as a strategic variable with respect to unionized labor and discuss a number of alternative explanations. First, we investigate whether the changes in leverage are statistically significant. We then study the sources and uses of cash flow to examine how firm's policies (financing, investment, and operations)

Figure 1 Change in Book Leverage



Notes. This figure contains the average book leverage for firms that experience a strike or no strike incident to a contract negotiation in year t.

are related to changes in leverage around contract negotiations. We then provide some discussion about how the results presented in the paper relate to some alternative explanations. Finally, we use variation in the strike outcomes to corroborate our results.

5.1. Changes in Leverage Around Contract Negotiations

Are the changes in leverage shown in Figure 1 significant? To examine whether or not this is the case, we examine the average change in leverage between time $t+n$ and t (where $n=-4,-3,\ldots,+3,+4$) and report the results in Table 5. In panel A we show results for firms that do not experience a strike and in panel B we show results for firms that do. We examine both book and market leverage.

For firms that do not experience a strike, both book and market leverage are significantly lower in the years preceding the contract negotiation. For example, the book leverage three years before the negotiation is 1.2 percentage points lower than the book leverage at negotiation. After the contract negotiation, we observe changes in leverage reflecting those pictured in Figure 1, none of which are statistically significant. By contrast, firms that experience a strike (panel B) see an economically significant decrease in leverage prior to the contract and a subsequent increase afterward. Notably, the poststrike increase in leverage is more pronounced and statistically significant than the prestrike decrease (the one exception involves market leverage; between years $t+4$ and t, market leverage decreases by a statistically significant 0.042).

Table 5 Changes in Leverage Surrounding Contract Negotiations

	$t-4$	$t-3$	$t-2$	$t-1$	$t+1$	$t+2$	$t+3$	$t+4$
				Panel A: No-strike firms				
Δ *Book leverage*	−0.014**	−0.012**	−0.010**	−0.009**	−0.001	−0.006	−0.004	−0.001
	(−2.39)	(−2.40)	(−2.27)	(−2.79)	(−0.43)	(−1.24)	(−0.75)	(−0.16)
Δ (*Book leverage − Industry median leverage*)	−0.014**	−0.013**	−0.010**	−0.010**	−0.009*	−0.009	−0.006	0.000
	(−2.50)	(−2.44)	(−1.99)	(−2.33)	(−1.76)	(−1.46)	(−0.78)	(0.02)
Δ (*Book leverage − Target leverage*)	−0.014**	−0.013**	−0.010**	−0.010**	−0.009*	−0.009	−0.006	0.000
	(−2.50)	(−2.44)	(−1.99)	(−2.33)	(−1.76)	(−1.46)	(−0.78)	(0.02)
Δ *Market leverage*	−0.017*	−0.015*	−0.015**	−0.014**	0.005	0.008	0.008	0.017
	(−1.90)	(−1.93)	(−2.11)	(−2.77)	(0.93)	(0.99)	(0.80)	(1.57)
Δ (*Market leverage − Industry median leverage*)	−0.021**	−0.019**	−0.016**	−0.019**	−0.012	−0.003	0.002	0.018
	(−2.51)	(−2.37)	(−2.04)	(−2.76)	(−1.38)	(−0.25)	(0.21)	(1.53)
Δ (*Market leverage − Target leverage*)	−0.021**	−0.019**	−0.016**	−0.019**	−0.012	−0.003	0.002	0.018
	(−2.51)	(−2.37)	(−2.04)	(−2.76)	(−1.38)	(−0.25)	(0.21)	(1.53)
Observations	338	403	409	418	378	339	303	270
				Panel B: Strike firms				
Δ *Book leverage*	0.017	0.014	0.003	0.001	0.008*	0.011	0.023**	0.030**
	(1.58)	(1.50)	(0.49)	(0.22)	(1.83)	(1.48)	(2.32)	(2.24)
Δ (*Book leverage − Industry median leverage*)	0.003	0.001	−0.003	0.000	0.005	0.009	0.021*	0.032**
	(0.26)	(0.14)	(−0.40)	(0.08)	(1.00)	(1.05)	(1.95)	(2.28)
Δ (*Book leverage − Target leverage*)	0.005	0.005	−0.004	0.002	0.003	0.003	0.014	0.021
	(0.52)	(0.55)	(−0.60)	(0.37)	(0.65)	(0.35)	(1.52)	(1.63)
Δ (*Book leverage − Leverage of no-strike firms*)	0.031**	0.026**	0.013	0.010*	0.009*	0.017*	0.028**	0.031**
	(2.51)	(2.48)	(1.61)	(1.75)	(1.73)	(1.91)	(2.38)	(2.08)
Δ *Market leverage*	0.042**	0.023	0.002	0.000	0.015*	0.023*	0.036**	0.049**
	(2.27)	(1.37)	(0.19)	(0.03)	(1.85)	(1.75)	(2.05)	(2.40)
Δ (*Market leverage − Industry median leverage*)	0.000	−0.009	−0.009	−0.002	0.006	0.011	0.017	0.045**
	(0.02)	(−0.58)	(−0.83)	(−0.23)	(0.65)	(0.83)	(0.90)	(2.30)
Δ (*Market leverage − Target leverage*)	−0.006	−0.013	−0.018*	−0.004	0.007	0.009	0.020	0.042**
	(−0.44)	(−0.98)	(−1.75)	(−0.48)	(0.83)	(0.74)	(1.28)	(2.38)
Δ (*Market leverage − Leverage of no-strike firms*)	0.059**	0.038**	0.018	0.015	0.010	0.015	0.028	0.032
	(2.87)	(2.07)	(1.19)	(1.39)	(0.99)	(1.00)	(1.41)	(1.40)
Observations	116	119	123	127	120	107	98	84

Notes. This table contains the average changes in leverage ratios between year $t+n$ (with $n=-4,-3,\ldots,3,4$) and the year t of a contract negotiation. Panel A presents results for firms that do not experience a strike at time t. We include statistics for book leverage and market leverage and difference-in-difference estimates between changes in leverage and the corresponding change in three-digit SIC industry median leverage as well as target leverage ratios. In panel B we report results for firms that experience a strike at time t and add an additional difference-in-difference estimate between firms that experience a strike and those that did not. Contract negotiation data are from the BNA Labor PLUS database. Strike data are from the BNA Labor PLUS and BLS Work Stoppage databases. Accounting data are from the COMPUSTAT database. The sample extends from January 1993 to December 2008. t-Statistics are in parentheses.

* and ** denote significance levels of 10% and 5%, respectively.

To control for industry effects, such as firms mimicking the financing decisions of their peers (see Leary and Roberts 2005), we also report the average difference-in-difference between the change in a firms leverage relative to time t and the change in the median three-digit SIC industry leverage over the same period. The results are qualitatively similar; firms that do not experience a strike have lower leverage prior to a contract negotiation and leverage remains unchanged thereafter. Firms that do experience a strike do not significantly change their leverage before the strike but do increase it thereafter (changes are significant by year $t+3$ for book leverage and $t+4$ for market leverage).

To control for changes in a firm's estimated target leverage (the construction of which is discussed in §3), we also report the average difference-in-difference between the firm's change in leverage relative to time t and the change in the firm's estimated target leverage ratio. As before, the change in leverage for no-strike firms is significant before a contract negotiation and generally not significant after. For firms that experience a strike, only market leverage appears to increase significantly after a strike.

Finally, in panel B we report the average difference-in-difference between firms that experience a strike at time t and those that do not. We find that, relative to no-strike firms, firms that experience a strike have significantly decreasing leverage prior to a contract negotiation and, in the case of book leverage, significantly increasing leverage thereafter.

5.2. Firm Policies and Leverage Changes Surrounding Contract Negotiations

The dynamics of leverage presented in Figure 1 raise several questions. How do firms increase their leverage? Moreover, if the financing activity that is responsible for the leverage change constitutes a net source of cash, then how is this cash used?

We attempt to answer those questions by analyzing changes in the balance sheet and cash flow statements of the firm. In particular, we decompose the change in total assets into the change of all its components and scale everything by the total assets as of the beginning of the year:

$$\begin{aligned}\Delta\ \textit{Total assets} = {} & \Delta\ \textit{Current assets} + \Delta\ \textit{Physical assets} \\ & + \Delta\ \textit{Intangible assets} \\ & + \Delta\ \textit{Other assets} \\ = {} & \Delta\ \textit{Debt} + \Delta\ \textit{Equity} \\ & + \Delta\ \textit{Other liabilities}.\end{aligned}$$

We also examine the change in cash from the statement of cash flows, scaling each variable by total assets as of the beginning of the year:

$$\begin{aligned}\textit{Change in cash} = {} & \textit{Cash from operating activities} \\ & + \textit{Cash from investment activities} \\ & + \textit{Cash flow from financing} \\ & + \textit{Exchange rate effect}.\end{aligned}$$

We also further disaggregate the cash flow equation and separate cash from investments into *Capex and related* (sale of property minus capital expenditures) and *M&A and related* (sale of investments plus change in short-term investments plus other investing costs minus acquisitions minus increase in investments). Capex and related investment involves the purchase and sale of physical assets, whereas M&A and related includes merger and acquisition (M&A) activity and financial investments. We also separate the cash flow from financing into net debt issuance (long-term debt issuance minus long-term debt reduction plus the changes in current debt), net equity issuance (the sale of common and preferred stock minus the purchase of common and preferred stock), dividends, and other financing costs.

We examine the yearly cross-sectional averages of the components of the balance sheet and the cash flow equation for the four-year period before and after a contract negotiation, and for both strike and no-strike firms. This analysis is mainly illustrative of how leverage changes, on average, in particular years and the relevant tables are presented in the Internet Appendix (available as supplemental material at http://dx.doi.org/10.1287/mnsc.2015.2267). We find that years in which there is a large average increase in leverage there are corresponding increases in the change in debt and/or decreases in the change of equity (that is, when leverage increases, debt increases accelerate and equity increases decline relative to the previous period). This behavior is consistent with firms in our sample engaging in debt-for-equity swaps on average. This is supported by an examination of the corresponding statement of cash flows: net debt issuances increase during periods where equity repurchases also increase. We observe this behavior both before and after a contract negotiation and for both strike and no-strike firms. However, we find a wide variation in the intensity in which debt-for-equity swaps are implemented and a seemingly positive correlation with the directional changes in leverage.

We provide a more concise and easily interpretable view of the changes in the cash flow statement in Table 6, where we present the cross-sectional average of the firms' *average* cash flow sources before and after a negotiation. The before period is defined as the average of the years between $t-4$ to t. The after period is defined as the average of the years from $t+1$ to $t+4$.[9]

[9] Each variable is defined as sources minus uses. So, for example, investment and dividends are negative as they represent a net use, whereas operating cash flow is positive as it represents a net source. The qualitative results do not change if we exclude the negotiation year or if we use a different window, for example, $t-3$ to $t+3$ or $t-2$ to $t+2$.

Table 6 Sources and Uses of Cash Before Around Contract Negotiations

	Before	After	Difference	*t*-stat	Observations
Panel A: No-strike firms—Cash flow items scaled by total assets					
Operating activities—net cash flow	0.1050	0.0943	−0.0107**	−4.49	406
Investing activities—net cash flow	−0.0905	−0.0696	0.0209**	5.51	406
Capex and related	−0.0681	−0.0559	0.0122**	6.82	406
M&A and related	−0.0224	−0.0137	0.0087**	2.78	406
Financing activities	−0.0108	−0.0206	−0.0098**	−2.66	406
Net total debt issuance	0.0179	0.0113	−0.0066**	−2.11	406
Net equity issuance	−0.0080	−0.0120	−0.0040**	−2.19	406
Dividends	−0.0180	−0.0176	0.0004	0.91	406
Other financing	−0.0027	−0.0023	0.0004	0.07	406
Exchange rate effect	0.0000	0.0000	0.0000	−0.08	406
Change in cash and cash equivalents	0.0037	0.0041	0.0004	0.02	406
Panel B: Strike firms—Cash flow items scaled by total assets					
Operating activities—net cash flow	0.0956	0.0897	−0.0060	−1.21	127
Investing activities—net cash flow	−0.0788	−0.0769	0.0019	0.27	127
Capex and related	−0.0611	−0.0551	0.0061**	2.15	127
M&A and related	−0.0177	−0.0219	−0.0042	−0.79	127
Financing activities	−0.0145	−0.0118	0.0027	0.44	127
Net total debt issuance	0.0109	0.0182	0.0073	1.28	127
Net equity issuance	−0.0060	−0.0132	−0.0072**	−2.62	127
Dividends	−0.0146	−0.0143	0.0003	0.55	127
Other financing	−0.0048	−0.0025	0.0023	−1.27	127
Exchange rate effect	0.0000	−0.0002	−0.0001	−1.21	127
Change in cash and cash equivalents	0.0022	0.0008	−0.0015*	−1.79	127

Notes. For each component of the cash flow statement, this table presents the cross-sectional average of the firms' values before and after a negotiation. The "before" period is defined between $t-4$ and $t-1$ relative to a strike in fiscal year t, and the "after" period extends from $t+1$ to $t+4$. *Operating activities* is defined as the net cash flow from operating activities, *Capex and related* is sale of investments minus capital expenditures, and *M&A and related* is the sale of investments plus the change in short-term investments plus other investing costs minus acquisitions minus the increase in investments. *Net total debt issuance* is the long-term debt issuance minus long-term debt reduction plus the changes in current debt. *Net equity issuance* is the sale of common and preferred stock minus the purchase of common and preferred stock, and *Dividends* is equal to cash dividends. *Other financing* is equal to other financing costs. All variables are scaled by book value of total assets as of the beginning of the year. Panel A presents results for firms that do not experience a strike. Panel B reports on firms that experience a strike. The sample extends from January 1993 to December 2008. The difference between the after and before periods are reported as well as *t*-statistics for the difference.
* and ** denote significance levels of 10% and 5%, respectively.

Panel A presents results for firms that experience a contract negotiation but no strike, and panel B presents results for firms that experience a strike.

The analysis in Table 6 shows that, after a contract negotiation, firms that do not experience a strike (panel A) significantly reduce their debt issuance (an activity that would decrease leverage ceteris paribus) and increase their equity repurchases (an activity that would increase leverage ceteris paribus). Taken together, it is not obvious as to what the overall effect on leverage would be. Figure 1 and the results in Table 5 show that the leverage of these firms does not change significantly after a contract negotiation, and it would thus appear that these changes to financing cancel each other out with respect to their effect on firm leverage.

However, the analysis in Table 6 allows us to make several qualitative observations involving firms that experience a strike (panel B) that are consistent with the trends in Figure 1. First, firms accelerate the rate at which they issue debt and also accelerate the rate at which they repurchase equity after a strike, and both changes represent activities that increase leverage. The increase in debt issuance is not statistically significant relative to the period before the strike, although the corresponding increase in equity repurchases is statistically significant. The economic magnitude of the changes is approximately equal to each other: firms increase debt issuance by 0.73% per year (as scaled by total assets) after a strike while simultaneously increasing equity repurchases by a statistically significant 0.72% relative to the prestrike period. This change is consistent with firms executing, on average, a debt-for-equity swap after a strike, an activity that increases leverage ceteris paribus.

Second, netting out dividends (which are usually paid out of net income), the change in cash produced by debt-for equity swaps is negative before a strike and positive after a strike. However, the change in financing policy does not appear correlated with changes in the investment policy, which remains almost the same as before the strike. The excess cash may be used to buffer a decrease in the cash flow from operations (which averages 9.56% before the strike and 8.97% after).

Third, whereas the overall level of investment does not change, the mix of capex and M&A related activities does change. Although firms that do not experience a

strike see a drop in the average ratio of acquisitions to total investments from 24.7% to 19.7%, firms that experience a strike increase acquisitions from 22.4% to 28.5% of total investments while maintaining total investments relatively unchanged. Although it does not appear to be related to the changes in leverage or the financing activity, it is worth exploring as it might give us another dimension in which firms react to the outcomes of their negotiations with unions.

After a contentious negotiation, we speculate that firms may attempt to relocate production to locales that are less friendly to organized labor. To accomplish this, firms can choose to start or acquire new production units in geographical areas with more favorable labor laws and/or they may abandon production units that are located in areas where unions have more legal support.

To understand whether firms engage in this kind of activity, we examine both the acquisitions of new assets as well as the disposal of existing assets. Using the SDC database, we examine the characteristics of acquisition targets pursued by the firms in our sample. We divide new acquisitions into three categories according to the geographical location of the headquarters of the target and make several findings. First, after a strike, international acquisitions increase from 29% of the total to 35%, an increase that is significant at the 10% level. This is in contrast with firms that do not experience a work stoppage, who do not significantly change the proportion of international acquisitions. We also see a reduction of acquisitions of firms located in states without right-to-work laws (and are therefore more favorable to unions). After a strike, firms reduce the acquisition of assets in states without right-to-work laws from 46% to 39% of the total (again significant at the 10% confidence level). Overall, the evidence thus suggests that work stoppages are altering the behavior of firms in that, after a strike, they increase the proportion of acquisitions in locales that are less friendly toward unionized labor.

As to the disposal of existing assets, we construct a subsample of all firms that experience a strike and select those that report asset sales in the three years after the strike (the resulting subsample consists of 41 unique firms). We then collect the list of division, subsidiaries, and production plants that are listed in the Directory of Corporate Affiliations (DCA) for each firm in this subsample in the year before the strike. We record whether the same units are present in the DCA through the next three years. For each firm we compute the rate at which units that are located in states affected by strikes disappear from the directory and compare it with the rate of disappearance of units that are in the directory but are not located in states affected by strikes. Because the BLS and BNA data sets only inform us about the location in which strikes occur and only occasionally the name of the production facility involved, we make the assumption that all units that are located in a state in which a strike occurs are as likely to have contentious future labor negotiations. Obviously, this does not have to be true, but it should give us a good approximation of how strong unions are in a particular geographical area. We find that, after a strike, firms dispose of units that are located in states where the firm has experienced a strike at a faster rate than they dispose of units located in any other state. The difference is positive for any of the three years, although it peaks at the second year (with a difference of 13 percentage points)—a result that is marginally statistically significant despite a relatively small sample size.[10]

In summary, the evidence that we present supports the idea that firms in our sample are, on average, increasing debt issuance to repurchase equity. These debt-for-equity swaps are more prominent right before a contract negotiation and after a strike, thus leading to leverage increasing in those periods. This financing activity is consistent with the theoretical predictions of Perotti and Spier (1993), who argue that shareholders can improve their bargaining position with senior claimants (such as labor unions) by issuing debt to fund equity repurchases, an action that decreases both future and current liquidity. We do not find clear evidence that increased debt issuances are linked to increased investments, but we find some interesting evidence that after a strike firms invest more internationally and in states where union bargaining power is reduced (i.e., states with right-to-work laws). We also find evidence that, after a strike, firms tend to dispose of production units that are located in states where strikes have occurred more often than they dispose of production units in state where labor disputes have not occurred.

5.3. Alternative Explanations

In the previous sections, we have argued that the trends in leverage shown in Figure 1 are consistent with the hypothesis that leverage is used as a strategic variable. Overall, firms appear to increase leverage prior to a contract negotiation, and unions are more likely to strike firms with low leverage or firms that have decreased their leverage. After a contract negotiation, we have argued that firms that experience a strike are less likely to be satisfied with their bargaining position, and their subsequent increase in leverage is consistent with their wishing to improve it prior to the next round of contract negotiations. In this section, we discuss a number of alternative explanations for these results.

[10] The cumulative rate of disposal of units located in states affected by a strike is 19% in the first year, 23% in the second year, and 30.1% in the third year. The difference relative to units located in states not affected by strikes is 5.4% (t-statistic of 0.89), 12.9% (t-statistic of 1.73), and 4.1% (t-statistic of 0.58) for the first, second, and third years, respectively.

5.3.1. Precontract Negotiation Changes in Leverage. We first discuss the precontract negotiation changes in leverage. If leverage is used as a strategic variable then we expect that, if firms wish to improve their bargaining position relative to unions, then they would increase their leverage prior to a contract negotiation. For the majority of our sample (firms that do not eventually experience a strike), this appears to be the case. Figure 1 and the results in Table 5 show that these firms significantly increase their leverage prior to a contract negotiation.

However, the prenegotiation trend for firms that experience a strike shown in Figure 1 raises a number of questions. In particular, if lower leverage increases the probability of a strike, then it seems counterintuitive that firms that experience a strike would lower their leverage prior to a contract negotiation. Why does this appear to be the case? First, and opposite the main hypothesis of this study, firms that face a high probability of a strike may deliberately chose to decrease their leverage in the years prior to a contract negotiation to build liquidity to better weather a probable union strike. If firms decrease leverage in anticipation of a contentious labor negotiation, we would expect to obtain the results in Tables 3 and 4, as well as Figure 1, but for reasons that are different from the interpretation that we have provided.

To examine whether firms increase or decrease leverage in anticipation of a strike, we use a partial adjustment model similar to those used by Fama and French (2002), Flannery and Rangan (2006), and Lemmon et al. (2008). We make a key modification to the standard model. We argue that the probability of a strike should affect the target leverage ratio of a unionized firm and therefore include the probability of a strike (defined as the probability, measured at $t-n$, that a firm will experience a strike during year t) as one of the determinants of the target leverage ratio. We find that the expected strike likelihood is a positive and statistically significant predictor of precontract negotiation changes in both book and market leverage between time $t-n$ and time $t-1$, the year before a contract negotiation, and where $n=4, 3, 2$. These results are not consistent with the alternative explanation for the trend in Figure 1 outlined above. It does not appear that firms lower leverage in anticipation of a strike and instead appear to do the opposite. This analysis requires at least one variable in the strike probability model to satisfy the exclusion restriction. As the variables available to us are only weak instruments, we warn the reader from overinterpreting the result and describe the estimation procedure and report the results in the Internet Appendix.

5.3.2. Postcontract Negotiation Changes in Leverage. As illustrated in in Figure 1 and Table 5, the average leverage for firms that do not experience a strike do not appear to meaningfully alter their leverage after a contract negotiation. However, firms that do experience a strike dramatically increase their leverage in subsequent periods. What might explain this? Contract negotiations and strikes reveal information about the relative bargaining positions of a firm and its union, and this information should inform subsequent capital structure decisions. Strikes are costly to firms (shareholders in particular), and we argue that firms that experience a strike are less likely to be satisfied with their bargaining position relative to firms that do not experience a strike and that they will therefore be more likely to seek to improve it. In particular, if a firm views capital structure as contributing to a union's decision to strike and its outcome, then we expect firms to increase leverage after a strike. We formally test this hypothesis in the next section.

There are other possible explanations, however. For example, firms may be forced to rely on creditors in response to financial strain resulting from a strike, and this may drive the increase in poststrike leverage. Additionally, firms that experience a strike may change their business practice in a way that increases leverage, but not because firms wish to use leverage as a strategic variable in upcoming contract negotiations. For example, after experiencing a strike, firms may wish to move production to locales that are less friendly to unionized labor. If this results in acquisitions that are financed with debt, then leverage would increase but not for the reasons outlined above.

With regard to the possibility that firms rely on creditors in response to financial strain placed on the firm after a strike, we note that the results in Table 6 show that these firms both increase their debt issuance and their equity repurchases in the years following a strike. Although the increase in debt issuance may be consistent with firms responding to financial distress imposed by a strike, the corresponding increase in equity repurchases does not; equity repurchases are discretionary and are not typically undertaken by financially strained firms. Also, when dividends are included, net financing is negative for the firms in our sample, both before and after a strike. That is, financing consumes rather than generates cash for our firms on average. This is also inconsistent with firms relying on financing to cover losses related to a strike.

As to the possibility that firms are accelerating investment in an effort to reorganize production away from union-friendly locales, we note that the increase in M&A and related investment does appear to increase after a strike (although the increase is not significant), and we do find some evidence that after a strike firms tend to engage in acquisitions of targets that are geographically located away from powerful unions (other countries and states with right-to-work laws). However, the overall level of investment does not

change after a strike relative to the prestrike period and this observation, combined with the fact that net financing is negative after a strike, is not consistent with firms using debt to increase acquisitions.

Overall, and consistent with the results in Table 6, we argue that the poststrike increase in leverage is primarily driven by an increase in equity repurchases financed by debt. This is consistent with the theoretical predictions of Perotti and Spier (1993) that shareholders can improve their bargaining position with senior claimants (such as labor unions) by issuing debt to fund equity repurchases.

5.4. Strike Outcomes

As discussed in the previous section, we hypothesize that firms might not be satisfied with their bargaining power after a strike and therefore might chose to change their leverage, among other things, as a way to improve the outcome of the next negotiation. However, this response does not have to be uniform across firms. In particular, we might expect firms that lose a strike to be most dissatisfied with their bargaining position and be most likely to increase their leverage as a consequence. When firms win the strike, we expect them to be satisfied with their bargaining position and not deliberately increase their leverage.

To test these hypotheses, we examine the poststrike changes in leverage in panel regressions and report the results in Table 7. This table contains estimated coefficients for regressions in which the dependent variable is the change in leverage between the year of a contract negotiation (t) and the subsequent four-year period, or $\Delta Lev_{t+n} = Lev_{t+n} - Lev_t$, where $n = 1, 2, \ldots, 4$. The independent variables are corresponding changes in various control variables (discussed in §3) and a strike indicator variable equal to 1 if the firm experienced a strike in year t. We also include time and industry fixed effects. Standard errors are clustered at the firm level.

We find that, after controlling for changes in determinants of capital structure, our results shown in Table 5 hold: firms that experience a strike increase their leverage after a contract negotiation relative to firms that do not. Panels A and C of Table 7 contain results for regressions involving book and market leverage. The estimated coefficient on the strike dummy is positive for each year from the contract estimate. In the case of book leverage, this apparent increase in leverage is significant by year $t+2$, where the estimated coefficient on the strike dummy is 0.021. By year $t+4$, the estimated coefficient rises to 0.035. The poststrike increase in market leverage is significant by year $t+3$, at 0.033. By year $t+4$, the estimated coefficient rises to 0.048. Overall, these results are consistent with the previous analysis.

In panels B and D, we report results of similar regressions where we include indicator variables that indicate

Table 7 Changes in Leverage and Strike Outcomes

	Years relative to strike year			
	$t+1$	$t+2$	$t+3$	$t+4$
Panel A: Independent variable is Δ *Book leverage*				
Strike	0.011	0.021*	0.032**	0.035**
	(1.58)	(1.88)	(2.44)	(2.15)
Adjusted-R^2	0.21	0.13	0.28	0.27
Observations	538	486	443	396
Panel B: Independent variable is Δ *Book leverage*				
Strike: Union win	0.034*	0.057*	0.072**	0.067*
	(1.72)	(1.76)	(2.34)	(1.78)
Strike: No winner	0.014*	0.021	0.032**	0.038*
	(1.70)	(1.59)	(1.99)	(1.92)
Strike: Company win	−0.002	0.010	0.016	0.016
	(−0.26)	(0.61)	(0.86)	(0.64)
Adjusted-R^2	0.21	0.14	0.28	0.27
Observations	538	486	443	396
Panel C: Independent variable is Δ *Market leverage*				
Strike	0.013	0.020	0.033**	0.048**
	(1.45)	(1.47)	(2.11)	(2.23)
Adjusted-R^2	0.45	0.48	0.53	0.51
Observations	538	486	443	396
Panel D: Independent variable is Δ *Market leverage*				
Strike: Union win	0.049**	0.039	0.053*	0.068
	(2.59)	(1.21)	(1.76)	(1.54)
Strike: No winner	0.016*	0.020	0.032*	0.061**
	(1.66)	(1.28)	(1.78)	(2.55)
Strike: Company win	−0.006	0.013	0.027	0.016
	(−0.45)	(0.66)	(0.97)	(0.41)
Adjusted-R^2	0.45	0.48	0.53	0.51
Observations	538	486	443	396
	All panels			
Control variables	X	X	X	X
Time fixed effects	X	X	X	X
Industry fixed effects	X	X	X	X
State fixed effects	X	X	X	X
Union fixed effects	X	X	X	X

Notes. This table reports the estimated parameters of an empirical model of changes in leverage from a year in which a firm experiences a contract negotiation (t). The dependent variable is $\Delta Lev_{t+n} = Lev_{t+n} - Lev_t$, where $n = 1, 2, \ldots, 5$. Panels A and B report results for changes in book leverage, and panels C and D report results for changes in market leverage. In panels A and C, the independent variable of interest is an indicator variable that is equal to 1 if the firm experienced a strike during the contract negotiation at time t (*Strike*) and 0 otherwise. In panels B and D, the independent variables of interest are three indicators that are equal to 1 if the union won the strike (*Strike: union win*), the firm won the strike (*Strike: firm win*), or the winner was indeterminate (*Strike: no winner*) and 0 otherwise. All specifications include changes in the firm and industry characteristics that determine the firm's target leverage as in Byoun (2008), as well as constant and time and industry fixed effects. Of the 140 strikes in our sample, 13 result in a clear union victory and 36 result in a clear company win. Our sample is composed of 607 contract negotiations and 140 strikes involving at least 1,000 workers. Contract negotiation data are from the BNA Labor PLUS database. Strike data are from the BNA Labor PLUS and BLS Work Stoppage databases. Accounting and stock market data are from the COMPUSTAT and CRSP databases, respectively. The sample extends from January 1993 to December 2008. Standard errors are adjusted for clustering at the firm level; t-statistics are in parentheses.

* and ** denote significance levels of 10% and 5%, respectively.

whether the union won the strike, the firm won the strike, or no winner was determined (a discussion on the determination of the strike outcome is included in §3.1). As anticipated, when unions win the strike, the poststrike change in leverage is most pronounced: by $t+4$, the book and market leverage of the affected firms increases by 0.067 and 0.068, respectively (thought the result for market leverage is not statistically significant, with a t-statistic of 1.54). For the case of no winner being determined, the poststrike book and market leverage of affected firms increases 0.038 and 0.061, respectively, and both increases are statistically significant. Economically speaking, these poststrike increases in leverage are less important than when the union won. Finally, when firms win the strike, the estimated coefficient on the relevant indicator variable remains statistically and economically insignificant for all poststrike years.

In unreported results, the tests in panels B and D are repeated, with the indicator variable for the company winning the strike being replaced with the dummy variable for a firm experiencing a strike. In this specification, the interpretation of the t-statistics for the estimates on the indicator variables for the cases where either the union wins the strike or no winner was determined is whether leverage increases are different from the case when the firm won the strike. We find that, in the case of both book and market leverage, the difference between changes in leverage when either the union wins the strike or no strike winner was determined are significantly different from the case where the firm wins the strike until $t+3$, after which the differences are not statistically significant (t-statistics correspond to p-values of approximately 0.15).

Also in unreported results, we repeat the poststrike analysis reported in Table 5 but divide the sample into cases where the union wins the strike, no winner was determined, and where the firm wins the strike. The results are similar to those reported in Table 7. Relative to the strike year, firms that lose the strike increase their book and market leverage by 0.077 and 0.137 (with t-statistics of 2.20 and 3.60), respectively, by year $t+3$. This result is similarly pronounced in our difference-in-difference tests. When no winner was determined, the results are economically and statistically similar to the general case reported in Table 5. Finally, the poststrike change in leverage is generally not significant when firms win the strike.

6. Conclusion

A growing empirical literature has found that nonfinancial stakeholders affect capital structure decisions. We study one important nonfinancial stakeholder, labor unions. We provide evidence consistent with the union's decision to strike being affected by a firm's capital structure. Furthermore, we find evidence that firms use leverage as a strategic variable. Before a contract negotiation, firms increase their leverage. Also, we find that when firms experience a strike as part of a contract negotiation, they subsequently increase leverage consistent with an attempt to use debt as a strategic variable during the next round of contract negotiations.

Our study complements recent findings that the strategic use of debt features prominently in capital structure decisions. This suggests that the inclusion of variables motivated by the strategic use of debt would improve the empirical specifications of cross-sectional models of leverage. Also, because we find significant time-series variation in leverage ratios, our findings are consistent with firms actively make relatively large changes to their capital structure policies. Our paper is therefore in line with a relatively recent strand of the finance literature that questions the stability of average leverage ratios and documents large movements in individual firms' leverage, as in DeAngelo and Roll (2015) and Denis and McKeon (2012).

Supplemental Material

Supplemental material to this paper is available at http://dx.doi.org/10.1287/mnsc.2015.2267.

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