MICHIGAN ROSS

Working Paper

Unionization, Cash, and Leverage

Martin C. Schmalz
Stephen M. Ross School of Business
University of Michigan

Ross School of Business Working Paper
Working Paper No. 1215
April 2015

Unionization, Cash, and Leverage

Martin C. Schmalz*

Abstract

What is the effect of unionization on corporate financial policies? The average unionized firm responds with lower cash and higher leverage to a unionization election than the average firm escaping unionization. However, using a regression discontinuity design I find that the causal effect of unionization is close to zero on average, but heterogeneous across firms. For the subset of large and financially unconstrained firms, the causal effect is positive on leverage and negative on cash; the opposite is true for small and financially constrained firms. These results help reconcile controversially discussed views on how corporate finance and labor interact.

JEL Classification: J50, G32

Keywords: Unionization, cash, leverage, capital structure, labor adjustment costs

*schmalz@umich.edu. Stephen M. Ross School of Business, University of Michigan. This paper is based on the first chapter of my dissertation at Princeton University. I am deeply grateful to my adviser Markus Brunnermeier, as well as to David Sraer, Alexandre Mas, and David Lee for invaluable support, ideas, and advice, and to David Lee and Alexandre Mas for generously providing data. For helpful discussions and comments, I would like to thank Sergey Zhuk and other Princeton classmates, John DiNardo, Henry Farber, Mireia Giné, Bo Honoré, Oleg Itskhoki, Martin Kanz, Hyunseob Kim, David Lee, Thomas Mertens, Roni Michaely, Benjamin Moll, Ulrich Müller, José Scheinkman, Matthew Serfling, Hyun Song Shin, Toni Whited, as well as seminar participants at Bocconi, Boston University, Columbia GSB, Harvard Business School, HSE Moscow, IESE, INSEAD, London School of Economics, University of Michigan (Finance; Labor Economics), NBIM, Ohio State University, Princeton, Rochester, SMU, UC Irvine, University of Vienna, USC, and USI Lugano. Laurien Gilbert and Alexis Furuichi provided excellent research assistance. I gratefully acknowledge generous financial support through a Fellowship of Woodrow Wilson Scholars and an NTT Fellowship from the Mitsui Life Financial Center.

1 Introduction

Two polar views shape the debate on the interaction between labor and corporate finance. According to one view, firms strategically choose their financial policies to attain a better bargaining position in future wage negotiations with their employees. In particular, according to this "bargaining view," firms respond with higher leverage and lower cash to unionization or the threat thereof [Bronars and Deere, 1991, Matsa, 2010]; see also Baldwin [1983], Dasgupta and Sengupta [1993], Perotti and Spier [1993], Sarig [1998]. The intuition is simple: unionized workers have a decreased ability to extract rents when the firm has low cash reserves and already committed future cash flows to debt holders. Therefore, reducing financial flexibility can reduce workers' incentive to unionize. The alternative view holds that an increase in fundamental or operating risk arising from more rigid labor inputs leads to an increased need for financial flexibility [Mauer and Triantis, 1994, Gamba and Triantis, 2008, Pratt, 2011, Kahl et al., 2013, Chen et al., 2013].[1] Unionization is one reason why an increase in labor rigidity can occur. Therefore, according to the "financial flexibility" view, unionization should cause an accommodating response with more cash and less leverage.

While often discussed controversially, there is no contradiction between these forces. In theory, both can be present at the same time in the same firm, whereas it is an empirical question which one is quantitatively more important in the average firm, or in particular subsets of firms. Also, it is conceivable that the "bargaining" motive is more important *ex ante* (before a unionization attempt), whereas the "financial flexibility" motive is more important *ex post* (in case actual unionization occurred).[2] Lastly, it is important to understand if the response of firms becoming unionized is the

[1] This prediction is also consistent also with models emphasizing the effect of asset-market frictions on liquidity risk management [Bolton et al., 2011]. Note that human capital is an asset without market liquidity from the perspective of the firm. A similar prediction arises from firms' internalization of expected costs of bankruptcy born by their employees [Titman, 1984, Jaggia and Thakor, 1994, Berk et al., 2010]: implicit promises of wage insurance are only credible when the firm has the financial reserves to honor them. Agrawal and Matsa [2012], Brown and Matsa [2012], Kim [2012], Chemmanur et al. [2013] provide empirical support for variations of the insurance hypothesis, see also Caggese and Cuñat [2008], Sraer and Thesmar [2007], Shivdasani and Stefanescu [2010], Ellul et al. [2013].

[2] The effect could also be non-monotonic: too much financial flexility can hurt a firm's bargaining position, whereas too little financial flexility can reduce shareholder value because of financial distress costs. The empirical question is what the marginal effect of unionization is on the optimal level of financial flexibility.

effect of unionization *per se* (i.e., the effect of changes in labor rigidity as well as how bargaining is conducted as a result of unionization), or if the response is driven by potentially unobservable variables that are correlated with more or less strong support for unionization, such as employee morale. In other words, what is the causal effect of unionization on corporate financial policies? As is evident from an active empirical literature on the effect of unionization (and labor rigidity more generally) on corporate financial choices, there is much interest in these questions. Indeed, approaching answers to them is of first-order importance for advancing our understanding of which theories are most important in describing corporate financial decision making.

To answer these questions, this paper employs a data set that contains all unionization elections in U.S. establishments belonging to Compustat firms between 1961 and 1999. The data set and research design I use was first developed by DiNardo and Lee [2004], and extended by Lee and Mas [2012] to study the causal effect of unionization on productivity and wages as well as on stock returns, respectively. In contrast to earlier papers in the corporate finance literature that have used variation in the ability to form unions at the state or industry levels, I directly examine firms experiencing a unionization attempt. The research design also allows me to determine the causal effect of unionization on firm policies.[3]

I find that the average firm experiencing a unionization election reduces its cash-to-asset ratio and increases leverage. This observation is consistent with the bargaining view, but also with the notion that firms in bad financial condition impose more unemployment risk on employees, and employees facing higher unemployment risk have higher incentives to (attempt to) unionize in order to enjoy better protection of their interests in bankruptcy. Next, firms in which more than 50% of workers voted for unionization (which almost surely leads to unionization of the establishment) respond with 2% lower cash-to-asset ratios and +0.8% higher leverage to an election, compared to firms in which less than 50% voted for unionization (which almost never leads to unionization). This

[3]The data go back much further in time than other papers using the election design to investigate the unionization effect on other outcome variables, such as innovation [Bradley et al., 2013] or staffing levels and quality [Sojourner et al., 2012].

result is consistent with the predictions of the bargaining view. Importantly, however, this analysis ignores the information content of the precise share of the vote cast in favor of unionization, i.e., it gives equal weight to unionization events in which 100% of workers voted for unionization and events in which only 50.1% voted for unionization.

Because the degree of union support, reflected in the vote share for unionization, may be correlated with unobserved characteristics of the firm or the workforce, estimating the causal effect of unionization requires comparing observations in which only unionization but none of the other characteristics changed significantly. The discontinuity in unionization outcomes at the 50% vote share threshold affords such a comparison. I find that the financial policy response of close "winners" (from the perspective of the union) and close "losers" is almost identical, both in terms of cash and in terms of leverage. Thus, the causal effect of unionization *per se* is zero; the average effect is entirely driven by the tails of the vote share distribution. From this result alone, one can either conclude that the "bargaining" and "flexibility" effects exactly cancel out for the subset of firms at the discontinuity, or that unionization *per se* is not a first-order driver of corporate financial policy.

To try and tell apart these interpretations, I analyze the average and causal effects of unionization in subsamples of the data. If there is truly no causal effect of unionization, that should be true in subsamples of the data as well. By contrast, if both mechanisms are equally important for the average marginal firm, perhaps one is more important than the other in subsets of firms. To investigate this possibility, I split the sample by the degree to which the firms can freely adjust their financial policy at the time of the unionization election, as measured by ranking below (unconstrained) or above (constrained) the median of the Whited and Wu [2006] index of financial constraints. A firm presently unconstrained can become constrained in the future, and therefore has a motive for risk management; according to the flexibility view, it should increase cash and decrease leverage in response to an increase of labor rigidity. By contrast, a firm presently constrained faces a tradeoff between two exclusive uses of the existing levels of cash and debt capacity: using (and thus reducing) it now for investment, or keeping it for the future [Rampini et al., 2014]. Thus, a presently constrained

firm will, if anything, reduce its financial flexibility in response to an increase of labor rigidity, but definitely not increase it (because it can't). By contrast to these predictions, the simplest version of the bargaining view prescribes a tightening of financial flexibility for both types of firms.

Using the regression discontinuity design, I find that unionization causes a 6.2% increase of cash-to-assets ratios among financially unconstrained firms. This effect, corresponding to a 0.43-percentage-point increase from 6.86% to 7.29%, or $23 million for the average firm, is not only highly statistically significant but also economically sizable: the increase corresponds to about 13% of the average firm's wage bill. I also find a highly statistically significant 2.4% reduction of on-balance-sheet financial leverage, which corresponds to a 1-percentage-point decrease of leverage from 42.8% to 41.8%, or a $63 million reduction in debt financing. By contrast, financially constrained firms reduce cash and increase leverage, although these estimates are less precise. I view these results as most easily compatible with the view that financial flexibility is a relatively more important consideration for firms that can actively and freely adjust their financial policies. (Indeed, financial flexibility is considered the most important driver of corporate financial decisions in general [Graham and Harvey, 2001].) This interpretation is also corroborated by the finding that the average effect of unionization reported above (a decrease of cash and increase in leverage) is entirely driven by the subsample of financially constrained firms, which presumably cannot freely adjust their financial policies.

An alternative interpretation is that bargaining considerations are relatively more important in firms that are already financially constrained, whereas they are less important compared to financial flexibility in less financially constrained firms. Given the finding by Benmelech et al. [2012] of a link between financial distress and wage concessions, this interpretation seems plausible; however, a different explanation would need to address why bargaining considerations are unimportant in financially unconstrained firms.[4] Under either interpretation, the RD results indicate that union-

[4]Given that the measure of financial constraints I use is highly negatively correlated with firm size, the interpretation could also be that the bargaining motive is more important for small firms and financial flexibility is a more important concern for larger firms, e.g. because an increased cost in quasi-fixed [Oi, 1962] labor costs has the potential to spill over to other plans. Also, there is an ongoing debate on how well measures of financial constraints

ization *per se* is indeed an important determinant of corporate financial policies, but the effect is heterogeneous across firms.

The fact that the average firm that becomes unionized responds with a more aggressive financial policy supports the bargaining view, as advocated by earlier papers that use, among others, industry-level unionization rates and state-level variation in the threat of unionization [e.g., Bronars and Deere, 1991, Klasa et al., 2009, Matsa, 2010]. A marginal benefit of the present study is that I observe actual unionization events at the firm level – and can thus confirm that most firms becoming unionized indeed appear to follow the "bargaining" prescription.[5] By contrast to the broader validity of these "average" estimates, without further assumptions regression discontinuity estimates are only valid "locally," i.e., for a small subset of firms close to the discontinuity. The greater internal validity, however, allows a causal interpretation of the RD results. As per the interpretation proposed above, they support earlier findings showing that financial flexibility and labor rigidity or firing costs are positively correlated at the state or country level [Kuzmina, 2012, Simintzi et al., 2015, Serfling, 2014]. These authors' designs are valid for broader samples; the relative clean RD estimates are an advantage of the present paper. A remarkable distinction from the literature in general is that I can support both views within the same data set, depending only on the empirical model used and the subsamples of the data investigated. Together with the cross-sectional finding that the same answer does not apply to all firms even within the RD design, I hope to contribute to a more differentiated and less controversial debate of the question on how labor and corporate finance interact.[6]

measure financial constraints [Farre-Mensa and Ljungqvist, 2013].

[5] Matsa [2010]'s use of variation in the facility of unionization at the state level is an ideal proxy for the threat of unionization and thus most literally tests Bronars and Deere [1991]'s proposed interaction between "the *threat* of unionization, [and] the use of debt" (emphasize is mine). By contrast, I investigate the causal effect of actual unionization at the firm level. However, firms that successfully deterred workers from even attempting to unionize will never appear in my data set. Thus, our results speak to subtly different questions.

[6] Outside the corporate finance domain, Gourio [2007], Chen et al. [2012, 2011], Ochoa [2013] study the asset-pricing consequences of unionization and other forms of labor rigidity, and find that labor risk is associated with a higher risk of both equity and debt, leading to higher risk premia and cost of capital for such firms.

2 Data and Empirical Strategy

2.1 Endogeneity Concerns with OLS Estimates

The objective of this paper is to estimate the effect of unionization on cash-to-assets ratios and financial leverage. Cross-sectional regressions of the form

$$Y_i = \alpha + \beta \cdot D_i + \gamma \cdot X_i + \varepsilon_i, \tag{1}$$

where Y_i is firm i's financial strategy (e.g., cash-to-assets ratio, or leverage), D_i is a dummy that captures union presence, X_i are known determinants of the financial strategy, and β is the coefficient of interest, provide a good starting point. The coefficient β indicates the effect of a unionization election on the average firm that becomes unionized ($D_i = 1$) compared to the average firm that experiences an election, but escapes unionization.

While such an estimate is interesting, it should not necessarily be interpreted as the causal effect of unionization. One endogeneity concern is that some unobserved factors, Z_i, may jointly affect the net benefits of unionization and the optimal financial strategy of the firm. The existence of such a factor would lead to biased estimates of β. For example, labor adjustment costs (search, training, and firing costs) could be such a factor. Unionization is particularly attractive for employees who are easy to replace, because workplace security is low absent union representation. At the same time, bankruptcy costs and therefore the costs of debt are particularly low for such firms, resulting in higher levels of leverage.

Another reason for endogeneity concerns is a reverse-causality argument. Employees of firms with lower levels of cash may be more likely to seek unionization to achieve workplace protection, because bankruptcy is more likely in these firms. Low levels of cash in unionized firms would then not be the firm's response to unionization, but unionization would be the employees' response to low levels of cash.[7] As these considerations illustrate, β should not be interpreted as a causal effect

[7] The summary statistics of this paper indeed reveal that the average firm that experiences a unionization election

of unionization on the firm's financial strategy.

In an attempt to avoid or mitigate such concerns, this paper uses the regression-discontinuity design and data set of unionization elections pioneered by DiNardo and Lee [2004] (DL) and extended by Lee and Mas [2012] (LM); I follow their procedure in all respects, unless otherwise noted. The institutional background giving rise to this research design is briefly described as follows.

2.2 Institutional Background

The National Labor Relations Act grants unionized workers the right to collectively bargain with their employers about working conditions and wages. Yet both the benefits and costs of unionization differ across workers. In particular, whereas causal effects of unionization on wages are close to zero (DL), the benefits of unionization for workers can be large in terms of employment protection, particularly for workers that are easy to replace absent unionization, for example, because they carry little firm-specific human capital. By increasing firing costs, unionization artificially increases the costs of replacing a worker, thus making labor turnover less attractive to the firm irrespective of the level of firm-specific capital associated with the worker. Thus the employment-protection benefit of unionization is particularly large for workers that would otherwise impose low labor adjustment costs on the firm, while the benefit is small for workers that are already hard to replace absent unionization. Unionization also comes at a differential opportunity cost to workers. For example, individual differences in productivity are difficult to honor through differential wages when a collective-bargaining agreement exists. Moreover, the potential benefits of unionization extend to several dimensions, including working conditions, grievance procedures, and seniority rules [Freeman and Medoff, 1984]. Different workers might lose different existing privileges relative to other workers in these dimensions. Because the costs and benefits of unionization are not identical across workers, the decision regarding whether to unionize is a non-trivial social-choice problem.

As a solution to this problem, the establishment of a new bargaining unit in U.S. establishments

(whether the election eventually succeeds or not) has declining cash levels and increasing leverage.

typically requires a secret ballot election, in which workers vote for or against the establishment of union representation [Farber, 1999]. Of course, the outcome of the election is not known with precision before the fact, much less can the actors precisely control the outcome. In my data set, a bargaining unit is created in an establishment if and only if a simple majority votes in favor of unionization. That is, establishments with a union vote share larger than 50% get "treated" with unionization, whereas no unionization treatment occurs if the vote share is below 50%. "Compliance" with the treatment of a vote for unionization is 99.9%. (Lee and Mas [2012] provide more details.) This discontinuity in the treatment of firms with unionization at the 50% vote-share threshold suggests the use of a regression-discontinuity design. The idea is to compare the corporate-finance response to unionization elections in firms in which the union barely wins with those in which the union barely loses an election. The difference between these firms' financial-policy responses can be interpreted as the causal effect of unionization under some assumptions that I discuss and test below.

2.3 Regression Discontinuity Design

Following precisely the procedure of Lee and Mas [2012], I estimate the causal effect of unionization as the size of the jump in financial policy at the 50% vote-share threshold of a sixth-order polynomial of the union vote share. The regression specification is:

$$\Delta Y_i = \alpha + \beta \cdot win_i + \sum_{j=1}^{6} \gamma_j m_i^j + \varepsilon_i, \tag{2}$$

where ΔY_i is the firm's financial-policy response to a unionization election in one of its establishments, and m_i, defined as vote share minus 50%, is the winning margin attained in that election. win is a dummy that takes unity, $win_i = 1$, if the union wins the election, that is, when the vote share is larger than 50% or, equivalently, when the winning margin is positive, $m_i > 0$. β is the estimated causal effect of unionization, the parameter of interest. In all specifications, I test against

the null hypothesis that unionization has no casual effect on cash and leverage.

The results are robust to alternative specifications of the regression model, as I discuss in detail when presenting the results and robustness tests. Among them, I recognize that multiple elections can happen in the same firm. In some cases, they even overlap; that is, the year before one election can be the year of another election. Because the elections are likely to take place in different establishments, I follow LM and regard such observations as independent, but cluster errors at the firm level.[8]

2.4 Data Set Assembly

I combine annual CRSP/Compustat fundamentals from WRDS with the NLRB database of unionization elections assembled by LM. It contains vote-share information for all unionization elections that took place in the United States between 1961 and 1999.

An important manipulation of the data concerns the vote share. As DL explain, small elections are less likely than large ones to provide a close outcome. To avoid bias, I follow DL's procedure and drop elections with less than 100 votes cast, adapt the vote share for elections with an even number of votes cast to correct for a potentially remaining bias, and "bin" the remaining data in 20 equally spaced vote-share bins for both the graphical and quantitative analysis. The results are similar without this manipulation, or if I use a different number of bins. Like LM, because de-unionization elections are rare and most unions simply cease to be active when their support fades, I drop union-decertification events, retaining only certification elections.[9] I discard nine observations that do not satisfy basic accounting identities.

The characteristics of elections are as follows. Elections occur at a higher frequency in the earlier

[8] Excluding elections that are less than three years apart does not qualitatively change the results, possibly because multiple elections in the same firm more frequently arise in very large firms, and elections in very large firms have a relatively small influence on the main results: the elections take place at an establishment level, whereas the outcome variables are measured at the firm level.

[9] For decertification events, the median fraction of the workplace voting is 2 percent, and approximately 30 percent of elections in the sample involve less than 1 percent of the company's workforce [Lee and Mas, 2012]. Any potential results from these observations would therefore correspond to a very particularly selected sample, resulting in very weak external validity.

part of the sample; the average year of the election is 1976. Seventy-five percent of the elections occur in manufacturing industries (SIC codes starting in 2 or 3), 7% occur in Transportation, Communications, Electric, Gas&Sanitary Services (SIC codes starting in 4), and 7% in retail and wholesale trade (SIC codes starting in 5). Other industries represent less than 3% of the elections. The average number of votes cast in an election is 340; the maximum is 5,948. The average number voting for unionization is 146; the maximum is 2,760. 29.7% of unionization elections are successful (from the perspective of the union). I discuss the distribution of vote shares in more detail below.

2.5 Corporate Finance Variable Definitions

The dependent variable is the change of financial policy in response to unionization. I use log differences $ln(y_{after}) - ln(y_{before})$ to measure that change. y_{before} is the average of the three previous years' values, and y_{after} is the average of the current and next years' values. The variable definitions y_t are as follows: *cash-to-assets ratio* is cash and short-term investments (Compustat: CHE) divided by total assets (AT); *net leverage* is total liabilities (LT) minus cash and short-term investments, divided by total assets; *operating leverage* is defined as in Novy-Marx [2011] as the cost of goods sold (COGS) plus selling, general, and administrative expenses (XSGA), divided by total assets. *Market leverage* is total liabilities divided by the market value of equity.[10] Auxiliary results include as outcome variables profitability (EBITDA/sales), capital expenditures as a share of sales (capx/sales), number of employees (emp), and cost of labor as a fraction of sales (total staff expense xlr / sales).

A differential response may obtain for financially constrained and financially unconstrained firms, for example for "dynamic risk management" Rampini et al. [2014] reasons, as laid out in the introduction. It is conceivable that only financially unconstrained firms have the capacity to actively

[10] I focus my discussion on book leverage because I am interested in the firm's choice of leverage rather than changes in leverage driven by any market reaction to unionization elections. However, market-leverage results point in the same direction. Also, I take total liabilities as the numerator of the leverage measures to avoid that changes in accounting practices associated lead to a change in the level of leverage I measure. It turns out, however, that the results are robust to alternative definitions.

and freely adjust their capital structure and cash reserves, whereas financially constrained firms are constrained in their choices, and may follow a more passive policy. I proxy for financial constraints with the Whited-Wu measure of financial-constraints risk [Whited and Wu, 2006] (WW). That is, for every firm that comes up for election in year t, I calculate the index ww at time $t-1$ as [11]

$$ww = -0.091 * ib - 0.062 * div + 0.021 * ltlev - 0.0044 * ln(at) + 0.102 * isg - 0.035 * sg, \quad (3)$$

where ib is income before extraordinary items, div is a dividend-payer dummy, $ltlev$ is long-term debt (Compustat $dltt$) divided by total assets (at), $ln(at)$ is the natural logarithm of total assets, isg is average sales growth of firms in the same 3-digit SIC-code industry, and sg is the firm's sales growth (year-on-year log difference of sales).[12] I show in the robustness-tests section that qualitatively similar results obtain with four other standard measures of financial constraints.

For one of the robustness checks, I construct a ratings variable as an alternative measure of financial constraints. I use S&P and Fitch long-term ratings, as well as S&P short-term credit ratings. I construct a one-dimensional ratings variable from these three sources. In so doing, I can use more ratings observations than I could by relying on long-term ratings alone. I assign "1" to AAA, "2" to AAA-, and so forth for long-term ratings, and corresponding values for A-1+, A-1, ... for short-term ratings. The mean of the three numbers constitutes my ratings variable.

2.6 Summary Statistics

After conditioning all variables on median plus/minus 5 times interquartile range, I obtain 4,631 observations. Similar results obtain when I winsorize or condition on being within the 98% of observations; without filters, the results are slightly less precise because of outliers. I present summary

[11] The index does not substantially change from before to after the election in firms of either subsample; also, there is no significant time trend in which firms it sorts in which subsample. As a result, using the sample mean for each year does not significantly change the results. Also, splitting the sample by the WW-index, which is constructed from corporate finance variables, does not interfere with the research design, whose outcome variable is a corporate finance variable. The sample split is with respect to the *ex ante* WW index.

[12] I don't reestimate their model to avoid an endogeneity concern that arises because financial constraints risk is potentially one of the factors why some firms get selected into my sample.

statistics for the main variables in Table 1. To conserve space while providing a differentiated picture, I discuss the properties of the unconstrained and constrained subsamples, the combination of which constitutes the full sample. The number of observations in the unconstrained (1804) and constrained (1480) subsets is the number of elections for which I observe the full set of Compustat variables I use in any of the specifications. The sample of constrained firms under this measure is slightly smaller than the sample of unconstrained firms, because constrained firms tend to be smaller by construction with the Whited-Wu index, and smaller firms have more missing observations. However, in most tests, I have more than 2,200 observations for both subsamples, and the two samples are similarly large.

The average unconstrained firm has \$6.1billion total assets, a market cap of \$4.8 billion, and net leverage of about 42%. The average firm in the constrained subsample has \$598 million total assets, a market cap of \$321 million, and net leverage, similar to the unconstrained firms, of 43%. Cash-to-assets ratios are 6.8% and 6.5% on average, respectively. Both in the unconstrained and constrained subsamples, the average firm that experiences an election reduces cash by -4.1% and -2.6%, respectively, comparing the average in the three-year period after the election with the three-year period before the election, and increases net leverage by +2.6% or +2.4%.[13] This observation is consistent with the idea that firms under the threat of unionization strategically use reduced cash and increased leverage to attain a better (potential) bargaining advantage with workers. However, it is also consistent with the view that firms that expose their employees to more employment risk are more likely to be targeted by unionization attempts, because unions protect employees in bankruptcy. This is why an investigation of the response to actual unionization elections, conducted below, is of interest. The full sample of firms ranks within the middle tercile of Compustat firms in the year and quarter of the election in terms of size, profitability, and close to the median for Tobin's Q, profit margin, return on assets, and dividend ratio, whereas the average firm in the

[13] Also, compared to well-known average cash holdings of Compustat firms that have much increased in the last three decades [Bates et al., 2009, Opler et al., 1999, Azar et al., 2015], the sample firms' cash holdings are much lower. Recall that most observations occur before the big run-up in cash holdings.

"unconstrained" sample is larger and slightly more profitable by construction.

2.7 Validity Checks of the Regression Discontinuity Design

The key identifying assumption is that *precisely* predicting whether a union will closely win or closely lose an election is impossible. (See Lee and Lemieux [2010] for a formal description of the methodological procedure.) Otherwise, i.e. if the outcome were perfectly predictable, firms could react to the election outcome before it occurs, and a change in the financial strategy comparing the years after the election with the years before would not correspond to the firm's reaction to a surprise treatment with a shock to labor rigidity. However, the election outcome is not necessarily entirely unpredictable – the firm can have a general sense of whether it likely will become unionized or not. The design requires only that the precise outcome of the election cannot be predictable with perfect precision in a close neighborhood of the discontinuity threshold. An examination of whether the distribution of vote shares is continuous around the 50% threshold can validate this assumption. Figure 1 shows that the vote-share distribution is indeed quasi-continuous for the whole sample as well as for the unconstrained and constrained subsamples. It does not display an appreciable discontinuity at the 50% vote-share threshold for either subsample. It looks similar for all other sample splits I use later.[14]

Moreover, Table 2 shows that key observable characteristics of firms that end up getting barely unionized do not differ significantly from those of firms that end up barely escaping unionization ex ante, as measured in the year before the election. Thus firms in which the union barely wins are not systematically different with respect to their observable characteristics than firms in which the union barely loses the election, except for what concerns the assignment of unionization. In other words, for the set of firms close to the 50% vote-share threshold, assignment of unionization is indeed quasi-exogenous. As in a medical trial, I can therefore regard barely unionized firms as the

[14] McCrary [2008] tests are unsuitable to test the validity of the design used here not only because test is informative only if any manipulation is monotonic and thus not necessarily informative, but also because of the manipulation of the vote share as developed in DL and LM. I am grateful to John DiNardo for guidance on this question.

"treated" part of the sample and the barely not-unionized firms as the control group.

The quasi-continuity of the vote-share distribution and the t-test results are important pieces of evidence for the validity of the research design. However, of course, variables unobserved by the researcher but observable to the firm might correctly predict close election outcomes. This inherently untestable hypothesis is a remaining limitation of the research design. Other standard RD validity checks are presented in the robustness- and placebo-test sections below.

3 Empirical Results

3.1 Graphical Analysis

Figures 2 through 4 present the main results corresponding to the "causal effects" regression specification (2). Figure 2 plots firms' cash-to-asset responses to an election over the winning margin of the election. Each dot represents the average response of firms in a given vote-share bin. Establishments in the right half of each graph become unionized; establishments in the left half escape unionization. The establishments close to the vertical line at a zero winning margin belong to the treatment firms (on the right) and the control firms (on the left). The solid lines represent the fitted sixth-order polynomial through all observations; the dotted lines are 95% confidence intervals. The difference in responses between the treatment firms just to the right and the control firms just to the left of the winning threshold is the causal effect of unionization. The difference is represented by a "jump" in the solid regression line; it is significant if the confidence bands don't overlap. The coefficients reported in Table 3 correspond to any such jump at a winning margin of 0%.

The fact that there is no perceptible jump in the polynomial fitted through the dots in Figure 2 indicates that unionization per se has no significant effect on firms' choice of cash-to-asset ratios. Similarly, Figure 3 indicates that there is no causal effect on book leverage, and Figure 4 shows that the effect on market leverage is also not significantly different from zero. Panel C of Table 3 confirms these readings: the causal effect of unionization on cash-to-asset ratios and either book or

market leverage is close to zero.

However, notice that the average firm to the right of the threshold (i.e., a firm becoming unionized) clearly responds with lower cash and higher leverage, compared to the average firm to the left of the threshold. Thus, it appears that the average firm, and indeed most firms, respond to unionization by a tightening of financial flexibility. Before I investigate average effects in detail, however, I present a graphical analysis of the subsamples.

The upper left graph of figure 5 shows that unionization has a significant positive effect on unconstrained firms' cash-to-assets ratios. The upper right graph shows that that unionization has a significant negative effect on financial leverage. The lower left graph shows that a traditional measure of operating leverage does not capture the increase of labor rigidity imposed by unionization.[15] The lower right graph shows that unionization does not have a signifiant causal effect on market leverage either.

Figure 6 repeats the analysis of Figure 5 for financially constrained firms. It shows a qualitatively opposite pattern as Figure 6, although the results are less statistically significant. In particular, the causal effect on cash-to-assets ratios is negative and seems marginally statistically significant. The causal effect on financial leverage, operating leverage, and market leverage are all positive, also marginally statistically significant, as I will verify below.

3.2 Quantitative Analysis of Causal Effects

Table 3 presents the numerical values of the estimated causal effect of unionization on cash-to-assets ratios, net leverage, operating leverage, and market leverage. The upper panel displays the estimate for firms that are likely able to freely choose their financial strategies (the "unconstrained" firms). Unionization causes 6.2% higher cash-to-assets ratios, compared to a hypothetical, non-unionized, but otherwise identical firm. This effect, corresponding to a 0.43-percentage-point

[15] Whereas adjustment costs are not fixed costs and therefore don't affect present-period operating leverage, they do immediately increase the rigidity of operating leverage. The latter, of course, is difficult to measure, which is why regarding unionization as an immediately observable proxy is informative also about the broader question how labor rigidity affects corporate financial decisions.

increase from 6.86% to 7.29%, or $23.3 million for the average firm, is highly statistically significant. Concerning economic significance, it corresponds to about 13% of the average firm's wage bill. Given the amount of cash a firm needs to provide to survive a strike, this the estimate on cash-to-assets seems a large but reasonable order of magnitude. Also, note that given that the estimates represent the firm's response to an election in one of its establishments, one should regard this estimate as a lower bound. The 2.4% reduction of on-balance-sheet financial leverage corresponds to a 1-percentage-point decrease of leverage from 42.8% to 41.8%, or a $63 million reduction in debt financing for the average firm. The estimated coefficient on debt seems like an economically significant magnitude as well. As indicated by the plots, unionization has essentially no effect on operating leverage. Point estimates for market leverage point in the same direction as financial leverage, but the effect is not statistically significant. These results are consistent with Hypothesis 1, and inconsistent with the null hypothesis that there is no effect of unionization on cash and leverage for this subsample.

The second panel of Table 3 gives estimates for the constrained subsample. The estimates point to a 4.9% reduction in cash-to-asset ratios and a 2.3% increase of leverage as a causal effect of unionization. While the magnitudes are similarly large as for the unconstrained firms, none of the estimates are strongly statistically significant; neither are the estimated increases of market leverage and operating leverage. These results are consistent with Hypothesis 2. The third panel of Table 3 shows that the empirical design detects no causal effect of unionization on any variable in the pooled sample, which is expected given opposite predictions and empirical results for the two subsamples.[16]

[16]I do not expect the causal effect of unionization to be reliably related to industry classifications, because the unionization benefits to workers are inversely related to labor adjustment costs, that is, to the extent that replacing employees is costly. As a result, observing unionization in human-capital-intensive industries is rare. For the same reason, given a rare case of unionization in such an industry, its effect on the financial policy of affected firms should be small. An example is unionization at Goldman Sachs [Tabuchi, 2012], which I presume had little effect on the firm's financial policy choices. Indeed, estimating differences across industries in the causal effect of unionization does not reliably produce significant results. The insignificance can of course also be driven by a lack of power in these tests as the sample becomes quite small – recall that 75% of the sample firms are in manufacturing, and the next-largest industry accounts for only 7% of the elections.

3.3 Quantitative Analysis of the Average Effect of Unionization

A comparison of the average response to all unsuccessful (from the perspective of the union) elections on the left side of figures 5 and 6 with successful elections on the right side suggests the average firm that gets unionized responds with lower cash-to-assets ratios and higher net leverage to an election, compared to the average firm that does not get unionized. Formally, this comparison corresponds to estimating β in the following regression:

$$Y_i = \alpha + \beta \cdot win_i + \varepsilon_i. \tag{4}$$

In this specification, the identifying information contained in the vote share is discarded. Table 4 shows the quantitative results. Driven by elections in which the union wins or loses by a wide margin (as opposed to close elections), and entirely driven by the constrained subsample, the average unionized firm's cash-to-assets ratios decrease by 2% and leverage increases by 1%, compared to the average non-unionized firm. These average effects go in the opposite direction of the causal effects presented in the previous subsection. These results thus illustrate why a cross-sectional regression will estimate a biased effect of unionization per se on corporate financial policy.

Whereas one main advantage of this paper is the ability to cleanly identifying the causal effect of unionization, investigating the reasons why firms that do or don't get unionized by a wide margin change their financial policy in the opposite direction as the causal effect I estimate is very interesting as well. As previously discussed, employees have an incentive to form a union when their individual bargaining power is low. Employees have low bargaining power individually when the firm can replace an employee with an outside worker at low cost; in other words, when labor adjustment costs are low. This situation tends to arise when the worker has little firm-specific human capital. Firms with low labor adjustment costs should therefore be expected to have (i) high union vote shares and (ii) low cash and high leverage. However, such correlation does not correspond to a causal effect of unionization on cash and leverage, but it rather reflects a selection effect, or the

effect caused by an omitted variable, such as low firm-specific human capital.

3.4 Robustness Checks

This section provides several robustness checks. The two main dimensions are ensuring the estimates of the causal effect of unionization are driven neither by the particular choice of regression model nor by the particular index of financial constraints I use.

To ensure robustness with respect to the regression model, I run polynomial regressions of different orders on different vote-share windows, as suggested by Imbens and Lemieux [2008], see also . To illustrate, I report in Table 5 results of a linear regression (as opposed to a sixth-order polynomial regression) of the outcome variable on a union-win dummy and the vote share

$$Y_i = \alpha + \beta \cdot win_i + \gamma \cdot m_i + \varepsilon_i, \tag{5}$$

in which the sample is restricted to a window of +/- 20% around the 0% winning margin. The results, especially for unconstrained firms, are remarkably robust qualitatively and quantitatively, compared to the results obtained with a polynomial regression reported in Table 3: cash-to-assets ratios are estimated to increase by 6% as a causal effect of unionization, and net leverage is estimated to decrease by 2% for unconstrained firms. The constrained firms' response is -4.1% for cash to assets and +1.6% for net leverage. However, as in the main results, the effects for financially constrained firms are, if anything, marginally statistically significant.

Next, I investigate whether the particular index of financial-constraints risk I employ drives the results. Table 6 provides evidence to the contrary. I run a linear regression on the same set of 'close' (+/-20%) elections as in the previous test, and construct a dummy for each type of financial constraint. If an observation belongs to a firm above the median with regard to the respective measure of financial constraints, the dummy takes the value 1. Apart from the Whited index (WI), I use Kaplan and Zingales [1997]'s index of financial constraints (KZ), credit ratings (R), total assets (AT), and a dividend payer (Div) dummy. I report the interaction of the financial-constraints dummy

with the union-win dummy. The qualitative results are preserved, but also due to the smaller sample, not all specifications yield significance. In sum, the qualitative results seem robust to the choice of financial-constraint proxy. Because measures of financial constraints that less heavily correlate with size produce similar results, I conclude that size is less likely to be the only driver of heterogeneity in the causal effects but that indeed financial constraints seem to play a significant role.

The point estimates of these causal estimates do not decline when I limit the sample to elections in firms for which only one establishment has a unionization election between 1961 and 1999. However, significance is not robust when I reduce the sample so substantially, because many observations stem from large firms that have many establishments in which elections happen over the course of several years.

An alternative interpretation of the results is that barely unionized establishments are not "really" unionized in the following sense. Union support in barely unionized forms may be weak enough that the firm anticipates decertification shortly after the certification. The observed cash response would then be a sign of relief by the firm that the union "almost lost" the election, rather than an increase in precautionary cash holdings in response to increases in labor adjustment costs. Similarly, a bias results if firms that barely do not get unionized are more likely than other firms to get unionized in a potential future election. In that case, firms would take their expected unionization status into account when making present-day cash decisions. The estimated coefficients should then be regarded as a lower bound to the difference between a firm that is permanently unionized and one that is permanently not unionized. However, as previously discussed (and as already established by DL and LM), close elections do not predict future decertifications in my data set. In particular, closely "lost" elections do not predict the frequency of future elections, thereby making this channel an unlikely explanation for the results. To further alleviate this concern, I test whether excluding observations for which an election follows another one within three years changes the results, and find that it does not. As previously discussed, excluding all firms with multiple elections over the sample period reduces statistical significance (as a result of sample selection) but does not reduce

the point estimates. I conclude this hypothesis is also unlikely to drive the results.

Lastly, Roberts and Whited [2011] point out that the inclusion of linear controls in the regression specification should not change the estimated effect, but if anything increase precision. Indeed, when I include size and other known determinants of cash holdings or year-fixed effects as controls, the coefficient remains almost perfectly unchanged.

3.5 Placebo Tests

This section provides further validity checks of the RD design. In particular, I first show that variables that exhibit a discontinuity at the threshold in the above specifications do not also exhibit significant discontinuities at other, meaningless vote-share thresholds that lack an institutional underpinning and economic rationale [Kane, 2003]. In Figure 7, I run the polynomial regressions (2) with cash-to-assets as the outcome variable, but instead of allowing for a jump at the economically meaningful threshold of 50% vote share (or 0% winning margin), I allow for a discontinuity at 15%, 25%, 35%, ... , and 85% thresholds. Because there no economic reason exists for a discontinuity at any of these thresholds, observing significant jumps at one or more of them could be an indication of the instability of the research design. The graphs clearly show, however, that no significant discontinuities exist at any of the placebo thresholds. No significant discontinuities exist at placebo thresholds for the other outcome variables I used either.

Second, I show the response of cash-to-assets ratios to placebo elections at date $t-1$ and at date $t+1$, where t is the year of an actual election. Figure 8 presents firms' cash and leverage responses to such elections that did not actually take place. The change in cash-to-assets (upper graphs) and net book leverage (lower graphs), respectively, is measured a year before the actual election (left side) and a year after the actual election (right side), whereas the winning margin is the winning margin of the actual election at time t. Under the identifying assumption that firms cannot anticipate whether they will barely win or lose an election a year in advance, firms have no economic reason to change their financial policies differentially before the election takes place. The

left graphs for the $t-1$ placebo election are consistent with that notion: the financial policies of firms that will barely win or lose and election in the future are no different from each other. The graphs to the right for the $t+1$ placebo election show in addition that firms that have barely won or lost an election the year before do not change their financial policies differentially after a year after the actual election either.

3.6 Additional Outcome Variables

I present estimates of the causal effect of unionization from regression specification (2) on several outcome variables other than the previously discussed cash-to-assets ratios and financial, operating, and market leverage. In particular, I first investigate whether the increase in cash-to-assets ratios is primarily driven by increases in cash reserves or by a reduction in assets. Columns 1 and 2 of Table 7 show that the point estimate for increases in cash is larger than the point estimate for a reduction in assets, though both explanations seem to play a role. Column 3 estimates the casual effect of unionization on the ratio of EBITDA on sales. The insignificant coefficient with a point estimate close to zero indicates the profitability channel is unlikely to be the driver of the results. Column 5 of the same table shows that reductions in investment, as a fraction of sales, likely contribute to the increase of cash balances. (I normalize by sales rather than assets to enable a better comparison across industries; manufacturing relies more on tangible assets than retail.) Although a reduction of investment does not seem surprising as a causal effect of unionization, it is informative about the flexibility versus bargaining view that marginally unionized firms on average do not pay out the thus accumulated cash. Similarly, treated firms that are financially unconstrained seem to be able to reduce cash outflows by scaling down their workforce. The fact that unionization causes neither market value (column 4) nor compensation (column 7) in either subsample is consistent with the results in LM and DL, respectively. However, it should be noted that LM and DL's outcome variables are more precisely measured.

3.7 External Validity and Inherent Limitations

While regression discontinuity estimates are considered very "clean" estimates of causal effects, external validity is a general challenge to the regression-discontinuity methodology. The present paper is no exception. First, using unionization elections as an instrument implies that most establishments in my sample belong to manufacturing firms. At an econometric level, RD estimates are always weighted average treatment effects, where the weights are the ex-ante probability that the value of an individual's assignment variable will be in the neighborhood of the threshold. In other words, even within my sample, without further assumptions, the causal effect I estimate is only valid for the observations close to the vote-share threshold. Firm characteristics might exist that both explain why these observations are close to 50% and affect the true causal effect of unionization, which therefore may be different further away from the threshold. Table 8 investigates this possibility. It presents results of t-tests that test for differences in several pre-election observable characteristics between the set of firms close to the threshold and further away from the threshold. The only characteristic that is systematically related to whether a firm ends up closer to or further from the threshold is small size. As explained in section 2, is a mechanical property of the research design, which, following DL and LM, I mitigate as much as possible by excluding small elections. The fact that observable characteristics other than this mechanical one do not seem to be systematically related to whether an election is close suggests that the results could also be valid outside a narrow window around the 50% vote-share threshold. That said, the cost of a clean identification with the RD design is that a concern necessarily remains that the causal effect of unionization is different for firms with elections that do not fall in close proximity to the threshold.

4 Discussion and Conclusions

This paper provides new evidence on how the nature of a firm's labor relations affects liquidity risk management and capital structure. Specifically, I investigate firms' financial choices in response

to unionization elections. I find that the average firm that becomes unionized decreases cash and increases leverage, compared to the average firm that does not become unionized. This finding resonates with the view that firms secure a more advantageous bargaining position by reducing flexibility following a unionization event. I also find, however, that the causal effect of unionization, as measured with a regression discontinuity design, is close to zero for both cash and leverage. That means that unionization *per se* is not the driver of the average response found earlier, but that unobserved variables correlated with high union support (or the degree of union support itself) causes the response of the average firm. Interestingly, however, I find that unionization *per se* does significantly affect corporate financial policy in subsets of the data. Firms that look financially unconstrained according to standard measures appear to manage the increased fundamental riskiness arising from more rigid labor inputs with higher cash and lower leverage, i.e., they increase their financial flexibility. By contrast, financially constrained firms respond with lower cash and higher leverage. Under the interpretation that financially unconstrained firms can freely adjust their financial ratios, whereas financially constrained firms cannot, but instead follow a more passive policy, this subsample split strongly supports the view that financial flexibility is a more important determinant of corporate financial decision making for the marginal firm. I conclude that both the "bargaining" and the "financial flexibility" view are important frameworks that help understand the interaction between labor and corporate finance. Which one is relatively stronger depends on the subset of firms under investigation.

References

Ashwini K Agrawal and David A Matsa. Labor unemployment risk and corporate financing decisions. *Journal of Financial Economics*, 2012.

Jose Azar, Jean-Francois Kagy, and Martin Schmalz. Can changes in the cost of carry explain the dynamics of corporate "cash" holdings? *Working Paper*, 2015.

Carliss Y Baldwin. Productivity and labor unions: An application of the theory of self-enforcing contracts. *Journal of Business*, pages 155–185, 1983.

Thomas W Bates, Kathleen M Kahle, and René M Stulz. Why do u.s. firms hold so much more cash than they used to? *The Journal of Finance*, 64(5):1985–2021, 2009.

Efraim Benmelech, Nittai K Bergman, and Ricardo J Enriquez. Negotiating with labor under financial distress. *Review of Corporate Finance Studies*, 1(1):28–67, 2012.

Jonathan B Berk, Richard Stanton, and Josef Zechner. Human capital, bankruptcy, and capital structure. *The Journal of Finance*, 65(3):891–926, 2010.

Patrick Bolton, Hui Chen, and Neng Wang. A unified theory of tobin's q, corporate investment, financing, and risk management. *The Journal of Finance*, 66(5):1545–1578, 2011.

Daniel Bradley, Incheol Kim, and Xuan Tian. Do labor unions affect innovation? Technical report, Available at SSRN: http://ssrn.com/abstract=2232351 or http://dx.doi.org/10.2139/ssrn.2232351, 2013.

Stephen G Bronars and Donald R Deere. The threat of unionization, the use of debt, and the preservation of shareholder wealth. *The Quarterly Journal of Economics*, 106(1):231–254, 1991.

Jennifer Brown and David A Matsa. Boarding a sinking ship? an investigation of job applications to distressed firms. Technical report, National Bureau of Economic Research, 2012.

A. Caggese and V. Cuñat. Financing constraints and fixed-term employment contracts. *The Economic Journal*, 118(533):2013–2046, 2008.

Thomas J Chemmanur, Yingmei Cheng, and Tianming Zhang. Human capital, capital structure, and employee pay: An empirical analysis. *Journal of Financial Economics*, 110(2):478–502, 2013.

Huafeng J. Chen, Marcin Kacperczyk, and Hernán Ortiz-Molina. Do non-financial stakeholders affect the pricing of risky debt? evidence from unionized workers. *Review of Finance*, 16(2): 347–383, 2012.

Huafeng Jason Chen, Marcin Kacperczyk, and Hernan Ortiz-Molina. Labor unions, operating flexibility, and the cost of equity. *Journal of Financial and Quantitative Analysis*, 46(01):25–58, 2011.

Zhiyao Chen, Jarrad Harford, and Avraham Kamara. Operating inflexibility. In *Profitability and Capital Structure, EFA 2013 Cambridge Meetings Paper*, 2013.

Sudipto Dasgupta and Kunal Sengupta. Sunk investment, bargaining and choice of capital structure. *International Economic Review*, pages 203–220, 1993.

John DiNardo and David S Lee. Economic impacts of new unionization on private sector employers: 1984–2001. *The Quarterly Journal of Economics*, 119(4):1383–1441, 2004.

Andrew Ellul, Marco Pagano, and Fabiano Schivardi. Risk sharing within firms: Worldwide evidence. Technical report, 2013.

Henry S Farber. Mobility and stability: The dynamics of job change in labor markets. *Handbook of Labor Economics*, 3:2439–2483, 1999.

Joan Farre-Mensa and Alexander Ljungqvist. Do measures of financial constraints measure financial constraints? Technical report, National Bureau of Economic Research, 2013.

Richard B Freeman and James L Medoff. What do unions do. *Industrial Labor Relations Review*, 38:244, 1984.

Andrea Gamba and Alexander Triantis. The value of financial flexibility. *The Journal of Finance*, 63(5):2263–2296, 2008.

Francois Gourio. Labor leverage, firms' heterogeneous sensitivities to the business cycle, and the cross-section of expected returns. Working paper, Boston University, 2007.

John R Graham and Campbell R Harvey. The theory and practice of corporate finance: Evidence from the field. *Journal of Financial Economics*, 60(2-3):187–243, 2001.

Guido W Imbens and Thomas Lemieux. Regression discontinuity designs: A guide to practice. *Journal of Econometrics*, 142(2):615–635, 2008.

Priscilla Butt Jaggia and Anjan V Thakor. Firm-specific human capital and optimal capital structure. *International Economic Review*, pages 283–308, 1994.

Matthias Kahl, Jason Lunn, and Mattias Nilsson. Operating leverage and corporate financial policies. In *AFA 2012 Chicago Meetings Paper*, 2013.

Thomas J Kane. A quasi-experimental estimate of the impact of financial aid on college-going. *NBER Working Paper No. W9703*, 2003.

Steven N. Kaplan and Luigi Zingales. Do financing constraints explain why investment is correlated with cash flow? *Quarterly Journal of Economics*, pages 169–215, Febuary 1997.

Hyunseob Kim. Labor market size and employer capital structure: Evidence from large plant openings. Technical report, Johnson Graduate School of Management, Cornell University, 2012.

Sandy Klasa, William F Maxwell, and Hernán Ortiz-Molina. The strategic use of corporate cash holdings in collective bargaining with labor unions. *Journal of Financial Economics*, 92(3):421–442, 2009.

Olga Kuzmina. Capital structure and employment flexibility. In *Paris December 2012 Finance Meeting EUROFIDAI-AFFI Paper*, 2012.

David S Lee and Thomas Lemieux. Regression discontinuity designs in economics. *Journal of Economic Literature*, 48:281–355, June 2010.

David S Lee and Alexandre Mas. Long-run impacts of unions on firms: New evidence from financial markets, 1961–1999. *Quarterly Journal of Economics*, 127:333–378, 2012.

David A Matsa. Capital structure as a strategic variable: Evidence from collective bargaining. *The Journal of Finance*, 65(3):1197–1232, 2010.

David C Mauer and Alexander J Triantis. Interactions of corporate financing and investment decisions: A dynamic framework. *The Journal of Finance*, 49(4):1253–1277, 1994.

Justin McCrary. Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, 142(2):698–714, 2008.

Robert Novy-Marx. Operating leverage. *Review of Finance*, 15:103–134, 2011.

J. Marcelo Ochoa. Labor heterogeneity, volatility, and expected equity returns. Technical report, Duke University, 2013.

Walter Y Oi. Labor as a quasi-fixed factor. *The Journal of Political Economy*, 70(6):538–555, 1962.

Tim Opler, Lee Pinkowitz, René Stulz, and Rohan Williamson. The determinants and implications of corporate cash holdings. *Journal of Financial Economics*, 52(1):3–46, 1999.

Enrico C Perotti and Kathryn E Spier. Capital structure as a bargaining tool: The role of leverage in contract renegotiation. *The American Economic Review*, pages 1131–1141, 1993.

Ryan Pratt. A structural model of human capital and leverage. 2011.

Adriano A Rampini, Amir Sufi, and S Viswanathan. Dynamic risk management. *Journal of Financial Economics*, 111(2):271–296, 2014.

Michael Roberts and Toni M Whited. Endogeneity in corporate finance. 2011.

Oded H Sarig. The effect of leverage on bargaining with a corporation. *Financial Review*, 33(1): 1–16, 1998.

Matthew A Serfling. Firing costs and capital structure decisions. *Available at SSRN 2396599*, 2014.

Anil Shivdasani and Irina Stefanescu. How do pensions affect corporate capital structure decisions? *Review of Financial Studies*, 23(3):1287, 2010.

Elena Simintzi, Vikrant Vig, and Paolo Volpin. Labor protection and leverage. *Review of Financial Studies*, 28(2):561–591, 2015.

Aaron J Sojourner, Robert J Town, David C Grabowski, and Michelle M Chen. Impacts of unionization on employment, product quality and productivity: Regression discontinuity evidence from nursing homes. Technical report, National Bureau of Economic Research, 2012.

David Sraer and David Thesmar. Performance and behavior of family firms: Evidence from the french stock market. *Journal of the European Economic Association*, 5(4):709–751, 2007.

Hiroki Tabuchi. Former goldman employees form union in japan. http://dealbook.nytimes.com/2012/03/22/former-goldman-employees-form-union-in-japan/ April 18, 2013, New York Times, March 2012.

Sheridan Titman. The effect of capital structure on a firm's liquidation decision. *Journal of Financial Economics*, 13(1):137–151, 1984.

Toni M Whited and Guojun Wu. Financial constraints risk. *Review of Financial Studies*, 19(2): 531–559, 2006.

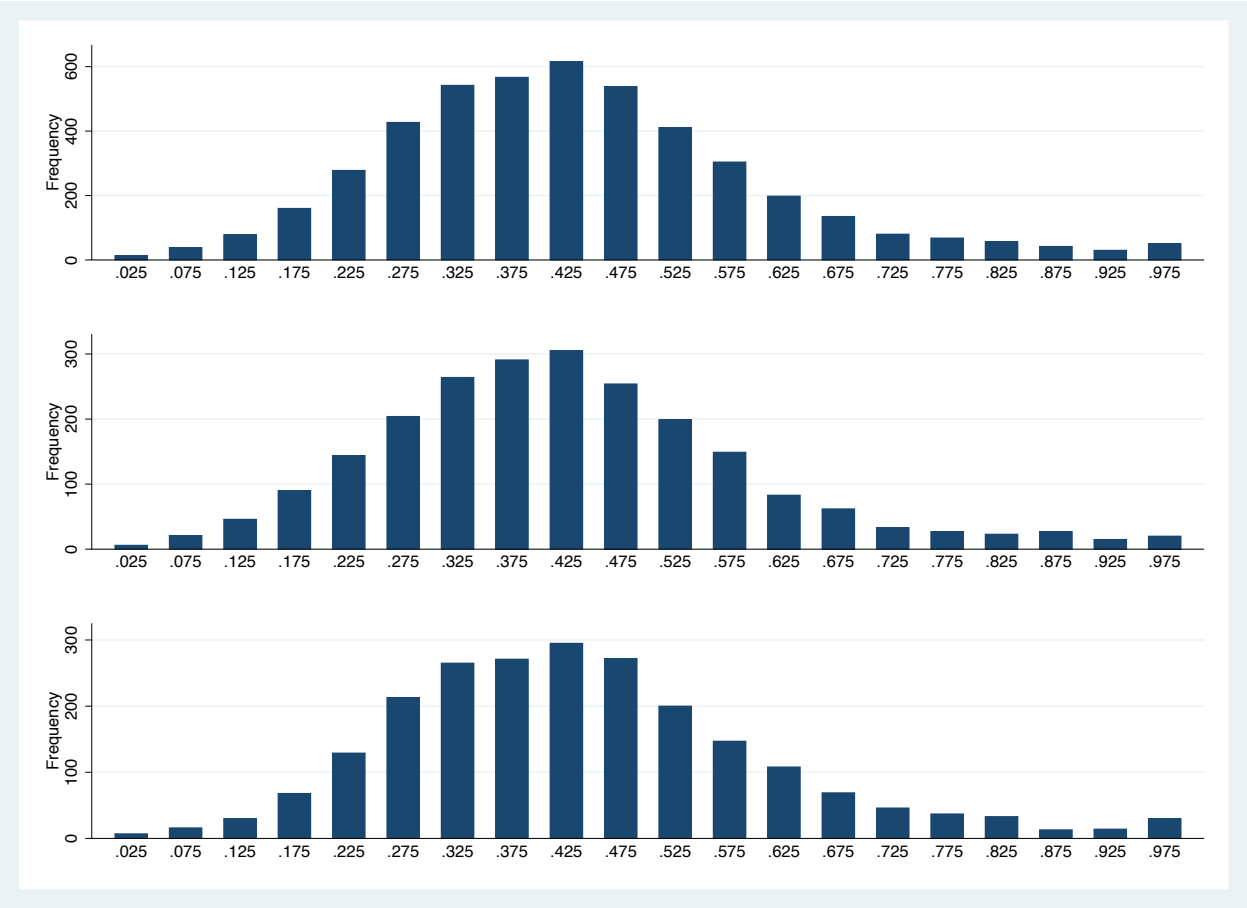

Figure 1: Vote-share distributions for the full sample (top), unconstrained firms (center), and constrained firms (bottom). The 20 vote-share bins are equally spaced and constructed as in Lee and Mas [2012]. The unconstrained/constrained split is at the sample median of the Whited-Wu financial-constraints index. An identifying assumption of the RD design is that no party can precisely control the election outcome just to the right and left of the 50% vote-share threshold. In other words, there must not be systematic sorting of firms, or subsets of firms within close proximity to that threshold. Such sorting would be visible from a discontinuity in the vote-share distribution at the 50% vote-share threshold. (Only quasi-continuity of the forcing variable's distribution is required, but not equal probabilities just right and left of the threshold.)

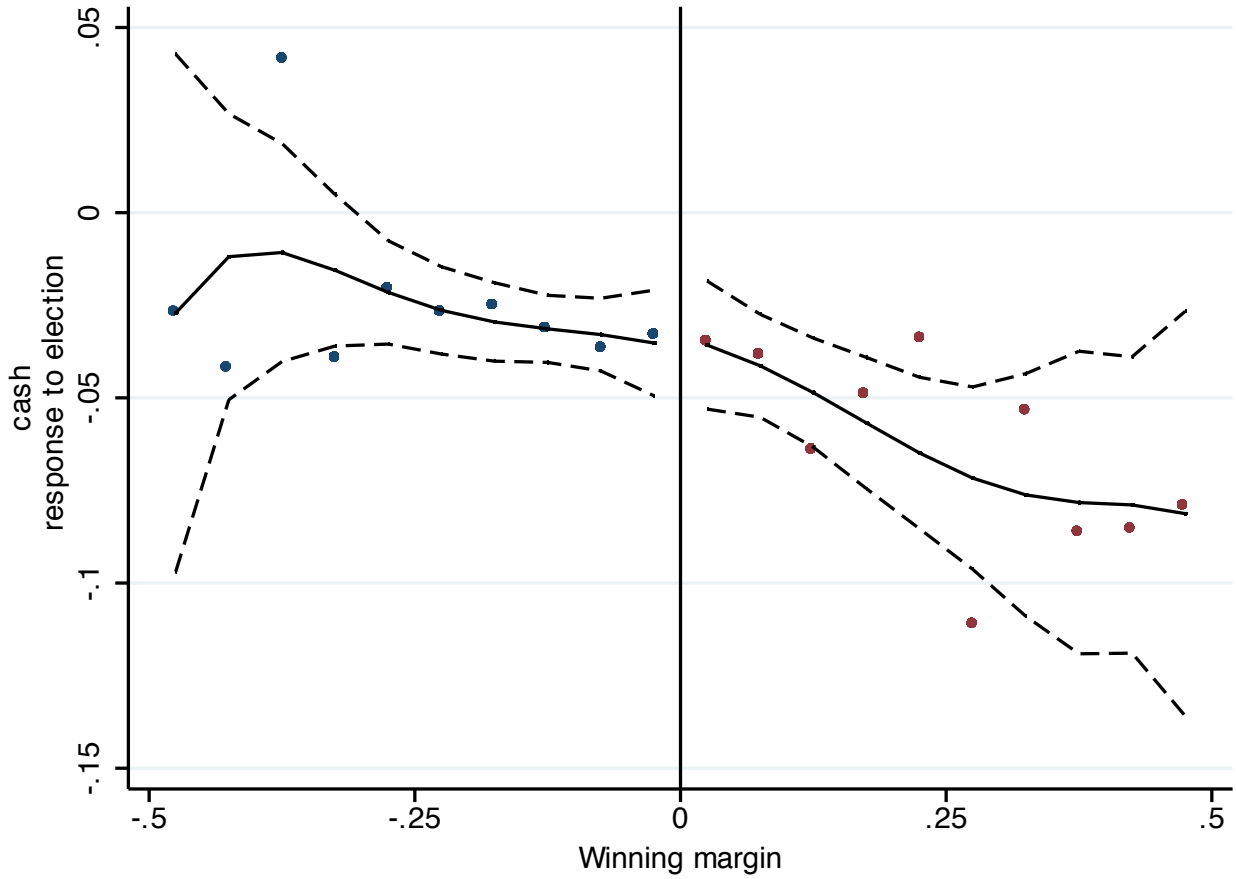

Figure 2: Unionization election response of cash-to-asset ratios ("cash"= cash divided by total assets). The dots depict the average cash-to-asset ratio within the vote share bins introduced in Figure 1. Winning margin is vote share minus 50%. The right hemispheres of the graphs show the response of firms in which a union gets established. The left hemisphere has the response of firms in which no union gets established. The solid line represents predicted values based on a sixth-order polynomial regression; the dotted lines are 95% confidence intervals. Standard errors are clustered at the firm level. Any discontinuity of the outcome variable at the 0%-winning margin threshold is the estimated causal effect of unionization.

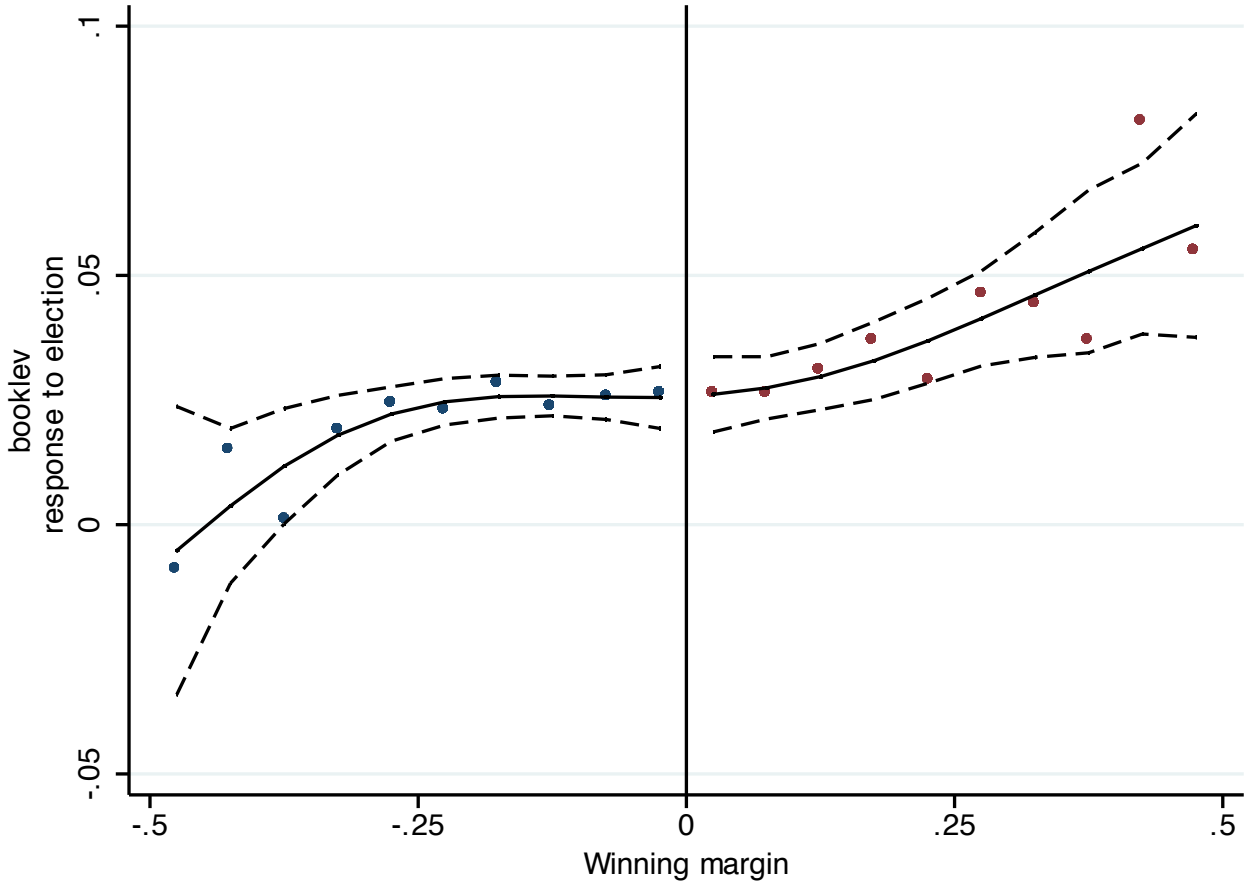

Figure 3: Unionization election response of net book leverage ratios. The dots depict the average cash-to-asset ratio within the vote share bins introduced in Figure 1.Winning margin is vote share minus 50%. The right hemispheres of the graphs show the response of firms in which a union gets established. The left hemisphere has the response of firms in which no union gets established. The solid line represents predicted values based on a sixth-order polynomial regression; the dotted lines are 95% confidence intervals. Standard errors are clustered at the firm level. A discontinuity of the outcome variable at the 0%-winning margin threshold is the estimated causal effect of unionization.

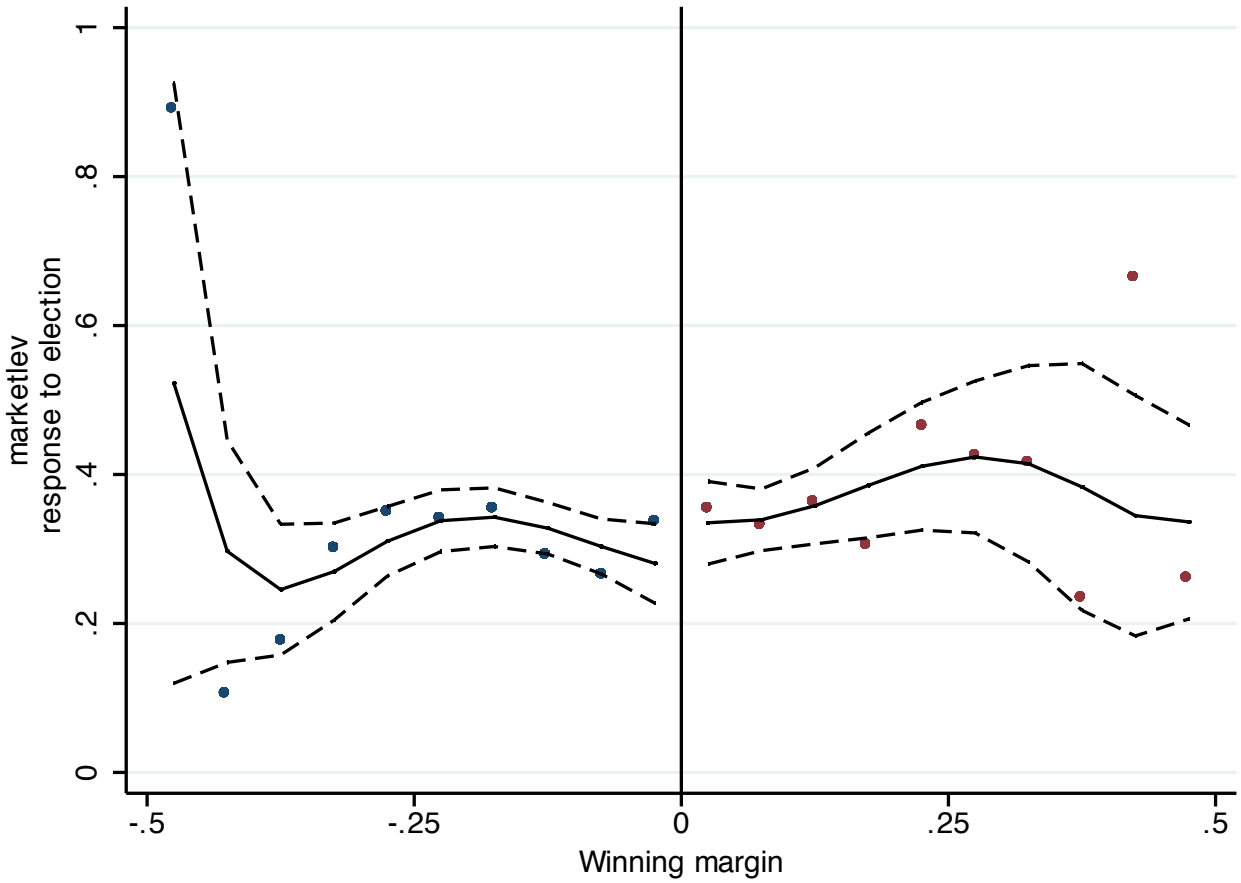

Figure 4: Unionization election response of market leverage ratios. The dots depict the average value of the outcome variable within the vote share bins introduced in Figure 1. Winning margin is vote share minus 50%. The right hemispheres of the graphs show the response of firms in which a union gets established. The left hemisphere has the response of firms in which no union gets established. The solid line represents predicted values based on a sixth-order polynomial regression; the dotted lines are 95% confidence intervals. Standard errors are clustered at the firm level. A discontinuity of the outcome variable at the 0%-winning margin threshold is the estimated causal effect of unionization.

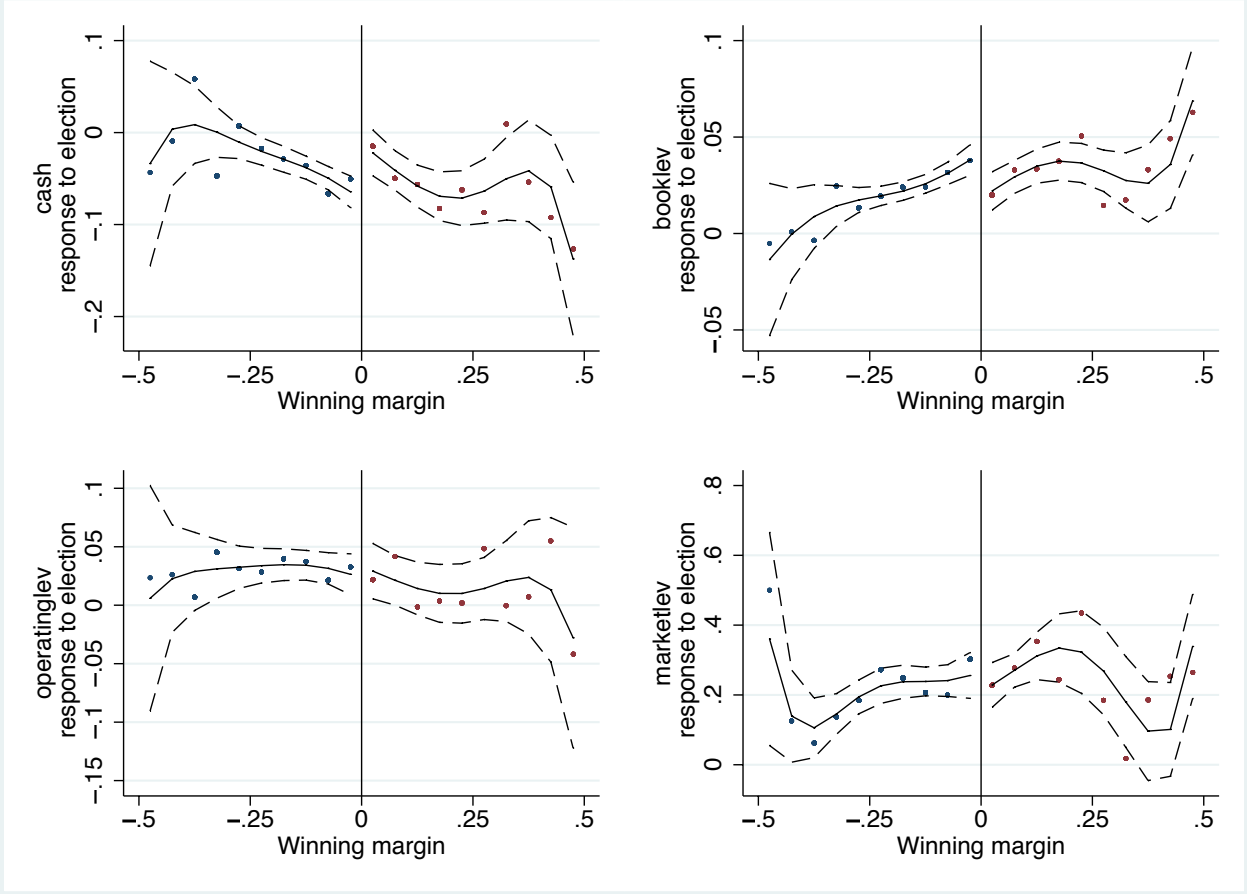

Figure 5: Election response of financially unconstrained firms' cash-to-asset ratios ("cash"= cash divided by total assets), net book leverage ("booklev" = debt minus cash divided by total assets), operating leverage ("operatinglev", defined as in Novy-Marx [2011]), and market leverage ("marketlev" = total liabilities / (market value of equity + total liabilities)). "Unconstrained" means being below the sample median according to the Whited-Wu index. The dots depict the average value of the outcome variable within the vote share bins introduced in Figure 1. Winning margin is vote share minus 50%. The right hemispheres of the graphs show the response of firms in which a union gets established. The left hemisphere has the response of firms in which no union gets established. The solid line represents predicted values based on a sixth-order polynomial regression; the dotted lines are 95% confidence intervals. Standard errors are clustered at the firm level. A discontinuity of the outcome variable at the 0%-winning margin threshold is the estimated causal effect of unionization.

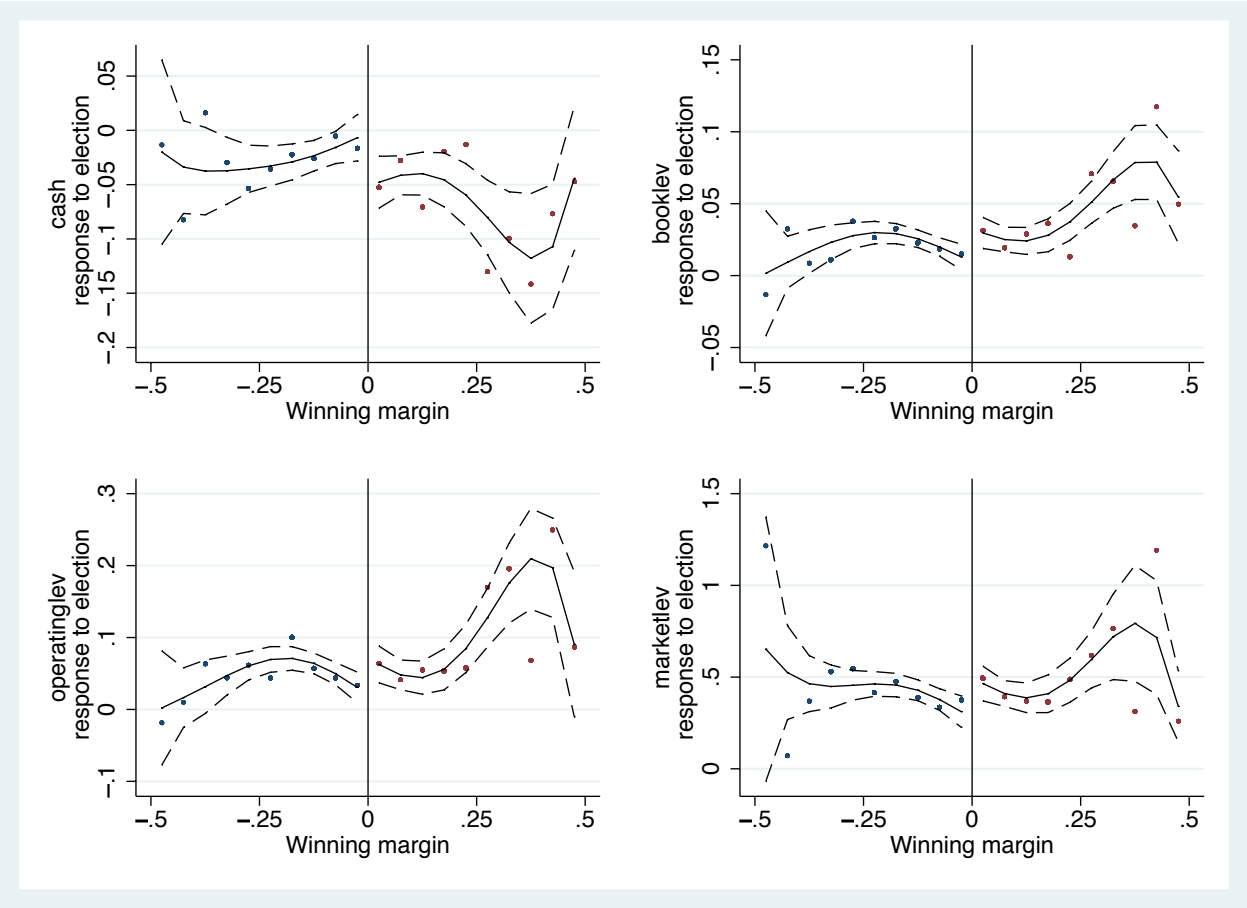

Figure 6: Election response of financially constrained firms' cash-to-assets ratios ("cash"= cash divided by total assets), net book leverage ("booklev" = debt minus cash divided by total assets), operating leverage ("operatinglev," defined as in Novy-Marx [2011]), and market leverage ("marketlev" = total liabilities / (market value of equity + total liabilities)). "Constrained" means being above the sample median according to the Whited-Wu index. The dots depict the average value of the outcome variable within the vote-share bins introduced in Figure 1. Winning margin is vote share minus 50%. The right hemispheres of the graphs show the response of firms in which a union is established. The left hemisphere has the response of firms in which no union is established. The solid line represents predicted values based on a sixth-order polynomial regression; the dotted lines are 95% confidence intervals. Standard errors are clustered at the firm level. A discontinuity of the outcome variable at the 0%-winning-margin threshold is the estimated causal effect of unionization.

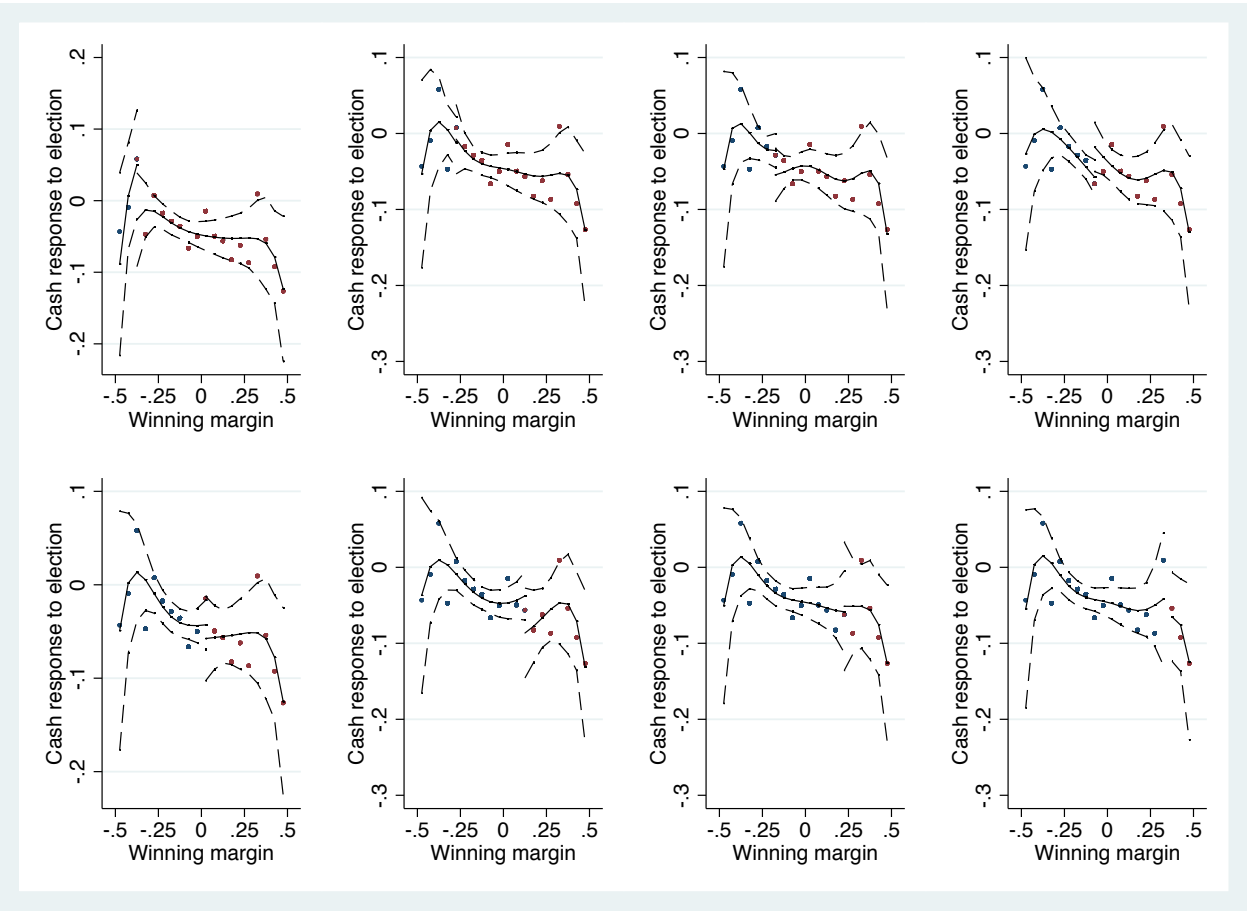

Figure 7: Changes of financially unconstrained firms' cash-to-assets ratios ("cash"= cash divided by total assets) with meaningless "placebo" winning thresholds of 15%, 25%, 35%, 45%, 55%, 65%, 75%, 85% (left to right, top to bottom). "Unconstrained" means being below the sample median according to the Whited-Wu index. The dots depict the average value of cash-to-assets ratios of firms falling in the vote-share bins introduced in Figure 1. Winning margin is defined as vote share minus 50%. A union is established if and only if the winning margin is positive. The solid line represents predicted values based on a sixth-order polynomial regression; the dotted lines are 95% confidence intervals. Standard errors are clustered at the firm level. The regression specification allows the line to jump at the above-described thresholds, instead of at the true winning threshold of 50% for which the results are presented in Figure 5. No economic reason exists for firms left or right of the placebo thresholds to respond differently to an election. Therefore, observing no jumps in any of the above graphs supports the validity of the research design.

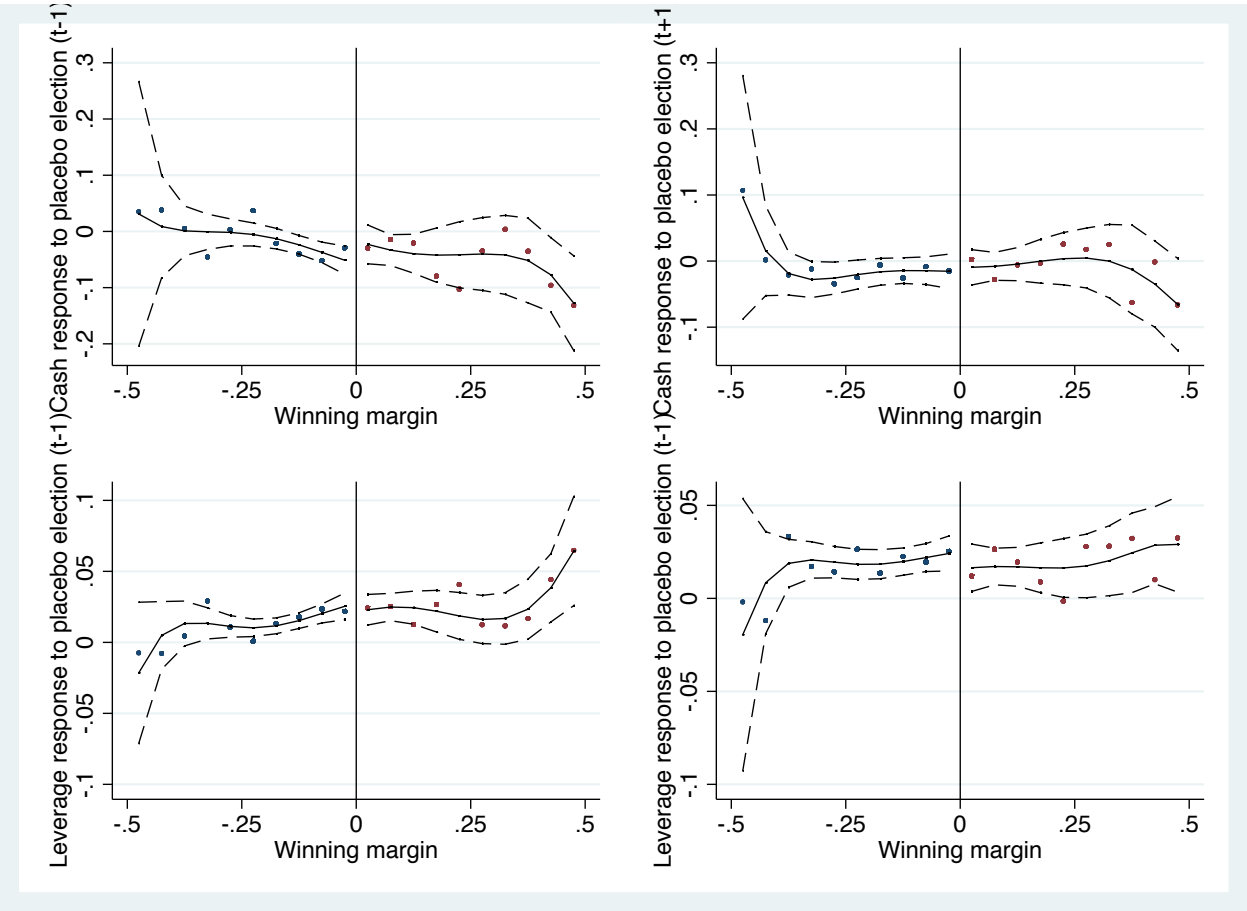

Figure 8: Response of financially unconstrained firms' cash-to-assets ratios ("cash"= cash divided by total assets) and net book leverage ("booklev" = debt minus cash divided by total assets) to "placebo" elections a year before or after the actual election. "Unconstrained" means being below the sample median according to the Whited-Wu index. The dots depict the average value of cash-to-assets ratios and net leverage of firms falling in the vote-share bins introduced in Figure 1. Winning margin is defined as vote share minus 50%. A union is established if and only if the winning margin is positive. The solid line represents predicted values based on a sixth-order polynomial regression; the dotted lines are 95% confidence intervals. Standard errors are clustered at the firm level. The regression specification allows for a discontinuity at a winning margin of 0. The outcome variable is measured not at the actual election date, but a year before and a year later, respectively. No economic reason exists for firms left or right of the 50% vote-share threshold to respond differently to a placebo election, that is, an election that did not take place. Therefore, observing no jumps in the above graphs supports the validity of the research design.

Table 1: Summary statistics for key variables for the full sample, 1961-1999, in thousands. "Unconstrained" ("constrained") means being in the lower (upper) half of the sample according to the Whited-Wu index of financial constraints. The abbreviations mean cash-to-asset ratio (c2a), net leverage (net lev), operating leverage (ol), and market leverage (mlev). The number of observations indicates the number of election observations with a complete set of Compustat variables.

All firms (N=3286)	mean	p50	sd	min	max
Total Assets	3649.912	833.397	10535.34	3.353	217123.4
Cash	223.9463	36.0025	673.3574	0	11044.3
Total Liabilities	2141.197	380.9935	7617.233	1.06	193777.9
Net Liabilities	1917.251	325.4515	7235.778	-206.184	182733.6
Book Equity	1479.486	418.551	3457.317	2.293	43542
Market Cap	2809.706	578.7755	7537.473	.51525	130562.3
Employees	62.06484	24	118.1942	.069	853
Cash-to-Assets	.0670297	.0462282	.0626051	0	.5488421
Net Leverage	.4205393	.433251	.1591972	-.2113108	.9144532
Operating Leverage	2.777399	2.418946	1.541548	.2783285	10.4073
Market Leverage	1.12717	.7285635	1.413714	.0240333	25.02227
Δc2a	-.0346486	-.0346303	.2209359	-1.024753	1.044239
Δnet lev	.0257099	.0243249	.0869404	-.4146557	.4324271
Δol	.0405121	.0334791	.1912406	-.949031	1.026049
Δmlev	.3321717	.1426453	.7692133	-.9513371	3.960541

Unconstrained (N=1804)	mean	p50	sd	min	max
Total Assets	6157.154	2073.047	13523.7	229.478	217123.4
Cash	376.2023	102.457	849.0552	0	11044.3
Total Liabilities	3606.023	1032.79	9935.108	46.643	193777.9
Net Liabilities	3229.821	877.107	9478.652	-206.184	182733.6
Book Equity	2499.784	1003.646	4319.37	167.132	43542
Market Cap	4853.042	1649.971	9597.73	104.247	130562.3
Employees	101.5764	48	143.4538	.75	853
Cash-to-Assets	.0686449	.0497468	.0615058	0	.4696362
Net Leverage	.4275859	.4382541	.1491718	-.175484	.8450245
Operating Leverage	2.561028	2.280039	1.352355	.302783	10.28264
Market Leverage	.8742799	.6598065	.8564128	.0240333	12.48054
Δc2a	-.0416534	-.0403861	.2170207	-.9403229	1.044239
Δnet lev	.0266018	.0246825	.0807946	-.3134976	.4010054
Δol	.0271484	.0307361	.1667665	-.8634019	.9647384
Δmlev	.2451178	.1155684	.635545	-.8831711	3.783424

Constrained (N=1480)	mean	p50	sd	min	max
Total Assets	597.9977	164.546	2563.175	3.353	38024
Cash	38.60879	7.2665	256.0329	0	5149
Total Liabilities	358.4299	70.9075	1655.312	1.06	25071
Net Liabilities	319.8211	60.393	1449.949	-23.715	19922
Book Equity	237.2594	85.782	994.6373	2.293	17814.6
Market Cap	321.9309	94.32464	1612.464	.51525	47639.19
Employees	13.96464	6.18	41.46172	.069	746
Cash-to-Assets	.0650536	.0429945	.0639261	0	.5488421
Net Leverage	.412323	.4272111	.1700965	-.2113108	.9144532
Operating Leverage	3.043607	2.597468	1.707662	.2783285	10.4073
Market Leverage	1.436713	.8756214	1.835705	.0405119	25.02227
Δc2a	-.0258859	-.0270909	.2254506	-0.944753	.9374832
Δnet lev	.0244402	.0235058	.0938262	-.4146557	.4324271
Δol	.0562991	.035959	.2160428	-.949031	1.026049
Δmlev	.4378143	.1950019	.8951082	-.9513371	3.960541

Table 2: t-test results for pre-election differences in key characteristics of observations just to the left and just to the right (+/-5%; +/-10%; +/-15%) of the 50% vote-share threshold. The abbreviations correspond to those in Table 1. The null hypothesis of no observable differences in key variables cannot be rejected, as is necessary for the identifying assumption to hold that no systematic manipulation of close elections exists.

Panel A: Unconstrained firms	c2a	net lev	ol	mlev	cash	assets	market cap
[-5%, -0%] vs. (0%, 5%] winning margin	0.17	-1.08	-0.62	-1.37	-0.07	-1.35	-0.97
[-10%, -0%] vs. (0%, 10%] winning margin	0.91	-0.73	-1.35	-0.47	-0.30	1.76	0.73
[-15%, -0%] vs. (0%, 15%] winning margin	1.31	-0.45	-0.28	0.77	0.39	-1.40	-0.39

Panel B: Constrained firms	c2a	net lev	ol	mlev	cash	assets	market cap
[-5%, -0%] vs. (0%, 5%] winning margin	0.06	-1.38	-0.99	-1.19	-0.29	-0.92	0.24
[-10%, -0%] vs. (0%, 10%] winning margin	0.34	-0.56	-0.96	-0.21	0.07	-0.56	-0.21
[-15%, -0%] vs. (0%, 15%] winning margin	0.49	-0.44	-0.21	0.91	0.13	-0.60	-0.58

Panel C: Full sample	c2a	net lev	ol	mlev	cash	assets	market cap
[-5%, -0%] vs. (0%, 5%] winning margin	0.38	-0.72	-0.89	-1.55	-0.07	-0.92	-0.85
[-10%, -0%] vs. (0%, 10%] winning margin	0.72	-1.07	-1.90	-0.74	-0.47	-1.41	-0.63
[-15%, -0%] vs. (0%, 15%] winning margin	0.59	0.06	-0.40	-0.38	0.49	-0.84	0.12

Table 3: Causal effect of unionization on changes of cash-to-assets ratio (c2a), net leverage (net lev), operating leverage (ol), and market leverage (mlev). The tables report coefficients on the union-win dummy from regressions of financial outcome variables on a sixth-order polynomial of the winning margin and on a *win* dummy, as well as corresponding p-values. "Unconstrained" means being in the lower half of the sample according to the Whited-Wu index of financial constraints; the definition of "constrained" is symmetric. Standard errors are clustered at the firm level.

Panel A: Unconstrained firms	Δ(c2a)	Δ(net lev)	Δ(ol)	Δ(mlev)
Union-win	0.0603***	-0.0237***	0.00985	-0.0574
	(0.0222)	(0.00843)	(0.0204)	(0.0797)
Observations	2,243	2,243	1,967	2,138

Panel B: Constrained firms	Δ(c2a)	Δ(net lev)	Δ(ol)	Δ(mlev)
Union-win	-0.0498*	0.0232**	0.0515*	0.222**
	(0.0270)	(0.0112)	(0.0266)	(0.111)
Observations	2,238	2,169	1,872	1,899

Panel C: Full sample	Δ(c2a)	Δ(net lev)	Δ(ol)	Δ(mlev)
Union-win	0.00320	0.000168	0.0261	0.0663
	(0.0178)	(0.00719)	(0.0167)	(0.0660)
Observations	4,489	4,416	3,845	4,040

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Non-causal correlations between unionization and changes of cash-to-assets ratio (c2a), net leverage (net lev), operating leverage (ol), and market leverage (mlev). OLS regression of the outcome variable on a union-win dummy, with the slope constrained to zero. The definition of "(un-)constrained" is according to Whited-Wu, as before. Standard errors are clustered at the firm level.

Panel A: Unconstrained firms	Δ(c2a)	Δ(net lev)	Δ(ol)	Δ(mlev)
Union-win	-0.0121	0.00528	-0.0125	0.0322
	(0.0118)	(0.00416)	(0.0119)	(0.0308)
Observations	2,243	2,243	1,967	2,138

Panel B: Constrained firms	Δ(c2a)	Δ(net lev)	Δ(ol)	Δ(mlev)
Union-win	-0.0290***	0.0106**	0.0180	0.0436
	(0.0107)	(0.00481)	(0.0127)	(0.0445)
Observations	2,238	2,169	1,872	1,899

Panel C: Full sample	Δ(c2a)	Δ(net lev)	Δ(ol)	Δ(mlev)
Union-win	-0.0210***	0.00825**	0.00406	0.0437
	(0.00770)	(0.00324)	(0.00872)	(0.0266)
Observations	4,489	4,416	3,845	4,040

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Causal effect of unionization on corporate finance variables from linear regressions on the winning margin and win dummy, with data within 20% of the winning threshold. All definitions are as in Table 3. The table shows that the results from Table 3 are robust to the choice of regression model.

Panel A: Unconstrained firms	Δ(c2a)	Δ(net lev)	Δ(ol)	Δ(mlev)
Union-win	0.0584***	-0.0201***	0.00899	-0.0372
	(0.0200)	(0.00759)	(0.0188)	(0.0724)
Observations	1,594	1,594	1,391	1,515

Panel B: Constrained firms	Δ(c2a)	Δ(net lev)	Δ(ol)	Δ(mlev)
Union-win	-0.0418*	0.0160	0.0441*	0.182*
	(0.0242)	(0.0105)	(0.0249)	(0.0975)
Observations	1,611	1,559	1,342	1,353

Panel C: Full sample	Δ(c2a)	Δ(net lev)	Δ(ol)	Δ(mlev)
Union-win	0.00686	-0.00144	0.0251	0.0658
	(0.0161)	(0.00657)	(0.0156)	(0.0593)
Observations	3,210	3,155	2,737	2,869

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Causal effect of unionization on cash-to-assets (c2a) and net leverage (net lev) of unconstrained and constrained firms for different measures of financial constraints. The dummy D takes value 1 if the firm falls in the upper half of the respective measure. OLS regressions on a union-"win" dummy, union-"win" dummy interacted with constraint-dummy, winning margin, and all interactions. Sample restricted to elections within 20% of the threshold. Standard errors clustered at the firm level. Kaplan-Zingales (KZ-D), ratings (R-D), and Whited-Wu (WI-D) dummies are 1 if more likely to be constrained; total assets (AT-D) and dividend (Div-D) dummies are 1 if less likely to be constrained according to the respective measure.

Outcome	Δ(c2a)	Δ(net lev)	Δ(c2a)	Δ(net lev)	Δ(c2a)	Δ(net lev)	Δ(c2a)	Δ(net lev)	Δ(c2a)	Δ(net lev)
win	0.0379*	-0.0107	0.0738	-0.0408	0.0584***	-0.0201***	-0.0338	0.0121	-0.113	0.0739**
	(0.0206)	(0.00718)	(0.0562)	(0.0274)	(0.0200)	(0.00759)	(0.0730)	(0.0371)		
KZ-D· win	-0.0559*	0.0173								
	(0.0299)	(0.0123)								
R-D· win			-0.0696	0.0426						
			(0.0581)	(0.0282)						
WI-D· win					-0.102***	0.0377***				
					(0.0316)	(0.0131)				
AT-D· win							0.0801***	-0.0265**		
							(0.0305)	(0.0128)		
Div-D· win									0.129*	-0.0816**
									(0.0750)	(0.0377)
Observations	3,209	3,154	3,195	3,140	3,209	3,154	3,208	3,153	3,209	3,154

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Causal effect of unionization on changes of cash (che), total assets (at), profitability (profit = EBITDA/sales), market value (mval), capital expenditures (capx = capex/sales), number of employees (emp), and cost of labor (comp = total staff expense / sales). The tables report coefficients on the union-win dummy from sixth-order polynomial regressions on the winning margin and win dummy, as well as corresponding p-values. "Unconstrained" means being in the lower half of the sample according to the Whited-Wu index of financial constraints; the definition of "constrained" is symmetric. Standard errors are clustered at the firm level.

Panel A: Unconstrained firms	Δ(che)	Δ(at)	Δ(profit)	Δ(mval)	Δ(capx)	Δ(emp)	Δ(comp)
Union-win	0.0754	-0.0317	-0.000677	-0.0242	-0.0295**	-0.0454**	0.0120
	(0.0665)	(0.0229)	(0.00192)	(0.0451)	(0.0137)	(0.0210)	(0.00834)
Observations	2,241	2,247	2,237	2,146	2,228	2,248	1,129
Panel B: Constrained firms	**Δ(che)**	**Δ(at)**	**Δ(profit)**	**Δ(mval)**	**Δ(capx)**	**Δ(emp)**	**Δ(comp)**
Union-win	-0.123	0.0208	0.000508	-0.0629	0.0213	0.0113	0.0107
	(0.0759)	(0.0261)	(0.00224)	(0.0724)	(0.0170)	(0.0265)	(0.0115)
Observations	2,243	2,253	1,979	2,182	2,182	2,176	561
Panel C: Full sample	**Δ(che)**	**Δ(at)**	**Δ(profit)**	**Δ(mval)**	**Δ(capx)**	**Δ(emp)**	**Δ(comp)**
Union-win	-0.0281	-0.00508	-4.91e-05	-0.0363	-0.00329	-0.0193	0.0114*
	(0.0178)	(0.00719)	(0.0167)	(0.0660)	(0.0111)	(0.0176)	(0.00640)
Observations	4,492	4,512	4,470	4,139	4,417	4,434	1,694

*** p<0.01, ** p<0.05, * p<0.1

Table 8: t-test results for pre-election differences in key characteristics of observations close (+/-5%; +/-10%; +/-15%) to the 50% vote-share threshold, compared to observations of firms that come up for election but end up far away from the threshold. The abbreviations correspond to those in Table 1.

Panel A: unconstrained subsample	c2a	net lev	ol	mlev	cash	assets	market cap
[-5%, +5%] vs. [-50%,-5%) or (+5%, +50%] winning margin	0.54	0.48	0.34	0.03	-0.07	-2.66	-0.43
[-10%, +10%] vs. [-50%,-10%) or (+10%, +50%] winning margin	0.22	0.43	-0.70	0.88	0.31	-1.97	-0.74
[-15%, +15%] vs. [-50%,-15%) or (+15%, +50%] winning margin	0.65	-0.29	0.42	-0.60	-0.13	-2.15	-0.95

Panel B: constrained subsample	c2a	net lev	ol	mlev	cash	assets	market cap
[-5%, +5%] vs. [-50%,-5%) or (+5%, +50%] winning margin	-1.51	0.61	-0.13	0.50	-1.26	-0.33	-0.43
[-10%, +10%] vs. [-50%,-10%) or (+10%, +50%] winning margin	-0.99	1.27	-0.15	1.31	-0.36	-0.81	-0.60
[-15%, +15%] vs. [-50%,-15%) or (+15%, +50%] winning margin	-1.13	1.20	0.02	-0.35	-0.92	-1.00	0.10

Panel C: full sample	c2a	net lev	ol	mlev	cash	assets	market cap
[-5%, +5%] vs. [-50%,-5%) or (+5%, +50%] winning margin	1.60	-0.24	1.11	-0.81	-0.79	-2.93	-0.47
[-10%, +10%] vs. [-50%,-10%) or (+10%, +50%] winning margin	1.78	-0.22	0.96	-0.57	0.17	-2.07	-0.57
[-15%, +15%] vs. [-50%,-15%) or (+15%, +50%] winning margin	2.58	-1.54	-0.03	-1.81	-0.04	-2.09	-0.70