Labor unions and payout policy: A regression discontinuity analysis

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Abstract

We study the causal effect of labor unions on corporate payout policy by using a regression discontinuity (RD) design. Passing a union election leads to an 8.7% lower dividend ratio and a 17.9% lower total payout ratio (including both dividends and share repurchases) than failing an election in the following year. The negative effect of unions on payout is absent in firms located in states with right-to-work laws but without work stoppage provisions. Operating flexibility appears to be a plausible underlying mechanism through which union power influences corporate payout, and firms use the saved earnings from reductions in payout to invest in net working capital rather than paying off debt or increasing cash holdings. Our paper sheds new light on the determinants of firm payout policy and the role of labor unions in corporate finance decisions.

Keywords: Payout policy; operating flexibility; labor unions; regression discontinuity design

JEL Classification: G35; G31; J51

1. Introduction

What determines a firm's payout policy? Since Miller and Modigliani (1961), financial economists have proposed numerous economic factors that determine a firm's payout policy including corporate taxes, signaling concerns, agency considerations, compensation practices, management incentives, and behavioral biases. However, very few studies have analyzed the role played by a firm's employees, one of the most important groups of stakeholders that both contribute to and get affected by firm operations, in shaping its payout decisions. This gap is surprising, given the large body of literature examining the relation between labor (as well as human capital) and other corporate policies such as capital structure (e.g., Titman, 1984; Titman and Wessels, 1988; Berk et al., 2010; Chemmanur et al., 2013, etc.) and innovation (Acharya et al., 2013, 2014).

As a claimant to firms' resources, workers generally compete with shareholders in extracting economic rents created by the business and thus prefer to retain cash flows within the firms as opposed to paying them out.[1] Understanding this motive, shareholders and the management would design payout policies to maximize their own benefits.[2] On the one hand, managers would want to keep a low payout ratio to cater to the preference of their employees so as to minimize the disruptive effect that an unhappy workforce might have on firms' operations. On the other hand, they would want to set a high payout ratio to avoid leaving resources behind for labor's rent extraction. The optimal payout policy therefore reflects the above tradeoffs and crucially depends on how labor influences firms' operations and decision making. Since unions represent a prominent form of organized labor, we study the influence of labor on corporate payouts from the perspective of a firm's union elections, and develop two competing hypotheses regarding the effect of labor unions on payout policy.

[1] A recent anecdote provides a good example in support of this argument: GM announced a dividend cut in February 2006 due to its persistent poor performance in the past few years since 2003. One important underlying force for the dividend cut was to get more concessions from the United Auto Workers, the union that represents GM's employees, because "everyone needs to share the pain, including shareholders." (Dow Jones News Service, February 7, 2006)

[2] It might appear that the above rent-seeking argument assumes that total firm value (i.e., the "size of the pie") is fixed and unaffected by corporate payout, whereas in reality employees may realize that a change in payout policy can affect firm profitability and value, which they care about because they are not only competing with shareholders for corporate resources but also cooperating with shareholders in generating profits and firm value. However, even if employees understand the consequences of payout policy for firm profitability (and the size of the pie), to the extent that employees care more about their own benefits, they would still prefer a lower (not a higher) payout than what the shareholders would optimally choose.

Our first hypothesis conjectures that labor unions reduce a firm's payout. To reduce cash flow risk and make their operations more flexible, firms need to increase the variable component of their cost structure, which entails linking their labor usage and expenses (salaries and other employee compensation) to sales revenue.[3] However, labor unions have a reputation for making wages sticky and layoffs costly, thereby increasing the adjustment cost of a firm's labor stock. Furthermore, unions frequently intervene in a firm's restructuring activities, such as blocking plant closures to save workers' jobs, which makes it harder for the firm to adjust its physical capital. As a result, unions will increase a firm's cash flow risk and reduce its operating flexibility.[4] In response to the threat from enhanced labor power, firms may decrease their payouts to accumulate precautionary cash to hedge against cash flow risk and to save for profitable investment opportunities. Further, labor unions often bargain with their employers for higher and more stable wages and better working conditions, putting an additional financial constraint on firm managers when they make corporate decisions.[5] As a result, an increase in labor power might tighten up a firm's financial situation, prompting it to cut down payouts to shareholders so as to free up some internal retained earnings for future investment use. Hence, the first hypothesis we propose, the *flexibility hypothesis*, argues that firms would cut their payouts after labor power increases.

An alternative hypothesis makes the opposite prediction. Firms can take a variety of strategic actions to improve their bargaining position against their workers. For example, DeAngelo and DeAngelo (1991) argue that, to gain concessions from labor unions, managers must create a credible perception that the firm's competitiveness is threatened by current economic conditions. Consistent with this argument, they show that unionized firms manage their earnings downward to create this perception prior to labor negotiations. Moreover, Bronars and Deere (1991), Hanka (1998), and Matsa (2010) find that issuing more debt helps firms to improve their bargaining position against workers, supporting the idea that by committing

[3] Consistent with this idea, Giroud and Mueller (2015) find that high leverage firms would decrease their work force more than low-leverage firms in the face of household demand shocks. By the same token, Kovenock and Phillips (1997) find that firms with greater debt are more likely to close plants, and Hanka (1998) shows that higher debt is associated with more frequent employment reductions, lower wages, and reduced pension funding.

[4] Chen et al. (2011a) show that reduced operating flexibility due to labor unions increases a firm's cost of equity.

[5] Using a RD design and the U.S. Census data, Frandsen (2012) finds that the passage of unionization elections in an establishment has a positive, causal effect on the wages of its employees, especially those of lower-income employees.

themselves to paying out a large portion of future cash flows to lenders, firms can effectively reduce their cash flows available for labor expropriation. Klasa et al. (2009) show that firms in more unionized industries strategically hold less cash to gain more bargaining advantages over labor unions, suggesting that a tighter cash position allows a firm to shelter its corporate income from unions' demands. Following the similar logic discussed above, since dividends enable firms to shield their cash flows from union demands, firms would prefer to commit to a high payout level in response to enhanced labor power. Therefore, our second hypothesis, the *bargaining hypothesis*, argues that firms would increase their payouts after labor power increases.[6]

While there might be an element of truth in both hypotheses, in practice it is difficult to identify the causal effect of labor unions on payout policy because of the endogeneity problem. The existence of labor unions in a firm could be correlated with its unobservable characteristics that affect the payout policy (the omitted variable concern). Alternatively, workers in a firm with reduced permanent cash flows and hence lower expected payouts may be more likely to form unions to protect their interests (the reverse causality concern). Both problems could make it very difficult to draw causal inferences from a standard ordinary least squares (OLS) framework that regresses a firm's payout ratio on its unionization status.

To overcome the endogeneity problem and establish causality, we rely on union elections that substantially alter labor power. We collect data on union election results from the National Labor Relations Board (NLRB), which allows us to compare changes in payout for firms that elect to become unionized to those that vote against it. Our main identification strategy is to use a regression discontinuity (RD) design that relies on "locally" exogenous variation in union status generated by these elections that pass or fail by a small margin of votes. This approach compares firms' payout subsequent to union elections that barely pass with that subsequent to elections that barely fail. The RD design is a powerful and appealing identification strategy because for these close-call elections, passing is very close to an independent, random event and therefore is unlikely to be correlated with firm unobservable characteristics.

[6] Note that an alternative interpretation of the bargaining hypothesis may actually predict the opposite, i.e., a negative effect of labor unions on corporate payout: in order to bargain well and get more concessions from unions, firm managers may voluntarily reduce payout to cater to the preferences of employees/unions. However, the additional analysis reported in Section 6 (to be discussed later in the paper) fails to find support for this alternative argument.

We first perform a variety of diagnostic tests to ensure that the key identifying assumption of the RD design, namely, there is no precise manipulation of votes by either workers or firms around the known threshold (50%) for a winning union election, is not violated.

We then show that unionization has a causal, negative effect on firm payout. According to our nonparametric local linear regression estimation, passing a union election leads to an 8.7% reduction in dividend payout ratio and a 17.9% reduction in total payout ratio (including both dividends and share repurchases) compared to failing an election in the following year. This result is robust to alternative choices of kernels and bandwidths, alternative measures of corporate payout (such as dividend yield, dividend over sales, dividend over cash flows, and dividend per share), and is absent at artificially chosen thresholds that determine union election outcomes. We also confirm that firms barely passing union elections and those barely failing to pass the elections exhibit similar pre-event characteristics that could affect a firm's payout policy.

Further, we explore how cross-sectional heterogeneity in labor power alters the negative effect of unions on payout policy. Since firms in states with right-to-work laws cannot force their employees to join the union or pay union dues as preconditions of employment, unions have significantly less bargaining power and thus matter less for payout policies in such states than those in non-right-to-work states. Similarly, state-level work stoppage provisions, which permit strikers to collect unemployment insurance during a labor dispute if their employer continues to operate at or near normal capacity, have been shown to affect union bargaining power in a positive way (see, e.g., Matsa, 2010). Consequently, the effect of unionization on payout policies should be stronger for firms located in states that have adopted work stoppage provisions than those states that have not. Consistent with the above conjectures, we find that the negative effect of unionization on firm payout is absent in firms located in states with right-to-work legislation but without work stoppage provisions, where unions have less power to expropriate rents.

We also find that operating flexibility is a possible underlying channel through which unions negatively affect firms' payout. Since unions increase a firm's cash flow risk and reduce the flexibility of its operations, we expect a stronger negative effect of union election passages on payout policies for firms with an ex ante lower level of operating flexibility because such firms face a larger threat from the winning elections and thus will pay out less to be better able to

save for profitable investment opportunities in the future. We find evidence consistent with this conjecture.

Another possible mechanism through which unions negatively affect firms' payout is financial flexibility. Labor unions often bargain with their employers for a higher and more stable wage level and better working conditions, putting an additional layer of financial constraint on firm management when they make investment decisions. Hence, if a firm is already financially constrained, the passage of union elections makes its financial situation even tighter, prompting it to cut down payouts so as to free up some internal retained earnings for future investment use. This argument suggests a stronger negative effect of passing union elections on payout policies for firms with ex ante more stringent financial constraints. However, after examining a comprehensive set of existing measures of financial constraints, we find mixed evidence regarding this hypothesis, which suggests that either financial flexibility is not an important channel through which unions affect payout policy or the existing financial constraint measures have their own limitations (as argued by recent studies such as Farre-Mensa and Ljungqvist, 2015).[7]

Our results so far suggest that firms reduce corporate payout and retain more internally generated earnings to cope with the upcoming operating inflexibility after the passage of union elections. A natural question is where the saved earnings (net income) go. Do firms keep these additional retained earnings in the form of liquid assets (e.g., cash and cash-equivalent marketable securities, net working capital such as accounts receivables, etc.) or use them to pay off debt so that they can increase their borrowing capacity? To answer this question, we analyze a firm's other corporate polices and find that firms use the saved earnings to invest in net working capital rather than paying off debt or increasing cash holdings.

Finally, we examine two plausible alternative explanations for the negative effect of unionization on payout. One alternative explanation is labor's innate distaste for dividends and its greater influence on managerial decision making after workers coordinate among themselves

[7] Another possibility to explain the lack of consistent evidence for the financial flexibility channel is that not paying dividends itself is an indication of tight (i.e., constrained) financial situation for a firm as argued by Fazarri et al. (1988), Almeida and Campello (2007) and Denis and Sibilkov (2009). Therefore, we do not observe much variation in payout among firms that are already financially constrained because the majority of such firms pay zero dividends anyway, regardless of their unionization status. In contrast, less financially constrained firms have more variation in their payout levels, which may allow us to observe a stronger effect of unions on payout.

more efficiently through unionization. In other words, the lower payout in unionized firms, though suboptimal from the shareholders' perspective, is preferred by the employees (unions) and thus managers cater to the preferences of employees to reduce dividends. If this alternative explanation is true, we should observe that the negative effect of unionization on payout is less pronounced if there is a better alignment of interests between employees and shareholders. We do not find evidence supporting this argument. Another alternative explanation is managers' perverse incentives to keep corporate resources within the firm. In the presence of such agency conflicts, managers would use the passage of union elections as an excuse to convince shareholders to agree with cutting dividends.[8] If this explanation is true, we would expect that the negative effect of unionization on payout is more pronounced in firms with worse corporate governance, but we actually find the opposite, which suggests that our results are more likely to be driven by firms' optimal response/strategy to the passage of union elections than by agency conflicts.

Overall we find evidence consistent with the *flexibility hypothesis* which argues that firms cut down corporate payouts as a response to passing union elections to increase or maintain their operating flexibility. However, one caveat of our study, which possibly applies to all other papers adopting the RD method, is that the RD design has strong local validity but weak external validity. Hence, one should interpret the results in this paper with care. For example, our RD analysis only allows us to make a causal argument for firms whose union elections pass or fail within close proximity around the threshold, but we cannot establish causality between unions and payout policy for firms that do not hold union elections in our sample.

We are not the first to examine this topic. A few previous studies (such as DeAngelo and DeAngelo, 1991; Matsa, 2006; and Chino, 2013) have already explored the implications of labor unions for payout policy. However, they either rely on small-sample evidence from selected industries or time periods, or document an association by focusing on industry-level union power. In contrast, our paper examines all Compustat firms that experience union elections over the entire thirty-year period between 1980 and 2011, and uses the RD design as the main identification strategy to establish a causal link between unionization and payout policy.

[8] Examples of such excuses could be "to preserve resources for future unexpected operational disruptions from organized labor" or "to reduce payout as a voluntary concession to unions/workers in order to enhance their loyalty, morale, and productivity."

Therefore, we offer a cleaner and more comprehensive perspective on this important research topic. Moreover, we make an attempt to identify possible underlying economic mechanisms through which unions affect payout decisions. In the meantime, our RD analysis on cash, debt, and working capital provides new evidence on the causal relation between labor unions and other important corporate financial policies.

The rest of our paper proceeds as follows. Section 2 discusses related literature. Section 3 describes the data and presents summary statistics. Section 4 provides our main results and robustness checks. Section 5 investigates underlying economic mechanisms. Section 6 discusses alternative explanations for our main findings. Section 7 concludes.

2. Related literature

Our paper extends the small literature on the effect of unions on payout policy. This topic is first examined by DeAngelo and DeAngelo (1991). Using a sample of seven major US steel firms in the 1980s, they make an important attempt to tackle the question and show that these firms reduce work force, manipulate earnings downward, cut management pay, and reduce dividends during union negotiations. However, we differ from their study in at least three crucial dimensions. First, and perhaps most importantly, we use the RD design as our main identification strategy that allows us to establish a causal link between unionization and payout policy, which the existing literature has not adequately achieved. Second, instead of studying a few major firms in a particular industry, our sample covers all Compustat firms that experience a union election between 1980 and 2011 and it spans across a wide range of industries. Third, we make an attempt to pinpoint possible underlying economic mechanisms through which unions affect corporate payout policy.

Two other closely related studies to ours also examine the implications of labor unions for payout policy. Matsa (2006) finds an insignificantly negative relation between union bargaining power and dividend policy. However, different from our sample that covers 32 years, his sample covers three cross-sectional snapshots in 1977, 1987, and 1999. Chino (2013) focuses on how industry-level union power affects a firm's payout policy, and finds that firms in industries with stronger labor unions tend to pay lower dividends when they are not profitable but commit to higher payouts when they are highly profitable. Nevertheless, both studies mainly

rely on OLS regression to make inference and thus cannot fully address the endogeneity problem in union power.

Our paper also adds to the voluminous literature about the costs and benefits of labor unions. This literature generally shows that unions can influence both investment and financing decisions of firms. Bronars and Deere (1991), Hanka (1998), Matsa (2010) find that unionized firms are more likely to increase financial leverage because it allows them to shield their cash flows from union expropriation. Perotti and Spier (1993) and Dasgupta and Sengupta (1993) also make similar arguments. Likewise, Klasa et al. (2009) argue that firms in unionized industries strategically hold less cash to maintain bargaining power with unions. Chen et al. (2011a, 2011b) find that the cost of equity is significantly higher but the cost of debt is lower in more unionized industries. Lee and Mas (2012) show negative abnormal stock returns over a long period subsequent to union victories, and Bradley et al. (2013) find that labor unions impede technological innovation. Both these two studies imply that unionization destroys firm value. More broadly, our paper is also closely related to the recent growing literature on how employee preferences and labor power affect firm policies such as capital structure (Berk et al., 2010; Bae et al., 2011; Chemmanur et al., 2013; Simintzi et al., 2014), innovation (Acharya et al., 2013, 2014), mergers and acquisitions (John et al., 2015), corporate financing decisions (Agrawal and Matsa, 2013), and corporate governance (Atanassov and Kim, 2009).

Our paper is related to the large literature on payout policy, starting from Bhattacharya (1979), John and Williams (1985), and Miller and Rock (1985), which identifies a variety of major factors influencing a firm's payout policy including corporate taxes, signaling motives, agency considerations, compensation practices, and management incentives (See DeAngelo et al. (2008) and Farre-Mensa et al. (2014) for excellent surveys).[9] Our results suggest that operating flexibility shaped by labor unions could be another important determinant of payout policy, consistent with the findings of Brav et al. (2005) that nowadays financial executives do not consider agency, signaling, or clientele effects as explanations for payout levels but still believe that the perceived stability of future earnings affects dividend policy.

Finally, our paper makes a direct contribution to a relatively small literature on the causes and consequences of dividend reductions. Kalay and Loewenstein (1986) find that late

[9] John et al. (2011) show that a firm's geographic location, besides other well documented determinants, also affects its payout policy.

announcements of dividends are disproportionately associated with dividend reductions. DeAngelo and DeAngelo (1990) argue that dividend cuts reflect managers' desire to reconcile the interests of various claimants during periods of financial distress. Michaely et al. (1995) study the price reactions to dividend omissions. Healy and Palepu (1988) and Benartzi et al. (1997) show an improvement in operating performance after dividend cuts. Chemmanur and Tian (2012, 2014) find that a significant proportion of dividend cutting firms prepare the market by releasing some information before dividend cuts and these firms perform better after dividend cuts. Our paper documents an additional factor that forces firms to cut dividends.

3. Data and descriptive statistics

Our data come from several sources. Union election data, including the closing date of an election, the number of eligible participants/voters, and the outcome of the election, are collected from the National Labor Relations Board (NLRB). We study union elections held between 1980 and 2011, and drop those either with a missing voting outcome or with fewer than 50 eligible participating employees.[10] Then we manually match our union election sample to Compustat by company name and address so that we can extract relevant financial statement information and other firm characteristics from Compustat. Since our study aims to analyze corporate payout policies within a three-year period following a labor union election, we need to minimize the confounding effect of other recent elections held by the same firm. To this end, we require a firm conducting a union election to be included in our sample only if it has no other elections during the past three years. In case there are multiple elections held by the same firm within one year, we retain the one with the largest number of eligible voters because this election is likely to matter most for corporate decision making. Our sampling procedure results in 1,234 unique union elections.

Our main payout policy variables are *Dividend Payout* (the dividend-to-earnings ratio), which is the total annual cash dividends distributed by a firm divided by its net income over the same fiscal year, and *Total Payout* (the total-payout-to-earnings ratio), which is the sum of total annual cash dividends and stock repurchases divided by net income. We follow Grullon and

[10] We focus on elections with at least 50 eligible participating employees because union elections with a smaller size may only have a negligible impact on corporate decisions. In addition, elections with fewer participants may also be subject to precise manipulation of votes, which violates the crucial identifying assumption of the RD design. This type of filter is commonly adopted by the labor union election literature (e.g., Lee and Mas, 2012).

Michaely (2002) to measure the amount of stock repurchases. Specifically, we define repurchases as the total expenditures on the purchase of common and preferred stocks minus any reduction in the redemption value of the net preferred stocks outstanding. Following recent literature such as Chay and Suh (2009), we drop firm-years with negative earnings but with non-negative cash dividends or total payouts because it is difficult to interpret such payout ratios.[11] To mitigate the concern for outliers (especially due to extremely small earnings), we winsorize payout ratios at the 5% level. All the rest of the firm characteristics, described in Appendix A, have been winsorized at the 1% level.

Panel A of Table 1 reports summary statistics for our union election sample. On average 42.8% of votes are in favor of unionization, with a standard deviation of 20.9%. Out of these 1,234 elections between 1980 and 2011, unions win 28.7% of them. Panel B of Table 1 presents summary statistics of the payout variables as well as other firm characteristics that are likely to affect a firm's payout decisions, including firm size (*Assets* and *Market Value*), profitability (*ROA*), asset tangibility (*PPE/Assets*), investment (*Capx/Assets*), leverage (*Debt/Assets*), cash holding (*Cash/Assets*), Tobin's Q (*TobinQ*), firm age (*Age*), cash flow volatility (*Cashflow Volatility*), institutional ownership (*Institutional Ownership*), sales growth (*Sales Growth*), and the GDP growth of the state where the firm's headquarter is located (*State GDP Growth*). The average dividend payout ratio is 24.0% with a standard deviation of 30.2%, and the average total payout ratio is 44.9% with a standard deviation of 54.7%.

Figure 1 plots the trend of union election frequencies and passage rates across our sample period. There is a considerable decline in the number of union elections over time, which is consistent with recent literature on union membership rate (see, e.g., Visser, 2006). The second graph in Figure 1 describes the passage rates (frequencies) for union elections held in each year. Despite the wide variation in passage rates across time, the majority of union elections fail to pass except for year 2008. The average passage rate of 28.7% in our sample is consistent with recent studies such as Lee and Mas 2012 (29.9%).

4. RD approach and main results

4.1 Empirical strategy and diagnostic tests

[11] Our results are qualitatively similar if we drop all firm-years with negative earnings, regardless of payouts.

We estimate the causal effect of unionization on payout policy by adopting a RD design, which assigns a firm's unionization status to our sample firms based on a simple majority (50%) passing rule. The RD design exploits a unique feature of the union election data—we observe the percentage of votes for unionization in every union election.

The intuition behind our RD strategy is as follows: elections that pass (leading to unionization in the firm) or fail (not leading to new unions inside the firm) within a narrow bandwidth around the 50% threshold should follow the pattern of a quasi-randomized experiment. Essentially, this empirical approach compares payout policies of firms that that barely pass union elections to those of firms that barely fail to pass elections. It is a powerful and appealing identification strategy for our purpose because for a close-call union election, unionization is "locally" exogenous in the sense that it is unlikely to be systematically correlated with any unobservable characteristics that might lead to its unionization status. In other words, the assignment of a treatment effect (i.e. unionization status) to the group of our sample firms is likely to be random, which helps us to identify the causal effect of unionization on corporate payout policy. This feature of the RD approach means that our empirical test design is not prone to the usual endogeneity problems or sample selection issues. Another advantage of the RD design is that we do not have to include observable covariates in our analysis (as in a standard multiple regression framework) because firms falling in a narrow band around the threshold are similar in all dimensions of characteristics. Hence, firm covariates are unnecessary for identification (see earlier survey papers on the RD approach such as Imbens and Lemieux, 2008, and Lee and Lemieux, 2010, for a more detailed discussion on this less stringent requirement).

The success of a RD strategy hinges on the satisfaction of the key assumption of imperfect control, which requires that agents (voters and firms) in an election cannot *precisely* manipulate the forcing variable (i.e., the share of favorable votes) near the known cutoff.[12] The implication is that the distribution of the forcing variable should not have any jumps around the discontinuity point (i.e., the 50% threshold). To check the validity of this assumption, we perform two diagnostic tests.

[12] Note that this assumption does not require the absence of vote manipulation in the elections. As long as agents do not have *precise* control over the forcing variable (even though some manipulation exists), an exogenous discontinuity still allows for random assignment to the treatment (see, e.g., Lee, 2008).

First, Figure 2 plots a histogram of the sample distribution of vote shares (i.e., the percentage of votes in an election in favor of unionization) across 50 equally-spaced bins (with a bin width of 2%). As we can observe, the vote share distribution is continuous within close proximity of the cutoff point (the 50% threshold), which indicates no sign of precise manipulation by voters or firms.

Second, we perform a formal statistical test, developed by McCrary (2008), for discontinuities in the density of the vote shares, which is depicted in Figure 3. The dots represent the density estimate and the bold line is the fitted density function of the forcing variable (union vote shares) surrounded by the 90% confidence interval. As one can observe, the density of vote shares appears smooth and its fitted curves show little indication of a strong discontinuity near the 50% threshold. The Z-statistic for the McCrary test of discontinuity is 1.384 (The coefficient of estimate is 0.299 with a standard error of 0.216), which is statistically insignificant. Thus we are unable to reject the null hypothesis that the density function at the cutoff is continuous, indicating that no agents have precisely manipulated the votes around the known threshold to achieve their desired unionization status. Our finding of no precise manipulation around the known cutoff is consistent with the previous literature (e.g., DiNardo and Lee, 2004).

In summary, the above two tests show that the key identifying assumption of agents' imprecise control is not violated, supporting our test premise that the variation in unionization status is as good as that from a randomized experiment (Lee, 2008).

4.2 Main RD- results

In this subsection, we report the main RD results. We consider both short-term and long-term effects of unions. Hence, we examine payout policies one, two, and three years subsequent to the elections.

Before we explore the effect of unions on payout policy in rigorous regression analyses, we first check the relation between passing a union election and payout visually. Figure 4 demonstrates the RD results graphically in an intuitive way. Figures in the left column plot *Dividend Payout* and those in the right column describe *Total Payout*. The x-axis is the forcing variable, vote share, which is the percentage of votes in favor of unionization. To the left of the 50% cutoff point, firms fail to unionize after the labor union elections; to the right of the cutoff, firms succeed in becoming unionized. As in previous figures, the spectrum of vote share is

divided into 50 equally-spaced bins (with a bin width of 2%).[13] The dots in the graphs represent the average payout ratio in each bin, and the solid line is the result of a fitted quadratic polynomial (with a 90% confidence interval). From Figure 4, we observe a discontinuity in both payout variables across the cutoff in each of the three years post the union election: we observe a significant drop in payout when moving the vote share from the left to the right of the 50% threshold. These patterns are consistent with a negative effect of unionization on payout policies.

Next, we adopt a global polynomial model to implement the RD approach (e.g., Cuñat, Gine, and Guadalupe, 2012) using all observations in our sample. The global polynomial model estimates the following specification:

$$Payout\_Ratio_{t+N} = \alpha + \beta Unionization_t + P_l(v,c) + P_r(v,c) + \varepsilon_t \quad (1)$$

where t indexes time (i.e., year of the election) and N = 1, 2, or 3. $P_l\ (v,\ c)$ is a flexible polynomial function for observations on the left-hand side of the threshold c with different orders; $P_r\ (v,\ c)$ is a flexible polynomial function for observations on the right-hand side of the threshold c with different polynomial orders; v is the vote share of an election (percentage of votes in favor of unionization). Since union elections win with a simple majority of support among the voters, c equals 50% in our setting. *Payout_Ratio* is either *Dividend Payout* or *Total Payout*, and *Unionization* is a dummy that equals one if the vote share exceeds 50%, and zero otherwise. In this regression, β measures the difference in the slopes of these smoothed functions ($P_l\ (v,\ c)$ and $P_r\ (v,\ c)$) at the cutoff point, capturing the causal effect of passing a union election on firm payout N (N=1, 2, or 3) years down the road. However, since the RD estimates are essentially weighted average treatment effects where the weights are the ex-ante probabilities that the vote shares fall in the neighborhood of the win region (Lee and Lemieux, 2010), this coefficient should be interpreted locally within close vicinity of the 50% cutoff.

We present the results estimating Equation (1) in Table 2. Panel A performs the estimation with polynomials of order three. As one can observe, the coefficient estimates on *Unionization* are all negative and statistically significant at the 1% or 5% level in most years, except for *Total Payout* in year 3, suggesting a negative effect of unionization on corporate payout policy. In terms of economic magnitude, the estimates for year 1 (N=1) suggest that firms passing union elections will have a cash dividend payout 18.1% lower and a total payout ratio

[13] Alternative choice of bin widths does not change our results qualitatively.

23.2% lower than those without successful union elections one year after the elections. Panel B and C estimate Equation (1) by using polynomials of order two and four, respectively, and find qualitatively similar results.[14]

The results from a global polynomial model point to a negative effect of unions on payout policy. However, one potential concern with the above methodology is that it uses all elections in the sample, even those with voting shares far away from the cutoff point, although they are weighted much less than the elections with voting shares close to the cutoff point during estimation. To provide more convincing evidence on the causal effect of unionization on payout policy, we implement a nonparametric local linear estimation in the vicinity of the 50% threshold using the optimal bandwidth suggested by Imbens and Kalyanaraman (2012) that minimizes the mean squared error in a sharp RD setting. Compared to the global polynomial method, the local linear estimation model has better local fitness (Bakke and Whited, 2012), more attractive rate optimality, and superior bias properties (Fan and Gijbeles, 1992, and Hahn, Todd, and van der Klaauw, 2001).

Panel A of Table 3 presents the local linear estimation results using the triangular kernel. Consistent with the results from the global polynomial estimation, the coefficient estimates on *Unionization* are all negative and statistically significant at the 1% or 5% level in most years after the union elections. Specifically, firms with winning union elections have a cash dividend payout 8.7% lower and a total payout ratio 17.9% lower than those that fail to pass union elections one year afterwards. While the statistics literature has shown that a triangular kernel is optimal for estimating local linear regressions at the boundary as it puts more weight on observations closer to the cutoff point (Fan and Gijbeles, 1992), we still adopt a rectangular kernel in Panel B of Table 3 to check the robustness of our findings. We obtain qualitatively similar results. Overall, the evidence presented in this subsection suggests a negative, causal effect of unionization on firm payout, consistent with the *flexibility hypothesis*.

Panel C of Table 3 reports local linear regression results for other important firm characteristics (covariates) that have been shown to affect corporate payout policies in the literature. One important assumption of the RD design is that there is no discontinuity in firm

[14] In untabulated analysis, we repeat the analysis using polynomials of other orders such as one or five, and find similar results. For example, if we use polynomials of order five, the RD estimate for *Dividend Payout* one year after the union election is -0.173 (with a t-stat of 1.65) and that for two years after the union election is -0.282 (with a t-stat of 3.59).

characteristics other than the unionization status across the known cutoff point. In other words, firms close to the left and the right of the cutoff point (i.e., those with vote shares slightly above or below the 50% threshold) should be similar in terms of observable, predetermined characteristics that might affect the outcome (payout policies) and/or the assignment variable (vote shares). If there are any significant "jumps" in the distribution of these important characteristics near the 50% threshold, then the treatment effect we observe using the RD design could be biased.[15]

Hence, we perform a diagnostic test for this assumption by running local linear regressions on various firm characteristics (summarized in Panel B of Table 1) at a "predetermined date" to the elections, determined as follows. Since unions must collect signatures from all eligible voters (employees) six months before filing the case to the NLRB and the average gap between the filing date and the closing date of an election is three months, any events happening to the firm during this 9-month period could potentially affect both the assignment variable (vote shares) and these firm characteristics simultaneously, making the later not predetermined. Therefore, to examine these pre-election covariates in a clean way, we pick the date one year before the reported closing date of the union elections as the "predetermined date" and analyze the covariates at the fiscal year ending date immediately before this date.

As is shown, none of the local linear RD estimates for these firm characteristics are statistically significant, suggesting that there is no discontinuity in the distribution of these covariates around the known threshold. Most importantly, the predetermined values of our key dependent variables, *Dividend Payout* and *Total Payout*, do not show discontinuity around the narrow bandwidth of the cutoff point, suggesting that our main RD results are unlikely to be driven by *ex ante* differences in payout policies between firms passing union elections and those whose elections fail.

4.3 Robustness checks

[15] Note, however, that this assumption is much less restrictive than textbook assumptions regarding endogeneity (such as the exclusion restrictions) in that it does not require those predetermined characteristics to be exogenous: as long as they are determined prior to the assignment variable (the voting share) and continuously distributed around the cutoff point (i.e. with no jumps), then the RD procedure will still yield valid and consistent estimates. See Lee and Lemieux (2010) for a more detailed discussion of this assumption and related tests.

In this section, we report a comprehensive set of robustness checks that examine the sensitivity of our RD results to various model assumptions. First, we check whether our local linear regression estimates are sensitive to the choice of bandwidths, which reflects a classical tradeoff between bias and precision. On the one hand, a wider bandwidth makes use of more observations within the local neighborhood of the cutoff and thus yields more precise estimates. On the other hand, such wide bandwidths may introduce more noise and bias into the estimation because it has used more "non-local" observations away from the cutoff where linear approximation is accurate. The converse is true for narrower bandwidths.

Hence, to address the concern that our results in Table 3 are driven by the bandwidth we have chosen, we plot the estimated local RD coefficients along with their 90% confidence intervals (on the vertical axis) as a function of the chosen bandwidth (on the horizontal axis) in Figure 5. A value of "100" on the horizontal axis represents the optimal bandwidth suggested by Imbens and Kalyanaraman (2012). "200" means 200% of (i.e., two times of) the optimal bandwidth, "300" means 300%, and so forth. The left-hand side figures describe *Dividend Payout* and the right-hand side ones are for *Total Payout*. As one can observe, the local RD estimates are almost always negative and stable, with both economic and statistical significance, over the whole spectrum of bandwidth choices. This result shows that our local linear RD estimates are unlikely to be driven by any specific choice of bandwidths.

Second, if our RD estimation truly reflects a negative, causal effect of unionization on corporate payout, we should not observe a similar effect if we artificially assume a threshold other than 50% that determines union election outcomes. Hence, we run placebo tests to check whether we still observe a discontinuity in payout ratios at randomly selected thresholds that are different from the true 50% threshold. We run this placebo test 1,000 times and plot a histogram of the distribution of the corresponding local RD estimates in Figure 6. The vertical dashed line stands for the value of the local RD estimate obtained using the true cutoff point of 50%. As we can observe, all of the histograms in Figure 6 are approximately centered around 0, suggesting that the negative effect of unionization on payout is absent if we artificially pick a cutoff point other than 50%. This placebo analysis enhances our confidence in the RD procedure and the resulting estimates, as it rules out chances as an explanation for our main findings in the previous subsection.

Third, we examine alternative measures of a firm's payout policy and present the results in Panel A of Table 4. Row (1) shows that unionization has a significantly negative effect on firms' dividend yields (i.e., cash dividends per dollar of stock owned) and total yields (i.e., total payout amount per dollar of stock owned) three years down the road. Specifically, firms with winning union elections have a dividend yield 1.1% lower and a total yield 1.5% lower than those that fail to pass union elections one year afterwards. Row (2) and (3) examine payout ratios using a firm's sales and free cash flows (rather than its earnings), respectively, as the denominator, and find similar (though slightly weaker) results to our main local RD results in Table 3.[16] In Row (4), we do not scale payout amounts but instead directly analyze cash dividends and total payouts on a per share basis. The local RD estimates remain negative and statistically significant for the majority of columns.

Fourth, we try alternative ways of constructing our sample and present the results in Panel B of Table 4. Recall that to construct our main RD sample, we retain the election with the largest number of eligible voters if there are multiple elections held by the same firm within one year. Another way of dealing with multiple elections held by the same firms is to simply keep the first one. Presented in Row (1) of Table 4 Panel B, the local linear regression results using such "first" elections within a year are both qualitatively and quantitatively similar to our main local RD results in Table 3. Another filter we apply when constructing our main sample is that we require a firm holding an election not to have union elections in the previous three years so as to avoid the confounding effects of such historical events. An alternative way of tackling the confounding effects of multiple elections held by the same firm is to require the firm not to have other elections both in the previous and the next three years. Although this is a more stringent filter, which reduces our sample size and thus the power of our tests, we still obtain similar (but slightly weaker) local linear RD results, which are summarized in Row (2) of Panel B.

Row (3) of Panel B adopts an even stricter data filter, requiring a firm to hold only one election throughout our sample period. Despite the fact that this filter substantially reduces our sample size, the local RD estimates continue to remain significantly negative for almost all regressions. Further, to check how our results are affected by the confounding effects of multiple elections held by the same firm, we keep all elections held by the same firm in our sample period

[16] Scaling dividend and total payouts using operating cash flows yields very similar results.

and run the local linear regressions using this enlarged sample. The results, presented in Row (4) of Panel B, show that unionization still has a significantly negative effect on payout policies within the three-year period after the union elections, though the economic magnitudes of the estimated RD coefficients are much smaller than those in Table 3, possibly due to the confounding (or offsetting) effects of multiple elections. Row (5) of Panel B examines union elections with at least 100 eligible participating employees (rather than 50 such employees, as in our baseline analysis) and our local RD estimates continue to remain significantly negative for the majority of columns, despite the decreased sample size due to this stricter sample filter. Lastly, to address the concern that a sizable fraction of the firms in our sample have zero total payouts and thus may behave differently from nonzero-payout firms in response to union election outcomes, we exclude those firms that have zero total payout in the year before union elections and report the results in Row (6). Our baseline local RD results continue to hold in this subsample of firms.

4.4 Cross-sectional heterogeneity in labor power

Having established a causal link between labor unions and payout policy, we next explore how cross-sectional heterogeneity in labor power alters the relation between unions and payout.

Since firms in states with right-to-work laws cannot force their employees to join the union or pay union dues as preconditions of employment, unions have significantly less bargaining power in such states than those in non-right-to-work states. As a result of the weaker union bargaining power in a right-to-work state, the passing of unionization elections is likely to have a smaller effect on payout policies in such a state than in states without similar legislations. By the same token, state-level work stoppage provisions, which permit strikers to collect unemployment insurance during a labor dispute if their employer continues to operate at or near normal capacity, have been shown to affect union bargaining power in a positive way because under such regulations labor strikes (often organized by unions) effectively become less costly for participating workers (see, e.g., Matsa, 2010). Consequently, the effect of unionization on corporate payout policies should be stronger for firms located in states that have adopted work stoppage provisions than those states that have not. We test the above conjectures in this subsection.

Table 5 reports the local linear RD estimates for firms located in those states either without right-to-work legislations or with work stoppage provisions (the top panel) as opposed to those located in those states with right-to-work laws but without work stoppage provisions (the bottom panel).[17]

As expected, firms winning union elections in states either without right-to-work legislation or with work stoppage provisions (which give unions more bargaining power and thus make unionizations more relevant for corporate decision making) have significantly lower payout than those failing the elections, both economically and statistically, within three years after the unionization. This result confirms our main finding that unionization leads to a decline in corporate payouts. On the other hand, the coefficient estimates on *Unionization* for firms located in right-to-work states but without work stoppage provisions (where unions have the weakest power) are negative and statistically insignificant across all three post-election years for the two payout measures, suggesting that unionization has little effect on payout policies in such states where unions are not favored by regulations.[18]

5. Underlying mechanisms

So far our empirical evidence suggests a negative, causal effect of labor unions on payout policy, consistent with the *flexibility hypothesis*. In this section, we aim to further understand the underlying mechanisms through which the passing of union elections reduces firm payouts. We achieve this goal by exploring how unionization affects dividend and total payout ratios differently in the cross section. In Section 5.1, we use the cross-sectional variation in a firm's operational leverage to examine whether the relation between labor unions and operating flexibility helps explain their negative effect on payout. In Section 5.2, we explore how the cross-sectional variation in a firm's degree of financial constraints affects the negative link between unionization and corporate payouts. In Section 5.3, we explore how firms make use of

[17] States with right-to-work laws as of 2011 (our union election sample end year) include Alabama, Arizona, Arkansas, Florida, Georgia, Idaho, Iowa, Kansas, Louisiana, Mississippi, Nebraska, Nevada, North Carolina, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, and Wyoming. States with work stoppage provisions as of 2011 include Delaware, Georgia, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Massachusetts, Mississippi, Missouri, Montana, Nebraska, New Hampshire, New Mexico, North Dakota, Oklahoma, Pennsylvania, Rhode Island, South Dakota, Texas, Utah, Vermont, Washington, West Virginia, and Wyoming.

[18] In untabulated analysis, we also find that the negative, causal effect of unionization on payout ratios is stronger when the elections take place in the same city as a firm's headquarter is located or when a firm has fewer geographic segments (as reported by the business segment data of Compustat), consistent with the notion that our results are stronger when establishment-level union elections matter more for corporate central decision making.

their "saved money" (i.e., the higher retained earnings) by examining a number of other corporate financial policies including leverage, cash holdings, and working capital management.

5.1 Operational leverage

The first underlying mechanism through which labor unions reduce corporate payouts is their adverse effect on firms' operating flexibility. To reduce cash flow risk and make their operations more flexible, firms need to increase the variable component of their cost structure, which entails linking their labor expenses (salaries and other employee compensation) to sales revenue. However, labor unions have a reputation for making wages sticky and layoffs costly, thereby increasing the adjustment costs of a firm's labor stock. Furthermore, unions frequently intervene in a firm's restructuring activities to save workers' jobs, such as blocking plant closures, which makes it harder for the firm to adjust its physical capital. Therefore, unions will increase a firm's cash flow risk and reduce its operating flexibility.

This argument implies a stronger negative effect of the passage of union elections on payout policies for firms with an *ex ante* lower level of operational flexibility because such firms face a larger threat from the winning elections and thus will decrease their payouts to a greater degree. By doing so, these low-operating-flexibility firms will prevent their cash flow risk from going up further, which allows them to become better able to save for profitable investment opportunities in the future. As a result, we expect that the negative effect of wining union elections on payout policies is stronger when firms have higher operational leverage (i.e., lower operating flexibility).

We follow the existing literature (e.g., Mandelker and Rhee, 1984; and Chen et al., 2011a) and estimate a firm's operational leverage as the elasticity of the firm's earnings before interest and taxes (EBIT) with respect to its sales, using the most recent 12 quarterly observations before an election's "predetermined date", which is defined in Section 4.2. Firms with operational leverage higher than the sample median are those with lower operating flexibility. The local RD estimation results for firms in the high operational leverage subsample (the top panel) and the low operational leverage subsample (the bottom panel) are reported in Table 6.

Consistent with our conjecture, the coefficient estimates on *Unionization* are all negative and significant in the top panel (i.e. for firms with high operational leverage), suggesting that the

negative effect of unionization on payout is mostly concentrated in firms with less flexible operations. On the contrary, the local RD estimates for firms with low operational leverage (in the bottom panel) are much smaller in magnitude than those in the top panel and statistically insignificant (with small t-statistics), perhaps due to the fact that such firms have flexible operations to begin with and thus the passage of unionization elections would have no material effect on its cash flow risk or operating flexibility, making it less necessary for them to reduce their dividend payouts.

Another test we perform to examine the operating flexibility channel is to conduct an event study on the union elections. First, we run nonparametric local linear regressions on the cumulative abnormal returns (CARs) around the closing dates of union elections (i.e., the events). We examine CARs calculated from the market model during various event windows, including those from day -3 to day +3, from day -1 to day +3, and from day 0 to day +5. We report the results in Table 7. Panel A examines the CARs using all the union elections. Panels B1 and B2 examine the CARs for subsamples separated by the level of operational leverage. As we can see, the market overall does not react significantly to the passage or failure of the union elections, which is consistent with the findings of previous studies such as Lee and Mas (2012). However, unionization has a significantly negative effect on the announcement returns for firms with high operational leverage (i.e., those having low operating flexibility), but has a small and insignificant effect on the announcement returns for firms with low operational leverage. These results suggest that the market only deems unionization as detrimental for a firm's equity value if the firm is operationally inflexible, which is consistent with our hypothesis that unions would tend to reduce the operating flexibility of a firm and thus hurt its future operations and long-term value.

Second, we also explore whether the market reacts differently to winning union elections based on a firm's operating flexibility (operational leverage). To that end, we regress the CARs of those winning union elections on *OL dummy,* a dummy variable that equals one if a firm's operational leverage is above the sample median and zero otherwise, as well as other control variables. We again examine alternative event windows and report the results in Table 8. Consistent with our hypothesis, the coefficient estimates of *OL dummy* are negative and significant in five out of six models, suggesting that the market responses to the passage of union

elections more negatively for firms with lower operating flexibility (high operational leverage). This result again supports our conjecture that operating flexibility might be a plausible economic channel through which unions affect firms' operations and thus alter their payout policies.

In summary, the evidence presented in this subsection suggests that operating flexibility is likely an underlying economic mechanism through which the passage of union elections reduces firm payouts.

5.2 Financial flexibility

The second possible economic mechanism we consider is a firm's financial flexibility. On behalf of the employees, labor unions often bargain with their employers for a higher and more stable wage level and better working conditions, putting an additional layer of constraint on firm management when they make investment decisions. Even if a profitable project suddenly becomes available, the firm managers may not be able to immediately grasp the investment opportunity because the high setup costs at the initial stage and subsequent cash flow uncertainties may lead to volatile cash outflows. This problem results in a possible delay in sending out paychecks or even a drastic restructuring of the company's workforce including layoffs and forced turnovers, all of which will invite severe criticism and face vehement opposition from the union leaders. To the extent that unions have bargaining power with the board and management, the firm may end up having to give up many value-enhancing investment opportunities unless it has enough financial flexibility (e.g., enough internal retained earnings, easy access to external capital, or few pre-commitments to pay out dividends).

Hence, if a firm is already financially constrained, the passage of unionization elections makes its financial situation even tighter, prompting it to cut down payouts so as to free up some internal retained earnings for future investment use. On the other hand, firms that are less financially constrained do not need to drastically change their payout policies because the newly established unions are unlikely to affect their future investment activities significantly. The above argument suggests a stronger negative effect of passing union elections on payout policies for firms with *ex ante* more stringent financial constraints.

To examine this conjecture, we use a comprehensive set of existing measures of financial constraints (such as the Kaplan-Zingales (KZ) index, the Whited-Wu (WW) index, the Size and Age (SA) index developed in Hadlock and Pierce (2010), and the Hoberg-Maksimovic

text-based (HM) index) as of the "predetermined date" to proxy for the strength of a firm's financial conditions. Firms with a given index above (below) the sample median are considered to be more (less) financially constrained.[19] We report the local RD estimation results for firms in the more financially constrained subsample and the less financially constrained subsample in Table 9. Panels A1 and A2 examine the KZ Index, Panels B1 and B2 explore the WW Index, Panels C1 and C2 study the SA Index, and Panels D1 and D2 examine the HM Index.

Our results are mixed regarding the financial flexibility channel. Consistent with our prior, the coefficient estimates on *Unionization* are mostly negative and significant in Panel A1 (i.e. for firms with more stringent financial constraints as measured by the KZ Index), suggesting that the negative effect of unionization on payout is concentrated in financially constrained firms. By contrast, the local RD estimates for less constrained firms measured by the KZ Index (in Panel A2) are much smaller in magnitudes than those in the top panel and are statistically insignificant (with tiny t-statistics). Moreover, three of the six estimated coefficients are actually positive, though statistically insignificant.

However, the evidence based on the WW Index (in Panels B1 and B2) seems to suggest the opposite: for most (five out of six) columns, the negative effect of unions on corporate payout is stronger for less financially constrained firms. We continue to observe mixed (i.e., inconclusive) results in Panels C1-C2 (the SA Index) and D1-D2 (the HM Index).

In untabulated analysis, we examine a variety of other firm characteristics that reflect a firm's financial flexibility used in the existing literature, such as firm size, firm age, cash flows, Tobin's Q, the existence of bond ratings, and firm profitability. We continue to find mixed evidence.

One caveat for the analysis in this section is that the literature so far has not reached a consensus as to which of the financial constraint measures is the best (or the "right") one (see recent papers such as Farre-Mensa and Ljungqvist (2015) and Hoberg and Maksimovic (2015) for a more detailed discussion on this issue). That is why we choose to examine a comprehensive set of financial constraint measures to ensure that our results are not driven by any particular measure adopted. While our mixed evidence above suggests that financial flexibility might not

[19] Note that the HM Index is available for Compustat firms only between 1997 and 2009, which reduces our sample size. Moreover, following the suggestions of Hoberg and Maksimovic (2015), we treat Compustat firms with missing HM index as the least financially constrained ones (i.e., setting their HM Index to the sample minimum before splitting the sample based on medians).

be an important underlying economic mechanism through which unions decrease firm payout, it could also reflect the possible limitations of existing financial constraint measures.

Another possible explanation for the lack of consistent evidence on the financial flexibility channel is that not paying dividends itself is an indication of tight (i.e., constrained) financial situation for a firm, as argued by previous literature such as Fazarri et al. (1988), Almeida and Campello (2007) and Denis and Sibilkov (2009). Therefore, we do not observe much variation in payout (especially dividends) among firms that are already financially constrained because the majority of such firms pay zero dividends anyway, regardless of their unionization status. In contrast, less financially constrained firms have more variation in their payout levels, which could allow us to observe a stronger effect of unions on payout. In other words, the fact that dividend payment is one indication of financial strength biases against us finding results consistent with our prior, even if unions do affect payout through the channel of financial flexibility.

Overall, based on the above discussions, we cannot conclude that financial inflexibility is a possible underlying mechanism through which unionization affects corporate payout.

5.3 Other financial policies

Our results so far suggest that firms reduce corporate payout and retain more internally generated earnings to cope with the upcoming operating inflexibility after the passage of union elections. A natural question is where the saved earnings (net income) go. Do firms keep these additional retained earnings in the form of liquid assets (e.g., cash and cash-equivalent marketable securities, net working capital such as accounts receivables, etc.) or use them to pay off debt so that they can increase their borrowing capacity? To answer this question, we analyze, in this subsection, a firm's other corporate polices (such as leverage, cash holdings, and working capital management) following its union elections.

To be consistent with our main payout measures (dividend ratios and total payout ratios), we scale a firm's change of balance sheet items (such as long-term debt, cash, and working capital) over a fiscal year following the union elections by its earnings (net income) during the same year. These normalized change-variables can then indicate how the firm makes use of these saved earnings after cutting payouts to its shareholders. However, in such tests, we have to drop

those firm-years with negative or zero earnings because otherwise there is nothing to "save" (i.e., it is difficult to interpret the results with non-positive earnings).

Table 10 summarizes the local linear RD regression results of other financial policies. Panel A examines a firm's capital structure decisions, measured by its change in total debt over earnings (*Δ Total Debt over Earnings*), within the first three years after the passage of union elections. As we can see, the coefficient estimates on *Unionization* are economically small and statistically insignificant, suggesting that firms do not use the additional saved retained earnings to pay off their total debt.[20] Similarly, Panel B finds that a firm's cash policy, measured by its change in cash and short-term investments (*Δ Cash over Earnings*), is not significantly affected by the passage of union elections either. However, Panel C of Table 10, which examines a firm's working capital management, measured by its change in net working capital (*Δ Working Capital over Earnings*), shows that the coefficient estimates on *Unionization* are positive and statistically significant in the first two years after the union elections, suggesting that firms use saved earnings mostly in the investment of its net working capital (i.e., liquid assets not necessarily in the form of cash).[21]

The above results indicate that strategic (bargaining) concerns of firm managers after unionization, as documented in previous literature (e.g., Matsa, 2010, and Klasa et al., 2009), might have limited their abilities to keep saved earnings within the firm in the form of very liquid assets (i.e., cash and cash-equivalent marketable securities) or to use such savings to pay off debt.[22] This is because unions could have easily observed and verified such usage of corporate resources and thus pressured firm managers to use these additional savings to improve worker welfare instead, nullifying managers' effort to maintain operating flexibility after the enhancement of union power. In contrast, firms would use their saved earnings (from reduced payout) mostly to invest in the net working capital such as increasing its accounts receivables

[20] The results remain qualitatively similar if we examine long-term debt only.

[21] Note that debt, cash, and working capital are balance sheet items whereas earnings and payout (dividends) are income statement items. We examine changes (rather than levels) of these balance-sheet variables because they are directly related to the saved earnings by paying out less. For example, a firm with a lower payout ratio could use the larger retained earnings to pay down debt, invest in net working capital, or increase its cash holdings.

[22] In untabulated analysis, we also examine alternative measures of capital structure (such as the book and market-value leverage ratios) and alternative measures of cash policy (such as cash over assets or PPE) in the local RD setting, and continue to find insignificant coefficient estimates before *Unionization.*

and/or decreasing its accounts payables, as they could make easier arguments to union leaders why such operational changes are necessary to the firm's long-run sustainability.

In summary, this subsection shows that to deal with the operating inflexibility brought by the passage of union elections, firms would reduce corporate payout to shareholders and keep saved earnings within the firm in the form of liquid assets not easily grasped or negotiated away by the unions.

6. Alternative explanations

While the concerns over operating inflexibility could be the main channels through which unionization reduces corporate payout, several alternative mechanisms could give rise to the same empirical results documented above.

One alternative explanation for the negative effect of unionization on payout is labor's innate distaste for dividends and its greater influence on managerial decision making after workers coordinate among themselves more efficiently through unionization. As a claimant to firms' resources, workers generally compete with shareholders in extracting economic rents created by the business and thus prefer to retain cash flows within the firms as opposed to paying them out. In fact, they sometimes regard corporate payout as "wages paid to shareholders" (e.g., Michaely and Roberts (2012)) and thus generally oppose the idea of increasing dividend payouts. When workers organize themselves more efficiently through unionization, they gain more bargaining power against shareholders in shaping the firm's financial policy by either sending more of their representatives onto the board of directors (as documented in the previous literature such as Hunter (1998) and Appelbaum and Hunter (2004)) or negotiating with corporate managers more aggressively about limiting/reducing corporate payout. Thus, our documented negative effect of unionization on corporate payout could be due to the fact that unions actively pressure managers to reduce their dividends and total payouts, rather than due to a voluntary and optimal response of shareholders (and managers) to the passage of union elections out of concerns for future operating inflexibility.

To examine the above alternative explanation, we divide our sample of firms based on the alignment of interests between employees and shareholders, which is measured by employees' stock ownership through pension plans. We obtain detailed asset holdings of a firm's pension plans (including both defined contribution plans and defined benefit plans), required to

be reported in the IRS Form 5500, from Boston College's Center for Retirement Research (CRR) Data Enclave website. Since such pension plan data are only available from 1992 to 2007, we limit our tests to union elections held during this time period when doing this subsample test. Following Rauh (2006), we adopt two measures of employees' stock ownership through pension plans: the fraction of firm equity ownership by pensions (i.e., the percentage of the firm's equity market value held by employees through pension plans) and the percentage of pension holdings invested in the firm's own securities (i.e., the market value of the firm's own securities held by their pension funds divided by the pensions' total asset holdings).

Larger employee stock ownership through pension plans means better alignment of interests between employees and shareholders, because employees in such firms care more about shareholder wealth than those in firms with smaller employee stock ownership. Hence, unions in firms with larger employee stock ownership would not use their influence over firm management to push for employee-friendly decisions that may ultimately lead to a decline in shareholder wealth. If the alternative explanation for our findings (based on labor's innate distaste for dividends and union power) is true, we would expect that the negative effect of unionization on payout is weaker in firms with larger employee stock ownership because workers in such firms would have less incentives to use their enhanced union power (through the passage of union elections) to limit or reduce corporate payout.

Table 11 reports the local RD estimation results for the subsample analysis based on the fraction of firm equity ownership by pensions (Panels A and B) and the percentage of pension holdings invested in the firm's own securities (Panels C and D). As we can observe, there is no clear evidence that the negative effect of unionization on dividend payout or total payout is weaker in firms with larger employee stock ownership (in Panels A and C). In fact, there is some mild evidence that the effect of unionization on payout policy is slightly stronger in firms with larger pension ownership (when comparing Panels A and B), which is inconsistent with the alternative explanation based on labor's distaste for dividends and the direct influence of union power after the passage of union elections. In fact, the evidence in Panels A and B is more consistent with the voluntary response of shareholders (and managers) to unionization because for firms with larger employee stock ownership through pensions, more of their employees have the same concerns as other shareholders over the upcoming operating inflexibility after the

passage of union elections and thus are willing to support the firms' decisions to limit or reduce corporate payout.[23]

Another alternative explanation for our documented results is managers' distaste for corporate payout due to their empire building incentives or preferences for private benefits of control, which motive them to keep corporate resources within the firm rather than distributing them back to shareholders. In the presence of such agency conflicts, managers would use the passage of union elections as an excuse to convince shareholders to agree with cutting dividends in the name of "preserving corporate resources for future unexpected operational disruptions from organized labor" or "reducing payout as a voluntary concession to unions/workers in order to enhance their loyalty, morale, and productivity." Under this alternative explanation, the lower payout in unionized firms is not caused by the optimal response of shareholders to cope with future operating inflexibility, but rather caused by perverse incentives of managers whose interests divert from those of shareholders.

To examine this alternative explanation, we divide our sample of firms based on their levels of corporate governance, measured by the entrenchment index (E-index) developed by Bebchuk, Cohen, and Ferrell (2009). While existing literature generally uses the G-index developed in Gompers, Ishii, and Metrick (2003) to measure the quality of corporate governance, Bebchuk, Cohen, and Ferrell (2009) find that six anti-takeover provisions of the corporate charter, which are part of the 24 anti-takeover provisions comprising the G-index, are most useful in capturing the effective level of managerial entrenchment and have the merit of not being vulnerable to the noise produced by the other anti-takeover provisions in the G-index.[24] Therefore, we measure corporate governance using the entrenchment index ("E-index") consisting of the six provisions discussed in Bebchuk, Cohen, and Ferrell (2009). A higher level of the E-index indicates poorer corporate governance because the managers are more entrenched. Then we partition our sample into two subsamples based on whether a firm's E-index is above the sample median, and test how our local RD results differ across these two subsamples. Since the E-index data are only available from 1990 to 2006, we limit our tests to union elections held

[23] In untabulated analysis, we also examine alternative measures of interest alignment between employees and shareholders (such as the ratio of defined benefit plans' holdings to defined contribution plans' holdings) and find similar results.

[24] The six provisions are staggered boards, limits to bylaw amendments, limits to charter amendments, supermajority requirements for mergers, poison pills, and golden parachutes.

during this time period when carrying out this subsample test. If the alternative explanation for our findings (based on managers' distaste for dividends due to agency reasons) is true, we would expect that the negative effect of unionization on payout is weaker in firms with better corporate governance (i.e., lower E-index) because managers in such firms would have fewer opportunities to use their power/influence to limit or reduce corporate payout.

We report the local RD estimation results for the subsample analysis based on the level of corporate governance in Table 12. Opposite to the predictions of the above alternative explanation, the negative effect of unionization on payout is much stronger (in terms of both statistical significance and economic magnitude) in firms with better corporate governance, suggesting that the lower payout level after the passage of union elections are unlikely to be driven by the agency conflict between shareholders and managers who hope to retain corporate resources within the firms for their personal benefits.[25]

In sum, this subsection shows that the documented negative causal effect of unionization on corporate payout is not likely to be driven by the preferences and control power of either employees or managers (i.e., agency conflicts), but more likely to be driven by firms' optimal response/strategy to the passage of union elections.

7. Conclusion

In this paper, we have studied the causal effect of labor unions on corporate payout policy. To establish causality, we have used a regression discontinuity design that relies on "locally" exogenous variation generated by union elections that pass or fail by a small margin of votes. Passing a union election leads to an 8.7% lower dividend payout ratio and a 17.9% lower total payout ratio (including both dividends and share repurchases) than failing the election in the following year. The negative effect of unions on payout is absent in firms located in states with right-to-work laws but without work stoppage provisions, and firms having high operating flexibility. Our results are robust to alternative choices of kernels and bandwidths, alternative measures of corporate payout, and are absent at artificially chosen voting thresholds that determine union election outcomes. We show that operating leverage is one possible mechanism through which unions negatively affect payout, and that firms use the saved earnings from

[25] In untabulated analysis, we also examine alternative measures of corporate governance used in the literature (such as product market competition and institutional ownership) and find very similar results to the ones using E-index.

reductions in payout to invest in net working capital rather than paying off debt or increasing cash holdings.

Overall, we find evidence consistent with the flexibility hypothesis which argues that firms cut down corporate payout as a response to passing union elections to increase or maintain their operating flexibility and reduce their cash flow risks. Our paper sheds new light on the determinants of payout policy and how organized labor influences corporate financial policies.

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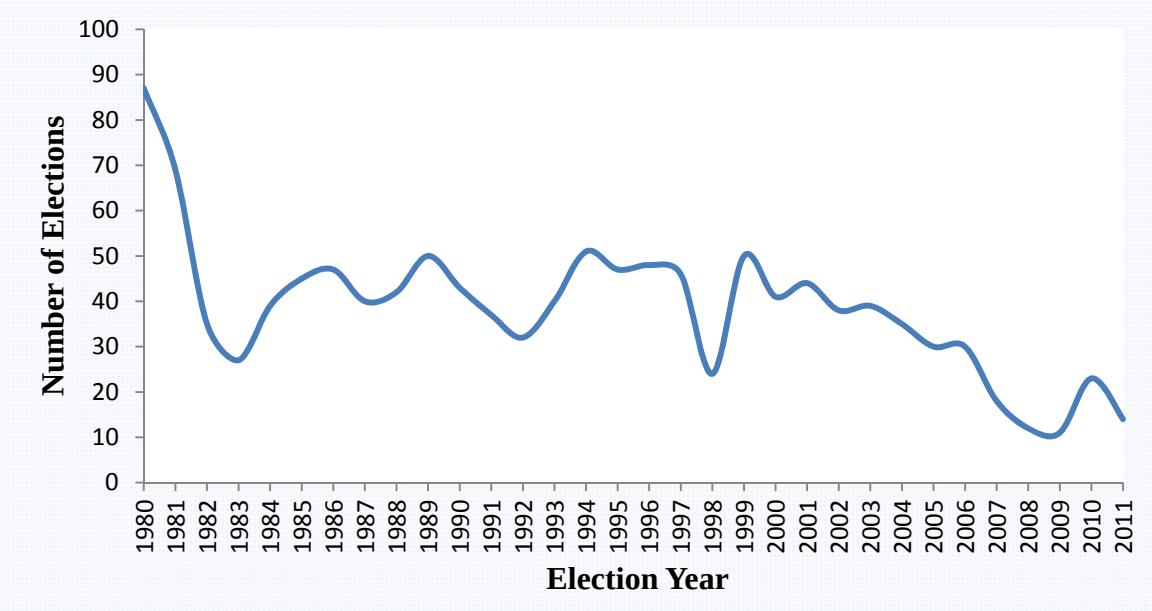
Appendix A: Definition of variables

Variable	Definition
Measures of payout policy	
Dividend Payout	Dividends (Compustat data #21) divided by income before extraordinary items (#237) measured at the end of fiscal year *t*;
Total Payout	Sum of Dividends (#21) and repurchase divided by income before extraordinary items (#237) measured at the end of fiscal year *t*. Repurchase is measured as expenditures on the purchase of common and preferred stocks (#115) minus any reduction in the redemption value of the net number of preferred shares outstanding (#56);
Dividend Yield	Dividends (Compustat data #21) divided by market value of equity (#199*#25) measured at the end of fiscal year *t*.
Total Yield	Sum of Dividends (#21) and repurchase divided by market value of equity (#199*#25) measured at the end of fiscal year *t*. Repurchase is measured as expenditures on the purchase of common and preferred stocks (#115) minus any reduction in the redemption value of the net number of preferred shares outstanding (#56).
Dividend Sales Ratio	Dividends (Compustat data #21) divided by sales (#12) measured at the end of fiscal year *t*.
Total Payout Sales Ratio	Sum of Dividends (#21) and repurchase divided by sales (#12) measured at the end of fiscal year *t*. Repurchase is measured as expenditures on the purchase of common and preferred stocks (#115) minus any reduction in the redemption value of the net number of preferred shares outstanding (#56).
Dividend Per Share	Dividends (Compustat data #21) divided by common shares outstanding (#25) measured at the end of fiscal year *t*.
Total Payout Per Share	Sum of Dividends (#21) and repurchase divided by common shares outstanding (#25) measured at the end of fiscal year *t*. Repurchase is measured as expenditures on the purchase of common and preferred stocks (#115) minus any reduction in the redemption value of the net number of preferred shares outstanding (#56).
Measures of covariates and other variables	
Assets	Book value of total assets measured at the end of fiscal year *t* (item #6);
Market Value	Market value of equity measured at the end of fiscal year *t*, calculated as #199 × #25;
ROA	Return on assets defined as operating income before depreciation (#13) divided by book value of total assets (#6) measured at the end of fiscal year *t*;
Leverage	Firm *i*'s leverage ratio, defined as book value of long-term debt (#9) divided by book value of total assets (#6), measured at the end of fiscal year *t*;
PPE/Assets	Property, plant & equipment (#8) divided by book value of assets (#6) measured at the end of fiscal year *t*;
Cash/Assets	Cash and short-term investments (#1) divided by book value of total assets (#6), measured at the end of fiscal year *t*;
Capx/Assets	Capital expenditures (#128) divided by book value of total assets (#6) measured at the end of fiscal year *t*;

TobinQ	Firm *i*'s market to book ratio, defined as market value of equity plus book value of assets minus book value of equity minus deferred taxes (set to zero if missing) divided by book value of assets, measured at the end of fiscal year *t*;
Age	Firm *i*'s age, defined as the number of years it has been listed on Compustat;
Cashflow Volatility	The standard deviation of firm *i*'s cash-flow-over-asset ratios using the most recent twelve quarterly observations before the end of fiscal year *t*. Cash flows are calculated as operating income before depreciation – interest -taxes - Δ(current assets - current liabilities*)*.
Institutional Ownership	Institutional holdings (in percentages) for firm *i* over fiscal year *t*, calculated as the arithmetic mean of the four quarterly institutional holdings reported by 13F filings.
Sales Growth	The logarithm of firm *i*'s sales (#12) measured at the end of fiscal year *t* minus the logarithm of its sales at the end of fiscal year *t-1*.
State GDP Growth	The GDP growth rate for the state where firm *i*'s headquarter is located, calculated as the logarithm of the state's GDP measured at the end of year *t* minus the logarithm of its GDP at the end of year *t-1*. State GDP data are obtained from the *Bureau of Economic Analysis* (www.bea.gov).
Operational Leverage	The elasticity of a firm *i*'s earnings before interest and taxes (EBIT) with respect to its sales using the most recent twelve quarterly observations before the end of fiscal year *t*;
KZ Index	Firm *i*'s Kaplan and Zingales index measured at the end of fiscal year t, calculated as -1.002 × cash flow [(#18+#14)/#8] plus 0.283 × Q [(#6+#199×#25-#60-#74/#6] plus 3.139 × leverage [(#9+#340/(#9+#34+#216)] minus 39.367 × dividends [(#21+#19/#8] minus 1.315 × cash holding (#1/#8), where #8 is lagged;
WW Index	Firm *i*'s Whited and Wu index measured at the end of fiscal year *t*, calculated as – 0.091 × [(income before extraordinary items + depreciation and amortization) /(assets)] [(# 18+#14)/#6)] – 0.062 × [indicator set to one if the sum of common dividends (#21) and preferred dividends (#19) is positive, and zero otherwise] + 0.021 × leverage[(#9)/(#6)] – 0.044 × log(assets) [log(#6)] + 0.102 × average industry sales (#12) growth [estimated separately for each three-digit SIC industry and each year, with sales growth defined as above] – 0.035 × sales growth;
SA Index	Firm *i*'s size and age (SA) index measured at the end of fiscal year *t*, calculated as $-0.737\times Size + 0.043\times Size^2 - 0.040\times Age$, where Size equals the log of inflation-adjusted total book assets (#6) (in 2004 dollars), and Age is the number of years firm *i* has been publicly listed with non-missing stock price information (in Compustat). When calculating the index, we follow Hadlock and Pierce (2010) to cap Size at the log of $4.5 billion and cap Age at 37 years;
HM Index	Firm *i*'s Hoberg and Maksimovic index measured at the end of fiscal year *t*. This text-based financial constraint measure, explained in more details in Hoberg and Maksimovic (2015), is derived purely from a firm's 10-K (specifically, the Management's Discussion and Analysis (MD&A) section). For each MD&A section, the Liquidity and Capital Resources subsection is extracted and processed by using the text processing software provided by meta Heuristica LLC to identify which firm-year filings contain statements suggesting that the firm may have to delay its investments due to financial constraints;
Δ Total Debt over Earnings	The change of total (short-term and long-term) debt divided by income before extraordinary items (#237) measured at the end of fiscal year *t*. The change of total debt is measured as the sum of debt in current liabilities (#34) and long- term debt (#9) at the end of fiscal year *t* minus the sum of short-term debt and long- term

	debt at the end of fiscal year *t-1;*
Δ Cash over Earnings	The change of cash and short-term investments divided by income before extraordinary items (#237) measured at the end of fiscal year t. The change of cash and short-term investments is measured as cash and short-term investments (#1) at the end of fiscal year t minus cash and short-term investments (#1) at the end of fiscal year *t-1*;
Δ Working Capital over Earnings	The change of working capital divided by income before extraordinary items (#237) measured at the end of fiscal year t. The change of working capital is measured as the difference between current assets (#4) and current liabilities (#5) at the end of fiscal year t minus the difference between current assets (#4) and current liabilities (#5) at the end of fiscal year t-1;
E-index	Entrenchment Index (E-index) data is obtained from Lucian Bebchuk's website (http://www.law.harvard.edu/faculty/bebchuk/data.shtml). It is the sum of six anti-takeover provisions including staggered boards, limits to bylaw amendments, limits to charter amendments, supermajority requirements for mergers, poison pills, and golden parachutes.

Figure 1: Number of union elections and passage rates by year
This figure plots the number of union elections by year (top) and the average passage rates by year (bottom). Union election results are from the NLRB over 1980 to 2011.



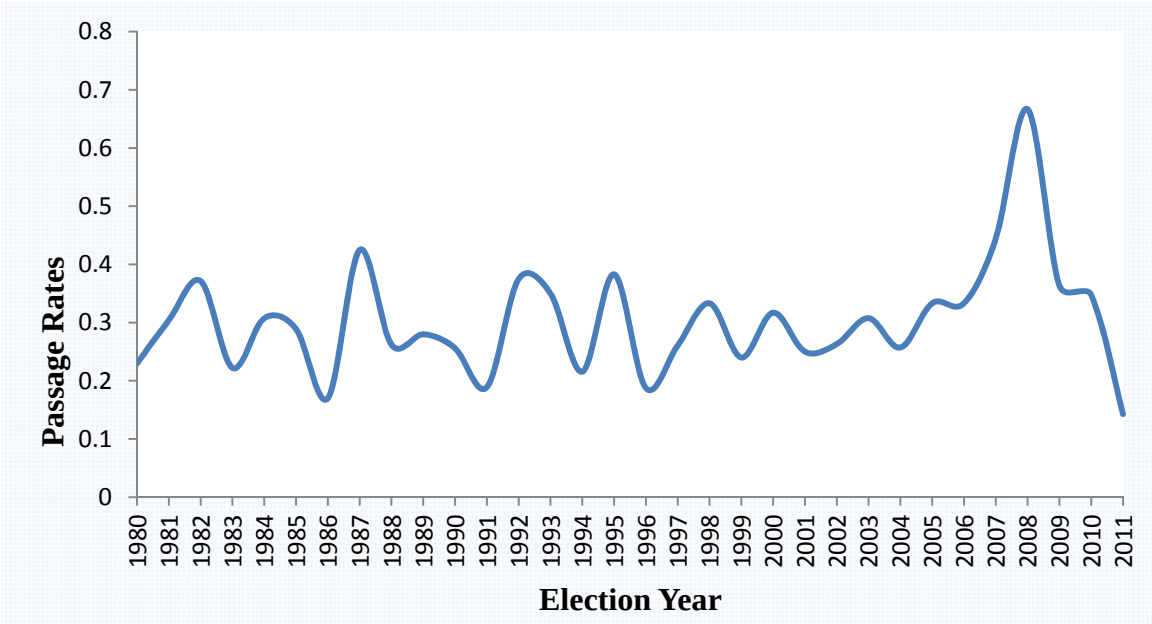


Figure 2: Distribution of votes

This figure plots a histogram of the sample distribution of vote shares (i.e., the percentage of votes in an election in favor of unionization) across 50 equally-spaced bins (with a bin width of 2%). Union election results are from the NLRB over 1980 to 2011.

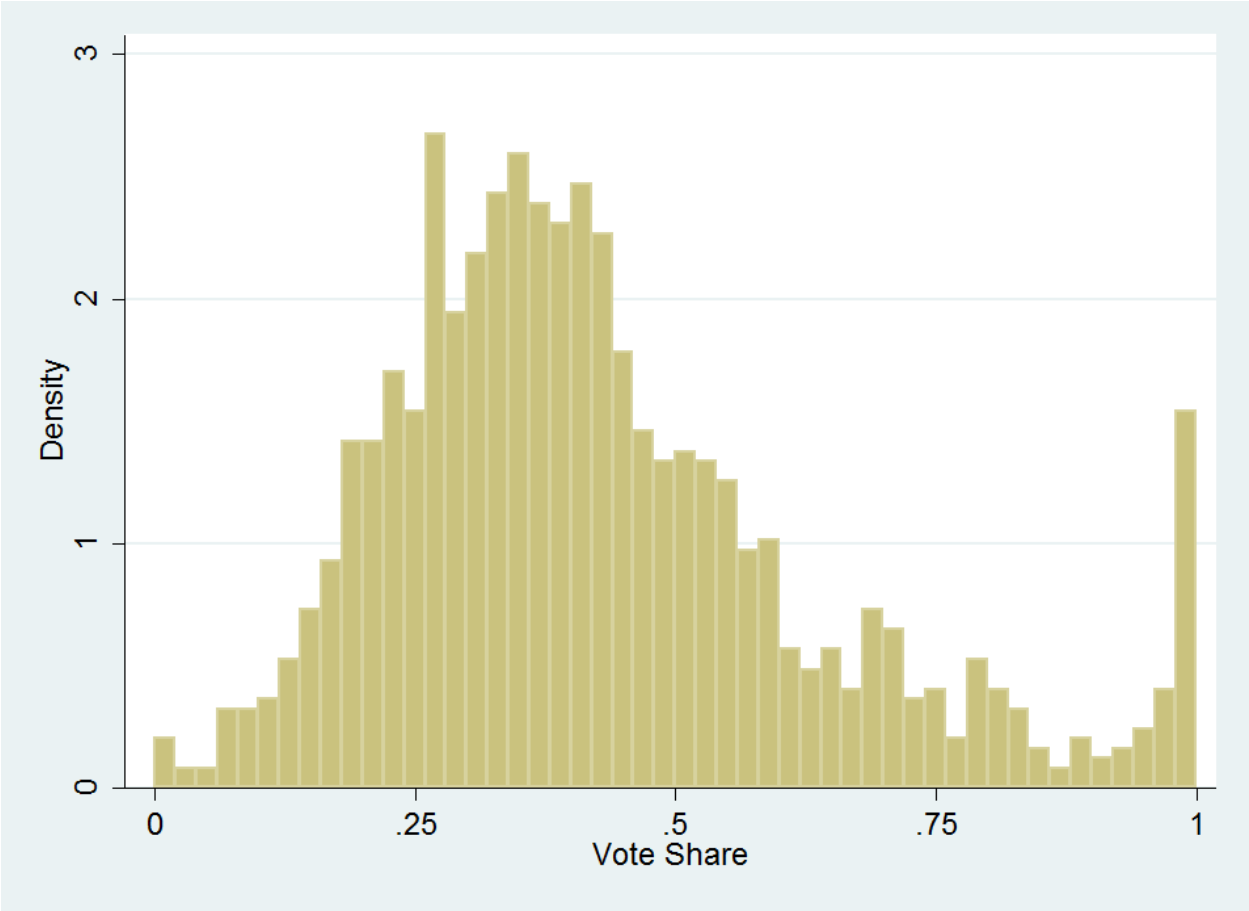


Figure 3: Density of union vote shares

This figure plots the density of union vote share (i.e., the percentage of votes in an election in favor of unionization) following the procedure in McCrary (2008). The x-axis is union vote shares. The dots represent the density estimate for each chosen bin and the bold line is the fitted density function of union vote shares with a surrounding 90% confidence interval. Union election results are from the NLRB over 1980 to 2011.

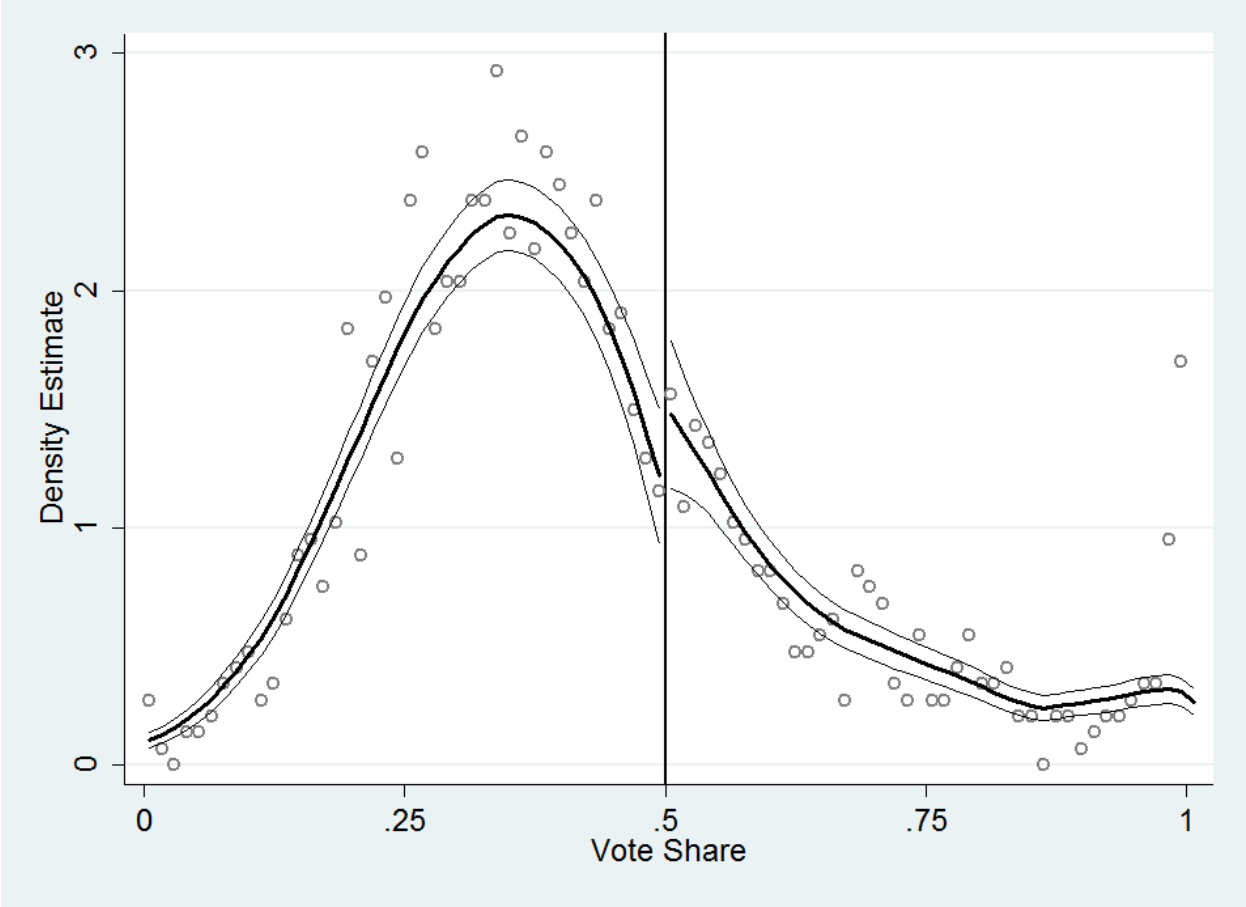


Figure 4: Regression discontinuity plots

This figure presents regression discontinuity plots using a fitted quadratic polynomial estimate with a 90% confidence interval around the fitted value. The x-axis is union vote shares (i.e., the percentage of votes in an election in favor of unionization). The dots depict the average *Dividend Payout* (left) and *Total Payout* (right), defined in Appendix A, in each of the 50 equally-spaced bins (with a bin width of 2%). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat.

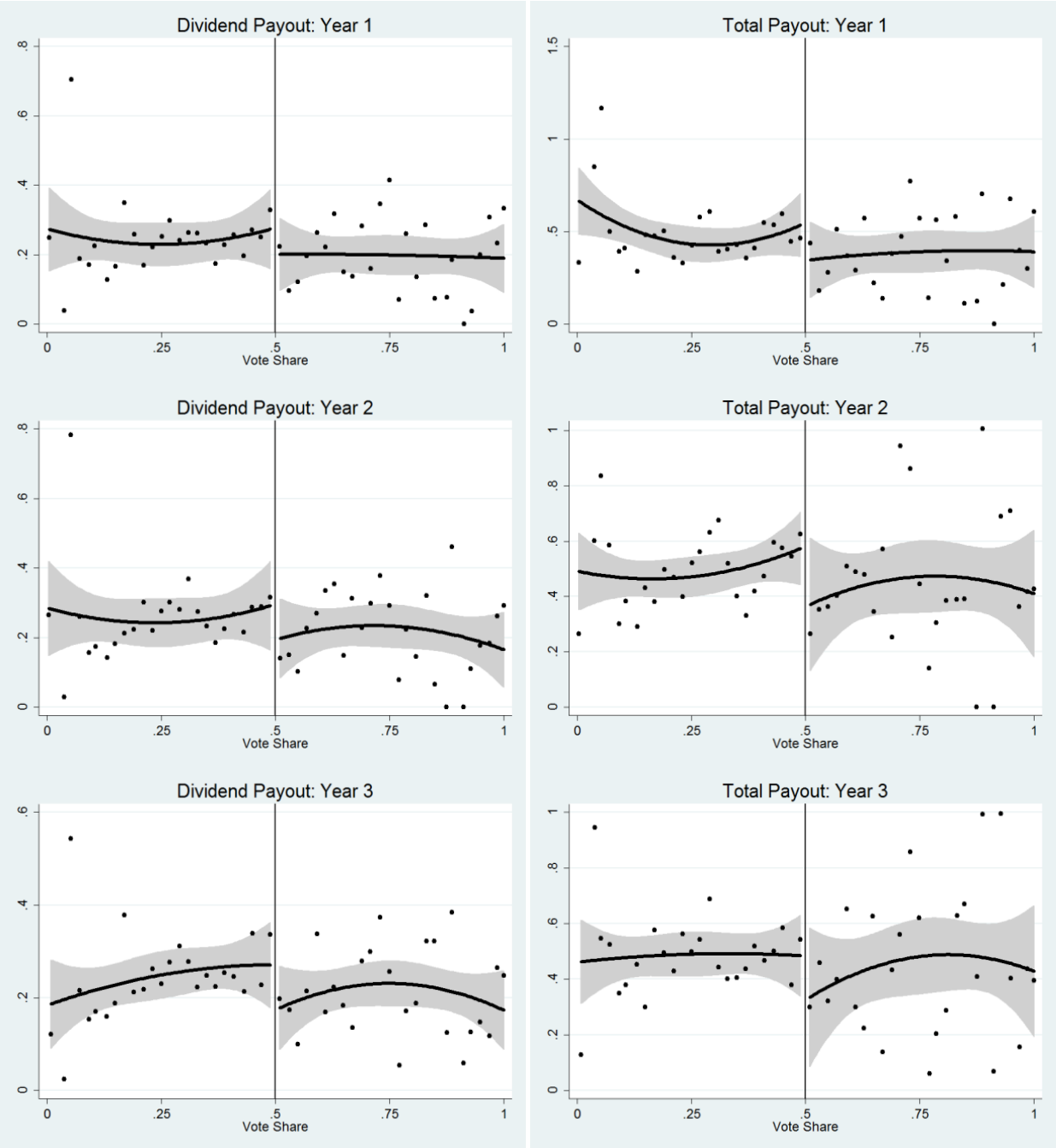


Figure 5: Alternative RD bandwidths

This figure plots the estimated local RD coefficients along with their 90% confidence intervals (on the vertical axis) against alternative values of bandwidths (on the horizontal axis). A value of "100" on the horizontal axis represents the optimal bandwidth suggested by Imbens and Kalyanaraman (2012). "200" means 200% of (i.e., two times of) the optimal bandwidth, "300" means 300%, and so forth. The left-hand side figures describe *Dividend Payout* and the right-hand side ones are for *Total Payout*. Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat.

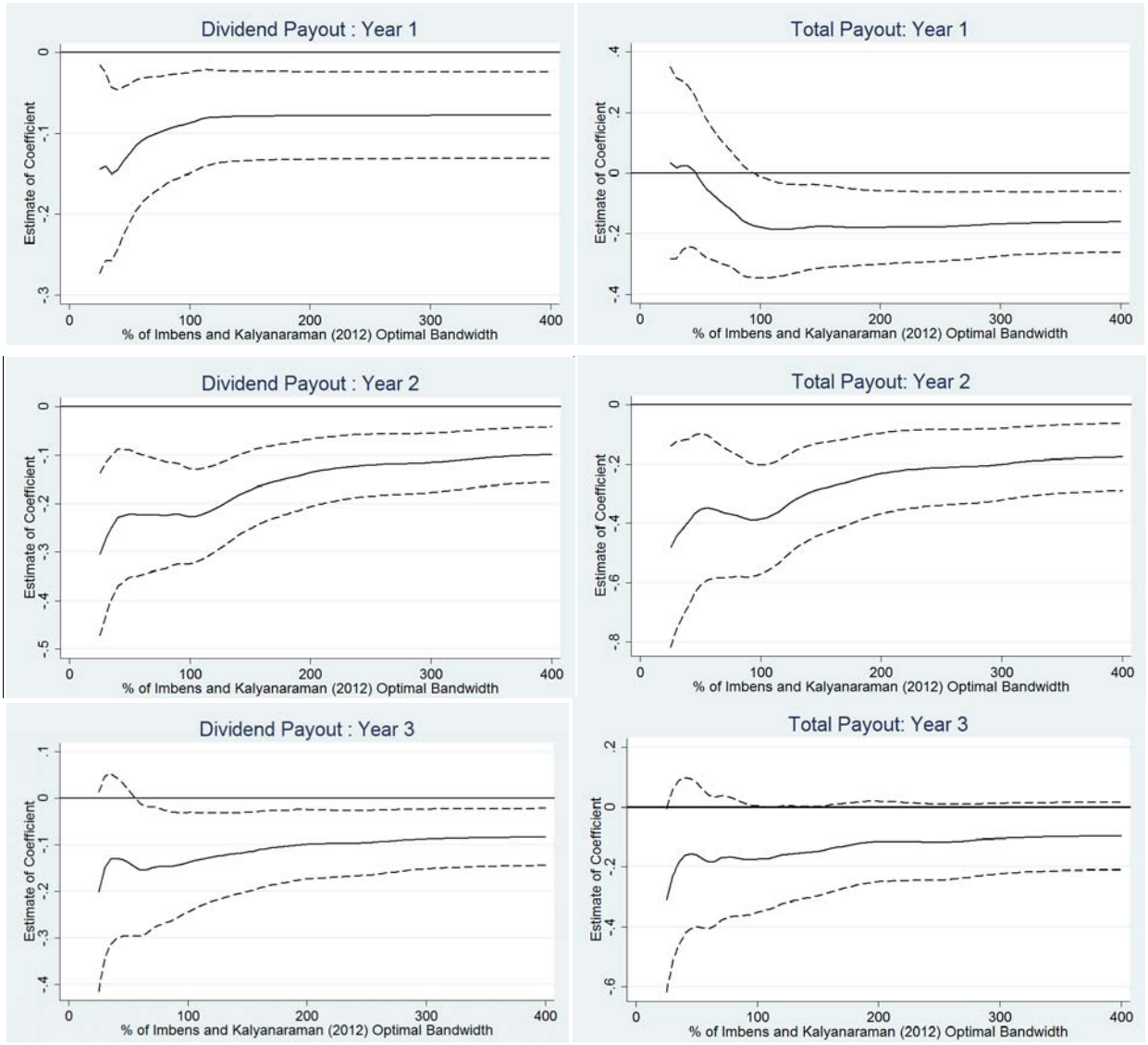


Figure 6: Placebo tests

This figure plots a histogram of the distribution of the local RD estimates from 1000 placebo tests. The x-axis represents the RD estimates from the placebo tests that artificially select an alternative threshold other than 50%. The dashed vertical line represents the RD estimate at the true 50% threshold. The y-axis is the density of the estimated coefficients for *Dividend Payout* (left) and *Total Payout* (right). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat.

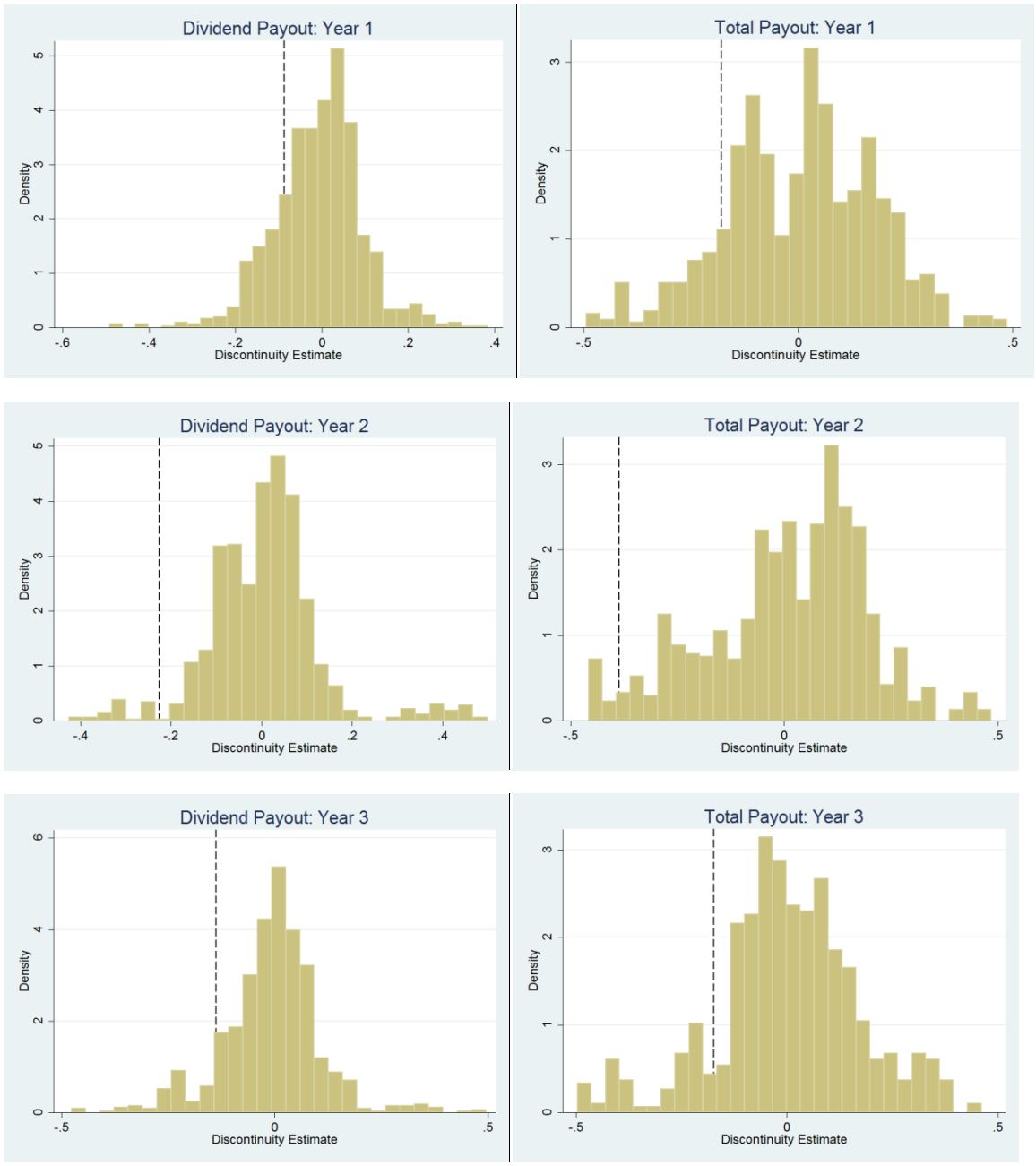


Table 1: Descriptive statistics

This table presents descriptive statistics of our sample, which includes 1,234 union elections taken place between 1980 and 2011. Panel A reports union election statistics. "*Vote Share*" is the percentage of votes in an election in favor of unionization. "*Passage*" is an indicator variable that equals one if a firm is unionized as a result of an election, and zero otherwise. Panel B reports summary statistics of variables used in our study. All variables are defined in Appendix A. Union election results are from the NLRB. Payout and other financial variables are from Compustat.

Panel A: Election statistics

Variables	Obs.	Mean	Median	Std. Dev.
Vote Share	1,234	0.428	0.388	0.209
Passage	1,234	0.287	0.000	0.452

Panel B: Summary statistics

Variables	Mean	25th pct	Median	75th pct	Std. Dev.
Dividend Payout	0.240	0.000	0.131	0.378	0.302
Total Payout	0.449	0.000	0.272	0.657	0.547
Assets(in Billions)	3.834	0.146	0.718	2.942	8.639
Market Value(in Billions)	3.122	0.088	0.468	2.258	7.925
Leverage	0.247	0.107	0.215	0.344	0.192
PPE/Assets	0.641	0.398	0.602	0.843	0.330
Cash/Assets	0.071	0.013	0.035	0.095	0.090
Capex/Assets	0.068	0.032	0.056	0.088	0.052
TobinQ	1.483	1.021	1.254	1.702	0.721
Age	20.385	7.000	17.000	31.000	14.786
Cashflow Volatility	0.041	0.019	0.032	0.052	0.036
Institutional Ownership	0.446	0.211	0.446	0.659	0.261
Sales Growth	0.099	0.003	0.076	0.159	0.204
State GDP Growth	0.067	0.044	0.062	0.089	0.033

Table 2: Regression discontinuity: Global polynomial

This table presents RD results from estimating a polynomial model specified in Equation (1). The dependent variables are *Dividend Payout* (left) and *Total Payout* (right) and the variable of interest is a unionization dummy. Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Panels A, B, and C use polynomial orders of three, two, and four, respectively. T-statistics adjusted for heteroskedasticity and within-firm clustering are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Payout ratios after the elections (polynomial order of three)

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.181***	-0.207***	-0.175**	-0.232**	-0.332***	-0.180
	(-2.711)	(-3.120)	(-2.415)	(-1.994)	(-2.622)	(-1.419)

Panel B: Payout ratios after the elections (polynomial order of two)

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.090*	-0.119**	-0.112**	-0.221**	-0.221**	-0.128
	(-1.864)	(-2.404)	(-2.128)	(-2.504)	(-2.279)	(-1.309)

Panel C: Payout ratios after the elections (polynomial order of four)

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.174**	-0.282***	-0.197**	-0.163	-0.477***	-0.261*
	(-2.038)	(-3.589)	(-2.192)	(-1.107)	(-3.138)	(-1.704)

Table 3: Regression discontinuity: Nonparametric local linear regression

This table presents local linear regression results using the optimal bandwidth following Imbens and Kalyanaraman (2012). Results are reported. In Panel A, the dependent variables are *Dividend payout* (left) and *Total Payout* (right) N years (N=1, 2, or 3) using triangular kernels. In Panel B, the dependent variables are *Dividend payout* (left) and *Total Payout* (right) using rectangular kernels. In Panel C, the dependent variables are other firm characteristics as of the predetermined date (defined as the date one year before the reported closing date of the union elections). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Payout ratios after the elections (triangular kernels)

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.087**	-0.226***	-0.137**	-0.179*	-0.386***	-0.174
	(-2.289)	(-3.787)	(-2.116)	(-1.744)	(-3.447)	(-1.606)

Panel B: Payout ratios after the elections (rectangular kernel)

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.071**	-0.238***	-0.145**	-0.222**	-0.383***	-0.162
	(-1.880)	(-3.717)	(-2.237)	(-2.079)	(-3.248)	(-1.449)

Panel C: Other firm characteristics at the predetermined date

Variable	Coefficient	Z-statistic
Dividend Payout	-0.041	-0.861
Total Payout	0.031	0.293
Assets	-0.235	-0.179
Market Value	-1.635	-1.366
ROA	-0.006	-0.472
Leverage	0.050	1.305
PPE/Assets	0.004	0.066
Capx/Assets	0.012	1.130
Cash/Assets	-0.011	-0.939
TobinQ	-0.131	-1.212
Age	-2.697	-1.078
Cashflow Volatility	0.012	1.438
Institutional Ownership	-0.050	-1.071
Sales Growth	-0.055	-1.160
State GDP Growth	-0.002	-0.524

Table 4: Robustness tests for local linear regressions

This table presents robustness tests for local linear regressions using the optimal bandwidth following Imbens and Kalyanaraman (2012). Results using triangular kernels are reported. Panel A reports local RD results using alternative payout measures. Row (1) examines *Dividend Yield* (left) and *Total Yield* (right). Row (2) examines *Dividend Sales Ratio* and *Total Payout Sales Ratio*. Row (3) examines *Dividend over Free Cash Flows* and *Total Payout over Free Cash Flows*. Row (4) examines *Dividend Per Share* and *Total Payout Per Share*. Panel B reports local RD results based on alternative ways of constructing the sample. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). Row (1) keeps the first (earliest) election held by a firm in the fiscal year. Row (2) requires a firm holding an election to have no other elections both in the previous and the next three years. Row (3) only analyzes firms holding exactly one union election throughout the whole sample period. Row (4) keeps all elections held by the same firm in our sample period. Row (5) examines union elections with more than 100 eligible employees. Row (6) examines firms with non-zero payouts before union elections. Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Alternative payout measures

(1)	***Dividend Yield*** $_{t+N}$			***Total Yield*** $_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.011***	-0.010***	-0.008***	-0.015***	-0.012**	-0.009
	(-3.272)	(-2.754)	(-2.590)	(-2.855)	(-2.038)	(-1.631)
(2)	***Dividend Sales Ratio*** $_{t+N}$			***Total Payout Sales Ratio*** $_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.006**	-0.005***	-0.004	-0.010***	-0.007	-0.004
	(-2.186)	(-2.952)	(-1.620)	(-3.034)	(-1.461)	(-0.689)
(3)	***Dividend over Free Cash Flows*** $_{t+N}$			***Total Payout ver Free Cash Flows*** $_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.007	-0.172**	-0.170**	-0.028	-0.188	-0.394***
	(-0.112)	(-1.975)	(-1.977)	(-0.269)	(-1.338)	(-2.624)
(4)	***Dividend Per Share*** $_{t+N}$			***Total Payout Per Share*** $_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.191**	-0.182**	-0.122	-0.413***	-0.427**	-0.142
	(-2.207)	(-2.357)	(-1.177)	(-2.589)	(-2.020)	(-0.562)

Panel B: Alternative ways of constructing the sample

	Dividend Payout $_{t+N}$			*Total Payout* $_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
(1) First election in a year						
Unionization	-0.121**	-0.220***	-0.107**	-0.167*	-0.365***	-0.147
	(-2.321)	(-3.289)	(-1.968)	(-1.650)	(-3.061)	(-1.318)
(2) No other elections in the previous and next three years						
Unionization	-0.133**	-0.176**	-0.139*	-0.258**	-0.323**	-0.167
	(-2.063)	(-2.341)	(-1.886)	(-2.564)	(-2.426)	(-1.285)
(3) Firms with only one election throughout sample period						
Unionization	-0.216*	-0.176**	-0.351***	-0.161	-0.292**	-0.488**
	(-2.609)	(-2.238)	(-2.593)	(-1.083)	(-2.429)	(-2.352)
(4) All elections						
Unionization	-0.066*	-0.090*	-0.058*	0.008	-0.210***	-0.083
	(-1.885)	(-2.560)	(-1.681)	(0.110)	(-3.109)	(-1.373)
(5) Elections with more than 100 eligible employees						
Unionization	-0.172**	-0.222***	-0.126	-0.249**	-0.389***	-0.134
	(-2.180)	(-2.751)	(-1.517)	(-2.064)	(-2.651)	(-1.071)
(6) Firms with non-zero payouts before union elections						
Unionization	-0.215**	-0.255***	-0.093	-0.222**	-0.393**	-0.075
	(-2.421)	(-2.587)	(-1.263)	(-2.165)	(-2.247)	(-0.653)

Table 5: Subsample analysis based on right-to-work laws and work stoppage provisions
This table presents local linear regression results using the optimal bandwidth following Imbens and Kalyanaraman (2012) for firms located in states either without right-to-work laws or with work stoppage provisions (Panel A) versus those located in states with right-to-work laws but without work stoppage provisions (Panel B). Results using a triangular kernel are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: States either without right-to-work laws or with work stoppage provisions

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.195**	-0.279***	-0.181**	-0.141*	-0.493***	-0.103
	(-2.351)	(-3.669)	(-2.060)	(-1.676)	(-3.626)	(-0.959)

Panel B: States with right-to-work laws but without work stoppage provisions

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.117	-0.073	-0.039	-0.038	-0.139	-0.237
	(-0.821)	(-0.683)	(-0.284)	(-0.101)	(-0.714)	(-1.226)

Table 6: Subsample analysis based on operational leverage

This table presents local linear regression results using the optimal bandwidth following Imbens and Kalyanaraman (2012) for firms with high operational leverage (Panel A) versus firms with low operational leverage (Panel B). We follow Mandelker and Rhee (1984) and estimate a firm's operational leverage as the elasticity of its earnings before interest and taxes (EBIT) with respect to its sales, using the twelve most recent quarterly observations before the predetermined date of the election. We then split the firms into two subsamples based on the sample median operational leverage. Results using a triangular kernel are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: High operational leverage

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.219***	-0.390***	-0.243**	-0.312***	-0.520***	-0.264*
	(-3.065)	(-3.611)	(-2.286)	(-2.682)	(-2.841)	(-1.947)

Panel B: Low operational leverage

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.088	-0.080	0.007	-0.119	-0.237	0.240
	(-0.981)	(-0.863)	(0.054)	(-0.946)	(-1.397)	(1.074)

Table 7: Nonparametric local linear regression on cumulative abnormal returns around union elections

This table presents local linear regression results on the cumulative abnormal returns (CARs) around union elections' closing dates (the event dates). Results using a triangular kernel are reported. The dependent variables are CARs calculated from the market model. We examine event windows from day -3 to day +3, from day -1 to day +3, and from day 0 to day +5. Panel A examines the CARs using all the elections. Panels B1 and B2 examine the CARs for subsamples separated by the level of operational leverage. Union election results are from the National Labor Relations Board (NLRB) over 1980 to 2011. Z-statistics are presented in the parenthesis. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: All elections

	CARs		
	[-3, 3]	[-1, 3]	[0, 5]
Unionization	-0.002	-0.015	-0.012
	(-0.075)	(-1.236)	(-1.086)

Panel B1: High operational leverage

	CARs		
	[-3, 3]	[-1, 3]	[0, 5]
Unionization	-0.051**	-0.038**	-0.023*
	(-2.055)	(-2.138)	(-1.714)

Panel B2: Low operational leverage

	CARs		
	[-3, 3]	[-1, 3]	[0, 5]
Unionization	0.026	-0.004	0.010
	(0.709)	(-0.132)	(0.349)

Table 8: Multivariate analysis of cumulative abnormal returns around union elections
This table presents multiple regressions of cumulative abnormal returns (CARs) around union elections on an operational leverage dummy (*OL dummy*), which equals one if a firm's operational leverage is above the sample median and zero otherwise. The dependent variables are CARs calculated from the market model using the union elections' closing dates as the event dates. We examine event windows from day -3 to day +3, from day -1 to day +3, and from day 0 to day +5. Definitions of control variables are in Appendix A. Union election results are from the National Labor Relations Board (NLRB) over 1980 to 2011. Only winning union elections are included in this analysis. Each regression includes a separate intercept and controls for industry fixed effects (based on two-digit SIC codes). T-statistics are presented in the parenthesis. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dep. Var.	*CARs*					
	(1)	(2)	(3)	(4)	(5)	(6)
Event Window	[-3,3]	[-3,3]	[-1,3]	[-1,3]	[0,5]	[0,5]
OL dummy	-0.022*	-0.020*	-0.018**	-0.015*	-0.018*	-0.017
	(-1.888)	(-1.784)	(-2.001)	(-1.723)	(-1.725)	(-1.381)
Market Value		0.004		0.003		0.002
		(1.437)		(1.444)		(0.915)
ROA		-0.075		-0.115		-0.081
		(-0.816)		(-1.401)		(-0.925)
PPE/Assets		-0.019		-0.029		-0.044*
		(-0.674)		(-1.301)		(-1.730)
Capx/Assets		0.081**		0.058*		0.037
		(2.332)		(1.888)		(0.936)
Leverage		0.002		-0.001		-0.015
		(0.189)		(-0.098)		(-1.614)
Tobin's Q		0.187		0.204*		0.220
		(1.226)		(1.774)		(1.473)
Observations	199	189	199	189	199	189
R-squared	0.249	0.326	0.241	0.306	0.192	0.244

Table 9: Subsample analysis based on financial constraints

This table presents local linear regression results using the optimal bandwidth following Imbens and Kalyanaraman (2012) for firms that are more financially constrained (Panels A1, B1, C1 and D1) versus firms that are less financially constrained (Panels A2, B2, C2, and D2). Panels A1 and A2 use the Kaplan-Zingales (KZ) index to proxy for a firm's financial constraints. Firms with KZ index below sample median are less constrained, and firms with KZ index above sample median are more constrained. Panels B1 and B2 use the Whited-Wu (WW) index, Panels C1 and C2 use the Hadlock and Pierce (SA) index, and Panels D1 and D2 use the Hoberg- Maksimovic (HM) text-based index. Results using a triangular kernel are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A1: More financially constrained (the KZ Index)

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.243***	-0.301***	-0.253**	-0.362***	-0.598***	-0.236
	(-2.976)	(-2.705)	(-2.494)	(-3.026)	(-3.584)	(-1.535)

Panel A2: Less financially constrained (the KZ Index)

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.026	-0.073	-0.017	-0.001	0.064	0.000
	(-0.269)	(-0.766)	(-0.160)	(-0.004)	(0.307)	(0.002)

Panel B1: More financially constrained (the WW Index)

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.161	-0.163*	-0.302***	0.010	-0.232	-0.127
	(-1.604)	(-1.842)	(-2.589)	(0.068)	(-1.313)	(-1.054)

Panel B2: Less financially constrained (the WW Index)

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.228**	-0.439***	-0.092	-0.474***	-0.780***	-0.245
	(-2.359)	(-4.272)	(-1.069)	(-3.228)	(-4.166)	(-1.222)

Panel C1: More financially constrained (the SA Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.104*	-0.125	-0.197*	-0.197	-0.186	-0.199
	(-1.720)	(-1.412)	(-1.889)	(-1.605)	(-1.508)	(-1.378)

Panel C2: Less financially constrained (the SA Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.100	-0.229***	-0.050	-0.108	-0.626***	-0.090
	(-1.122)	(-3.140)	(-0.468)	(-0.810)	(-2.924)	(-0.543)

Panel D1: More financially constrained (the HM Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	0.208	-0.099	0.403	0.280	-0.560***	1.033**
	(1.001)	(-0.689)	(1.464)	(0.646)	(-2.867)	(1.973)

Panel D2: Less financially constrained (the HM Index)

	Dividend Payout$_{t+N}$			*Total Payout*$_{t+N}$		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.356**	-0.348**	-0.117	-0.690***	-1.029***	-0.308
	(-2.263)	(-2.335)	(-1.107)	(-3.091)	(-3.223)	(-1.505)

Table 10: Nonparametric local linear regression of other financial policies

This table presents local linear regression results for other financial policies. Panel A examines a firm's capital structure decisions, measured by *Δ Total Debt over Earnings*. Panel B examines a firm's cash holding decisions, measured by *Δ Cash over Earnings*. Panel C examines a firm's working capital management, measured by *Δ Working Capital over Earnings.* Results using the optimal bandwidth following Imbens and Kalyanaraman (2012) and a triangular kernel are reported. Union election results are from the NLRB over 1980 to 2011. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Change in total debt

	Δ Total Debt over Earnings $_{t+N}$		
	N=1	N=2	N=3
Unionization	0.318	0.002	-0.091
	(1.099)	(0.007)	(-0.299)

Panel B: Change in cash holdings

	Δ Cash over Earnings $_{t+N}$		
	N=1	N=2	N=3
Unionization	0.851	0.178	-0.101
	(1.245)	(0.530)	(-0.232)

Panel C: Change in working capital

	Δ Working Capital over Earnings $_{t+N}$		
	N=1	N=2	N=3
Unionization	2.141*	2.037***	0.833
	(1.784)	(2.682)	(1.056)

Table 11: Subsample analysis of alternative explanations: Employee preferences
This table presents local linear regression results on subsamples based on the fraction of firm equity ownership by pensions and the percentage of pension holdings invested in the firm's own securities. Panel A examines firms with larger pension ownership (i.e., those whose equity market value held by employees through pension plans exceeds sample median), and Panel B examines those with smaller pension ownership. Panel C examines firms with a larger fraction of pension holdings invested in the firms' own securities (i.e., those whose percentage pension holdings in the form of their own securities are above the sample median), and Panel D examines those with a smaller percentage of pension holdings invested in the firms' own securities. Results using the optimal bandwidth following Imbens and Kalyanaraman (2012) and a triangular kernel are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). We use union election results from the NLRB over 1992 to 2007. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Larger pension ownership

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.351***	-0.416***	-0.021	-0.437**	-1.136***	0.287
	(-3.699)	(-2.824)	(-0.168)	(-2.159)	(-3.091)	(1.148)

Panel B: Smaller pension ownership

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.141	-0.133	0.055	-0.245	-0.191	0.103
	(-0.644)	(-0.928)	(0.212)	(-0.626)	(-0.548)	(0.239)

Panel C: Larger fraction of pension holdings invested in the firm's own securities

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.151	-0.319**	-0.055	-0.162	-0.773**	0.357
	(-0.934)	(-2.061)	(-0.328)	(-0.502)	(-2.427)	(1.470)

Panel D: Smaller fraction of pension holdings invested in the firm's own securities

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.383	-0.321**	0.110	-0.659**	-0.395	-0.042
	(-1.613)	(-1.974)	(0.488)	(-2.392)	(-0.887)	(-0.162)

Table 12: Subsample analysis of alternative explanations: Agency conflicts

This table presents local linear regression results on subsamples based on the level of corporate governance measured by the entrenchment index (E-index). Panel A examines firms with an E-index that is below the sample median (i.e., with better corporate governance) and Panel B examines those with an E-index above sample median (i.e., with worse corporate governance). Results using the optimal bandwidth following Imbens and Kalyanaraman (2012) and a triangular kernel are reported. The dependent variables are *Dividend Payout* (left) and *Total Payout* (right). We use union elections over 1990 to 2006. Payout data are from the Compustat. Z-statistics are displayed in the parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Better corporate governance

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.359***	-0.415***	-0.424***	-0.376**	-0.684***	-0.338***
	(-4.420)	(-5.093)	(-4.813)	(-2.539)	(-3.709)	(-2.876)

Panel B: Worse corporate governance

	***Dividend Payout*$_{t+N}$**			***Total Payout*$_{t+N}$**		
	N=1	N=2	N=3	N=1	N=2	N=3
Unionization	-0.127	-0.033	-0.027	-0.200	0.111	-0.066
	(-0.770)	(-0.206)	(-0.292)	(-0.751)	(0.552)	(-0.358)