The (non-) effect of labor unionization on firm risk: Evidence from the options market

Mohamed Ghaly, Alexandros Kostakis, Konstantinos Stathopoulos*

Journal of Corporate Finance, forthcoming

Abstract

Labor unionization has no causal effect on firm risk. Using a regression discontinuity design to study the impact of labor union elections on option-implied firm risk, we find that unionization per se does not affect investor perceptions about firm price, tail, or variance risk. This finding is robust to studying very short (5-trading day) and long (up to 2-year) windows around the elections. Moreover, there is no unionization effect on firm risk either in subsets of firms facing strong union bargaining power, or with characteristics that prior literature identifies as important determinants of the effect of unionization on firm outcomes.

JEL Classifications: J51, G30, G32.

Keywords: Unionization, labor power, option-implied firm risk, regression discontinuity design.

*The authors thank the Editor and three anonymous reviewers for their comments and suggestions. Kostakis and Stathopoulos acknowledge financial support from the Alliance MBS Strategic Research Investment Fund. Ghaly, m.ghaly@lancaster.ac.uk, Lancaster University Management School, Bailrigg, Lancaster, LA1 4YX; Kostakis, a.kostakis@liverpool.ac.uk, University of Liverpool Management School, Chatham Street, Liverpool L69 7ZH & Alliance Manchester Business School, The University of Manchester, Booth Street West, Manchester, M15 6PB; Stathopoulos, k.stathopoulos@manchester.ac.uk, Alliance Manchester Business School, The University of Manchester, Booth Street West, Manchester, M15 6PB.

1. Introduction

Despite the decline in union membership rates during the last decades,[1] the debate regarding the effect of labor unions on firm policy and performance is still ongoing. It is theoretically well understood that labor unions are powerful stakeholders who could, in principle, influence firm pay policy, employment, productivity, profitability, value, capital structure and investment decisions as well as survival and liquidation (see for example, Freeman, 1981; Abraham and Medoff, 1984; Freeman and Medoff, 1984; Ruback and Zimmerman, 1984; Connolly et al., 1986; Abowd, 1989; Hirsch, 1991; Hirsch, 2004; Stanfield and Tumarkin, 2018). However, it is only recently that empirical studies have focused on the *causal* effect of unionization on these corporate outcomes producing mixed evidence in the process.[2]

The related question of whether labor unionization affects firm risk has surprisingly attracted less attention in the literature and the existing empirical evidence is either circumstantial and implicit (see Chen et al., 2011, 2012; Lee and Mas, 2012) or focuses solely on bankruptcy risk (see DiNardo and Lee, 2004; Qiu and Shen, 2017; Campello et al., 2018). Empirical evidence is scarce because it is rather challenging to measure changes in firm risk around a unionization event as well as to identify a causal relationship between unionization and firm risk. Our study contributes to the literature by directly testing whether labor unionization has a causal effect

[1] Even though union membership has declined significantly in the U.S. over the years (DiNardo and Lee, 2004; Agrawal, 2012), there were approximately 15 million workers in 2017 who were union members according to the Bureau of Labor Statistics. They collectively represented more than 10 percent of the entire U.S. workforce. The highest union representations appear in labor intensive sectors, such as manufacturing, which have a significant contribution to both U.S. gross output and employment.

[2] DiNardo and Lee (2004) find that unionization has almost no impact on wages, employment, productivity, or business survival rates. More recently, Qiu and Shen (2017) find no material change in profitability, operating performance, book leverage, operating leverage, capital investment, tangibility or cash holdings due to labor unionization. On the other hand, Matsa (2010) finds that firms exposed to greater labor union bargaining power tend to use more financial leverage. Schmalz (2015) shows that, whereas the causal effect of unionization on cash holdings and leverage is close to zero on average, it is significant and heterogeneous across financially constrained and unconstrained firms. Bradley et al. (2017) argue that unionization leads to a reduction in R&D expenditure and the productivity of inventors. He et al. (2020) find that unionization has a negative effect on the dividend payout ratio, and Huang et al. (2017) show that passing a union election leads to a reduction in CEO compensation. In addition, Lie and Que (2019) document that union elections increase the probability of asset sales and render takeovers less likely, whereas Frandsen (2020) provides evidence that unionization sharply reduces employment, payroll, average worker earnings, and survival probability at the establishment level.

on firm risk. To this end, we utilize information on labor union elections and we employ measures of firm risk, as priced in the equity options market.

There are various channels through which labor unions can affect firm risk. First, they can increase firms' operating leverage and decrease their operating flexibility. In particular, through collective bargaining agreements, powerful labor unions can make wages stickier and oppose layoffs, rendering the adjustment of labor stock slower and more costly. Moreover, unions may delay or altogether block restructurings and plant closures, making it harder for firms to adjust their physical capital and, as a result, aggravate the cost of investment irreversibility. In both cases, a firm may end up with unproductive labor and capital stock, which could increase its net cash flow risk and render it more vulnerable to adverse shocks, especially during recessions. Hence, to the extent that investment irreversibility and operating leverage are sources of systematic risk (see inter alia Rosett, 2001; Zhang, 2005; Cooper, 2006; Novy-Marx, 2011), unionization may increase firm risk.

The second channel through which unions can affect firm risk is by influencing its financial leverage, provided that the latter is thought to affect the variability of firm residual income as well as its risk of becoming financially distressed. However, there are opposing views with respect to the direction of the effect that unionization has on financial leverage. On the one hand, the firm may strategically increase its leverage to improve its bargaining position towards labor unions, since the latter would be less able to extract rents from future cash flows that are already committed to debt holders (see Bronars and Deere, 1991; Dasgupta and Sengupta, 1993; Perotti and Spier, 1993; Matsa, 2010; Myers and Saretto, 2016). On the other hand, the firm may choose to increase its financial flexibility, and hence decrease its leverage, to offset the reduction in its operating flexibility due to unionization (see Gamba and Triantis, 2008; Simintzi et al., 2015; Woods et al., 2019). More generally, unions are expected to favor and promote financial policies that reduce the likelihood of default, inducing managers to choose lower financial leverage so as to reduce employees' exposure to unemployment risk and avoid the loss of firm-specific human capital, wages, and pension benefits (Berk et al., 2010). For example, unions may deter takeover activities (Dessaint et al., 2017; Lie and Que, 2019; Tian and Wang, 2020), which are typically associated with an increase in financial leverage.

There are additional channels through which unionization may affect firm risk. In particular, unions may increase strike risk, introducing another source of uncertainty regarding firm operations. They could also oppose the adoption of new technologies or encourage shirking, rendering the firm vulnerable to competitive forces and economy-wide changes, and hence increase firm risk. To the contrary, by providing employment insurance, unions could improve employees' morale, reduce their turnover, increase productivity, and thus decrease operational risk. Unions can also play an active shareholder role through their pension funds, improve corporate governance, reduce informational asymmetries with the management, and therefore reduce firm risk. They can also discourage management from undertaking excessively risky investment projects. In addition, organized labor can mobilize the media and exercise political pressure to bail out the firm in the case of financial distress; in that respect, unions may help reduce bankruptcy risk. Given the above discussion, the effect of unionization on firm risk is not clear *a priori*, but remains an empirical question.

Identifying the causal effect of labor unionization on firm risk is rather challenging because of the endogenous nature of unionization. In particular, there may be unobserved firm characteristics that affect both firm risk and the unionization decision (omitted variables concern). In addition, it may well be that employees in more risky firms are more likely to establish a union to protect their interests (reverse causality concern). To overcome this endogeneity issue, we utilize secret ballot union election results from the National Labor Relations Board (NLRB) and resort to a Regression Discontinuity Design (RDD), which exploits local variations in the election vote share that lead to discrete changes in the union legal status. Under standard regularity conditions, which we verify in the data, elections that pass or fail by a small margin generate "locally exogenous" variation in the unionization status of the firm, allowing us to examine whether the latter has a causal effect on firm risk.

To measure firm risk, we use information from the equity options market. This choice presents a number of advantages relative to alternative approaches using information from the stock or bond markets or even accounting information. First, options come with different strike prices, allowing us to capture different dimensions of equity risk, even if these do not subsequently materialize. Following the unionization event, the stock price may experience fluctuations (price risk), these price fluctuations may be large (tail risk), and stock return volatility may also substantially vary (variance risk). We measure stock price risk via the implied volatility of at-the-money equity options (*ATM*). Tail risk is captured by the relative expensiveness of deep out-of-the-money puts, which provide protection against large price drops. In particular, *LSKEW* is defined as the difference between the implied volatility of deep out-of-the-money puts and the implied volatility of at-the-money options, reflecting the left slope of the implied volatility curve. Finally, we compute the difference between the implied and realized variances of the underlying stock return, i.e., the variance risk premium (*VRP*), to measure variance risk.

The second advantage of this approach is that using options with short maturities allows us to isolate the causal effect on firm risk due to the unionization event. In particular, comparing the level of risk extracted from short-maturity options traded right after the unionization election with the level of risk extracted from short-maturity options just before the unionization election enables us to attribute the corresponding shift to the election outcome, because the latter is certainly the dominant firm-level event during the life of these options. Moreover, option-implied measures of risk are typically available on a daily basis. Hence, we can focus on very short windows around the unionization event to further enable causal identification. To the contrary, using realized stock returns or accounting information to measure risk typically requires much longer estimation windows that may blur the potential causality effect due to unionization with confounding firm-level events.

Comparing the shift in the level of risk around the unionization election across close union winners and close union losers, we can derive an estimate of the causal effect of unionization on firm risk. This approach is equivalent to a difference-in-differences estimation and inherently controls for firm fixed effects that may affect its risk level.

Our empirical tests show that unionization has no causal effect on firm risk. In particular, we find that close union winners do not exhibit a significantly different change in price, tail or variance risk relative to close union losers. These results are robust to the length of the window used around the unionization election to measure the option-based risk variables and they remain virtually identical when we use either the election or the case closure date, instead of the tally date, as the unionization event date. Moreover, our benchmark results remain unchanged if we adjust the firm-level shifts in the risk measures for contemporaneous market-wide shifts, taking into account potential time fixed effects. These results remain also robust if we alternatively employ the percentage change in these option-based risk measures, instead of the change in their levels, to control for a potential cross-sectional scale effect, or if we decompose option-implied price risk into its systematic and unsystematic components.

Our benchmark analysis estimates the average treatment effect due to unionization. However, this treatment effect might be substantially heterogeneous across subsets of firms. A prime suspect of heterogeneity is the bargaining power that elected unions are expected to have. To examine this issue, we partition our sample based on whether the election takes place in a state that has adopted Right-to-Work (RTW) laws or not, because unions are expected to have considerably stronger bargaining power in states without RTW laws. This analysis shows again no evidence of a causal effect of unionization on firm risk across both subsamples.

In addition, we examine the role of labor strikes prior to union elections on investor perceptions about the impact of unionization on firm risk. Labor strikes are highly disruptive events denoting substantial disagreement between managers and employees. If investors perceive union elections to be either an escalation of manager-employee conflicts or a mechanism for reducing further disruptions, they might significantly change their perceptions about the impact of unionization on firm risk. However, we find no evidence of a strong causal unionization effect for firms with or without strike actions prior to union elections.

Furthermore, recent literature highlights the differential effect of unionization on corporate outcomes between firms that are financially constrained or distressed and firms that are not (Chen et al., 2012; Schmalz, 2015; Campello et al., 2018). Thus, we also explore this potential source of heterogeneity by splitting our cross-section into subsamples of firms based on whether they are financially constrained or distressed. Again, we fail to find a significant change in firm risk following unionization for most of the subsets. Hence, we confirm that unionization *per se* is not a driver of firm risk, as this is priced in the options market.

Given the absence of a causal effect on firm risk around the unionization event, we additionally examine whether such an effect arises in the longer term. Arguably, it may take longer for market participants to understand and quantify the impact of unionization on firm operations and its risk profile. In that case, a change in firm risk might be priced with a substantial delay. Consistent with this argument, Lee and Mas (2012) find that the unionization effect on firm value takes 15 to 18 months to fully materialize. They attribute this persistent mispricing to limits-to-arbitrage. Admittedly, this argument is rather weak in our setting since limits-to-arbitrage are much less severe in the equity options market relative to the stock market. In fact, the options market may well lead the stock market with respect to price discovery (see inter alia Easley et al., 1998; Pan and Poteshman, 2006; Ge et al., 2016; Stilger et al., 2017). Therefore, we do not expect the options market to exhibit a delayed reaction to the unionization event. Nevertheless, we test this possibility and find no evidence that union winners experience a significant change in firm risk up to 2 years after the election date. This result holds true both for close union winners and across the entire sample of unionized and non-unionized firms.

Our study contributes to the strand of literature that examines the effect of labor unions on firm risk. First, recent studies argue for a causal effect of unionization on corporate outcomes and decision making. For example, Bradley et al. (2017) show that unionization leads to a reduction to R&D expenditure and productivity of inventors whereas Lie and Que (2019) demonstrate that union elections increase the probability of asset sales and reduce the likelihood of takeovers. Collectively, these outcomes, as well as other potential changes to firm policies due to unionization not yet identified in the literature, could have an impact on the risk profile of unionized firms as long as they are meaningful and do not offset each other in terms of their aggregate effect on firm risk. Our approach of measuring investor perceptions over the direct changes to (different kinds of) firm risk from a unionization event allows us to capture the *net* effect of unionization on risk, which is arguably more important to investors and policy makers, especially if these changes to the risk profile of individual firms are likely to propagate to financial markets through similar events.

Second, there is conflicting empirical evidence in the literature as to the impact of labor unionization on firm characteristics, cost of capital, and firm risk. For example, Chen et al. (2011) find that the degree of unionization at the industry level is positively associated with firms' implied cost of equity capital, attributing this finding to higher equity risk due to a reduction in operating flexibility and an increase in operating leverage. However, they find no evidence that the degree of industry unionization is associated with higher market (beta) risk, but only with higher exposure to the value (HML) factor. In contrast, Chen et al. (2012) find that firms in more unionized industries have lower bond yields, arguing that unions reduce debt risk by supporting less risky corporate policies, and reducing the probability of bankruptcy filing. Moreover, Lee and Mas (2012) report insignificant changes to firm betas after unionization but do not carry out a proper causality test for the impact of unionisation on firms' betas.

Even if one ignores the numerous studies reporting only associations and concentrates on the few studies arguing for causal effects, the evidence is still mixed. For instance, Matsa (2010) finds that firms exposed to greater labor union bargaining power tend to use more financial leverage. Schmalz (2015) reports increases to net, operating and market leverage as a result of successful union elections but only in financially constrained firms. At the same time, Qiu and Shen (2017) find no material change to book and operating leverage due to labor unionization. In sum, the issue of whether unionization affects firm characteristics is far from being settled and there is very little causal evidence on whether unionization affects firm risk. Our study aims to fill in this gap in the literature.

Third, our research design and empirical approach is better suited for capturing a causal effect of union elections on firm risk relative to prior studies. In particular, we use daily data from the equity options market which allows us to study very short (5-trading day) windows around the elections. In contrast, Lee and Mas (2012) introduce in a standard CAPM interactions of the market return with dummy variables for eight six-month periods within the -24 to 24 month interval, thus allowing for betas to change only at six-month intervals. Qiu and Shen (2017) look at the differences in annual default probability between the year prior to the election up to three years after the unionization event. Lastly, Campello et al. (2018) compare the differences in annual distance-to-default from the year prior to the unionization event up to 5 years after the election. Despite the use of an RDD approach in these studies, the long estimation windows for the dependent variables raise questions over the impact of potential confounding effects on the reported findings.

Finally, our option-implied risk measures also allow us to measure investor perceptions over different kinds of risk (price-, tail- and variance-risk) even if they do not subsequently materialise. This is an important addition to the three studies in the literature that have focused on bankruptcy risk (DiNardo and Lee, 2004; Qiu and Shen, 2017; Campello et al., 2018). For example, as we mention in Section 5.6, even though Campello et al. (2018) find that unionization does not affect a firm's likelihood of entering financial distress, one might argue that the enhanced claims of unionized employees over the assets of the firm during bankruptcy as well as the associated increase in expected bankruptcy costs that they report could lead to changes in investor perceptions over the firm's (price/variance) risk profile after unionization. Our measures allow us to directly test this conjecture.

Our study is organized as follows. The next section describes our data, sample, and risk measures. Section 3 presents our empirical strategy whereas Section 4 reports our main findings and robustness tests. Section 5 presents the results of additional analyses and Section 6 makes some concluding remarks.

2. Data Description and Summary Statistics

To study the effect of unionization on firm risk we match data from the NLRB covering all union elections in the U.S. between 1996 and 2011 with option data from OptionMetrics.[3] In this section, we provide a description of our data collection and matching procedures, the construction of the option-based measures, and the approach we use to compute changes in firm risk. We also present summary statistics for our key variables.

2.1 Labor Union Election Data

The main mechanism through which workers form a union in the U.S. is by a secret ballot election held at the workplace. In this election, workers vote for whether they want a specific union to be certified as their representative for purposes of collective bargaining with their employer. These elections are overseen by the NLRB.[4] We collect union election data for the 1996–2011 period. Data from 1996 to 1998 are obtained from Thomas Holmes's website (Holmes, 2006). The 1999–2011 data are collected from the NLRB website.[5] For every election during our sample period, we gather information on the name of the employer involved, election location, key election dates such as the petition filing, election, tally, and case closure dates, the number of eligible voters and participants, and the voting outcomes. Following Campello et al. (2018), we exclude elections with fewer than 50 voters. This leaves us with 10,291 elections during our sample period. Other than the employer's name, the NLRB dataset does not provide any company identifiers. This makes the matching procedure with option data challenging. We follow a similar algorithm to the one used in Lee and Mas (2012) to match union election and option data by company names.[6] We further review all matches

---

[3] Our sample starts in 1996 because this is the first year that data are available in OptionMetrics.

[4] DiNardo and Lee (2004) and Ferguson (2008) provide a detailed description of the union election process.

[5] Thomas Holmes provides the election data for the 1977–1999 period on his website (http://users.econ.umn.edu/~holmes/data/geo_spill/index.html). The NLRB provides compiled data for all the elections in the 1999–2011 period through the following link: http://catalog.data.gov/dataset/nlrb-cats-final-r-case-data-bulk-19990101-20110930-in-xml.

[6] Specifically, we use Stata's reclink2 command to perform the match. To find the best name match, reclink2 relies on bigram scores. Bigram is an approximate string comparator, which is computed from the ratio of the number of common two consecutive letters of the two strings and their average length minus one. The bigram score used in reclink2 is a modified version where a pair of strings with up to four common prefix letters also gets additional credit (Wasi and Flaaen, 2015).

manually and discard any incorrect ones. Our final sample consists of 586 union elections with available equity option data in OptionMetrics.

2.2 Option-based Measures

Following Kelly et al. (2016), we use information in equity option prices to define three measures of firm risk: stock price risk, tail risk, and variance risk. Price risk refers to the risk of stock price fluctuations, tail risk corresponds to the risk of a large drop in stock price, and variance risk amounts to the risk of a potential shift in stock return volatility. Equity options are typically used by market participants for protection or hedging against these types of risk. Consistent with the investor hedging motive hypothesis (see Kelly et al., 2016), the prices of options whose life spans the unionization election event are expected to be informative regarding the aspects of risk potentially arising due to unionization. Hence, the option-based measures we use should reflect the price paid by market participants to hedge against the corresponding aspect of risk.

Similar to An et al. (2014) and Bali et al. (2017), we compute these measures using information from OptionMetrics' 30-day Volatility Surface file. This file contains implied volatilities for standardized equity options with constant maturities along a grid of the delta space, which is a natural measure of option moneyness. The implied volatility surface provides a standardized way of measuring the expensiveness of equity options with different strikes and maturities.[7]

Similar to Kelly et al. (2016), we measure price risk via the implied volatility of at-the-money equity options. We define *ATM* as the average implied volatility of at-the-money calls and puts:

$$ATM = (ATM_{Call} + ATM_{Put})/2 = (CIV_{50} + CIV_{55} + PIV_{-45} + PIV_{-50})/4,$$

where CIV_{50} (CIV_{55}) is the implied volatility of the 0.5 (0.55) delta call and PIV_{-50} (PIV_{-45}) is the implied volatility of the -0.5 (-0.45) delta put.[8] A higher *ATM* value indicates that at-

[7] Firm-level equity options are typically American-style options. OptionMetrics compute the interpolated implied volatility surface separately for puts and calls using a kernel smoothing technique. The underlying implied volatilities of equity options with various strikes and maturities are computed using an adapted Cox-Ross-Rubinstein binomial tree model that accounts for the early exercise premium of the American-style options and the dividends that firms are expected to pay during the lives of the options.

[8] We opt for the average implied volatility of at-the-money calls and puts instead of the implied volatility of either at-the-money calls or at-the-money puts only, to average out any discrepancies caused by put-call parity deviations

the-money options have become more expensive. This reveals an increase in price risk, which may be caused by market participants purchasing at-the-money options to hedge themselves against this type of risk.

Tail risk is proxied for by the difference between the implied volatility of deep out-of-the-money puts and the implied volatility of at-the-money options:

$$LSKEW = DOTM_{PUT} - ATM = (PIV_{-20} + PIV_{-25})/2 - ATM,$$

where PIV_{-20} (PIV_{-25}) is the implied volatility of the -0.2 (-0.25) delta put. $LSKEW$ captures tail risk because it measures the expensiveness of deep out-of-the-money puts, which are typically used for protection against large stock price drops, relative to the expensiveness of at-the-money options. A higher $LSKEW$ value indicates that deep out-of-the-money puts have become relatively more expensive, reflecting the increased demand by market participants who seek protection or insurance against a feared large price drop. This measure is very similar to the SKEW measure of Xing et al. (2010) and its informational content is virtually the same as the one embedded in the left slope or steepness of the implied volatility curve.[9]

In addition, we proxy for variance risk by the difference between the implied and the realized stock return variances, i.e., the variance risk premium:

$$VRP = ATM^2 - RVOL^2,$$

where $RVOL^2$ is the realized variance of the underlying stock computed over the life of the option. We obtain $RVOL$ from the 30-day Historic Volatility file provided by OptionMetrics.

---

in American-style options. Moreover, it should be noted that at-the-money call (put) options do not exactly correspond to a 0.5 (-0.5) delta. Hence, we use the average implied volatility of 0.5 and 0.55 delta calls (similarly, -0.5 and -0.45 delta puts) because we have found that the exact at-the-money point most often lies between these two delta points.

[9] Kelly et al. (2016) measure tail risk by directly estimating the left slope of the implied volatility curve of equity index options. To this end, they require the implied volatilities of at least 3 out-of-the-money puts with the same maturity. However, for firm-level options, such a number of out-of-the-money puts with the same maturity is not typically available on a daily basis. Hence, our measure of tail risk is based on the interpolated implied volatility surface provided by OptionMetrics. Nevertheless, as mentioned above, the two approaches are equivalent by construction.

VRP reflects the price paid by market participants to hedge against feared shifts in stock return variance relative to the objective expectation of future variance.[10]

2.3 Computing Changes in Firm Risk

Our study seeks to gauge the causal effect of unionization on firm risk. To identify this causal effect, we compare the post-election change in each of the option-based measures defined above across closely won and closely lost union elections.

Specifically, we define the post-election change in the risk level of firm i as:

$$\Delta Risk_i = Risk_{i,post-election} - Risk_{i,pre-election},$$

where $Risk_{i,pre-election}$ denotes firm $i\acute{}s$ risk level just before the election and $Risk_{i,post-election}$ denotes its risk level right after the election.

Our option-implied measures of risk are typically available on a daily basis. Hence, we can focus on very short windows around the unionization event to isolate its causal effect on firm risk. To the contrary, measures of risk that are based on accounting variables would be typically measured at the quarterly or annual frequency. Moreover, measures of risk computed from realized stock returns would typically require much longer estimation windows. In both cases, the potential causality effect due to unionization could be blurred by confounding firm-level events.

In our main analysis, we calculate changes in option-implied risk by comparing the average of daily risk measures in the 10-trading day window before and after the election tally date, i.e., the day the votes are counted and the result is typically announced. We exclude from this estimation window the tally date as well as the two trading days before and after it. We do so in order to avoid picking up possible overreactions just before or after the event date, which are typical of the firm-level price discovery process, and to account for delays in the announcement of the result, which could be prevalent in close elections, e.g., because of vote recounting.[11] This benchmark estimation window on either side of the election event allows us

[10] This interpretation is valid under the conventional assumption that the average realized variance provides an unbiased estimate of the expected variance over the option's life.

[11] Our findings are not sensitive to this choice. We have repeated our analyses after excluding only the tally date from the estimation window and our results remain unchanged.

to use a sufficient number of observations to average out noisy daily estimates without relying on an overly large window, which might capture confounding events and bias our estimates.[12] Nevertheless, in our robustness analysis, we alternatively compute these measures using different windows.

To fix ideas, in our benchmark analysis, we define the post-election change in the option-implied risk level of firm i as:

$$\Delta Risk_i = \overline{Risk_{i,[\tau+3,\tau+10]}} - \overline{Risk_{i,[\tau-10,\tau-3]}},$$

where τ is the election tally date, and the numbering of days corresponds to trading days. To ensure that these averages are computed using a sufficient number of daily observations, we set a minimum filter of 3 non-missing daily observations in each of the $[\tau - 10, \tau - 3]$ and $[\tau + 3, \tau + 10]$ windows.

To cleanly identify the effect of unionization on firm risk, we rely on information from short-maturity options. In particular, we compute the option-implied measures using the 30-day Volatility Surface file provided by OptionMetrics. In this way, we ensure that the horizon of the option-implied information used in the pre-election window spans the unionization election date. Hence, comparing the level of risk extracted from short-maturity options traded right after the unionization election with the level of risk extracted from short-maturity options just before the unionization election enables us to attribute the corresponding shift to the unionization election outcome, because the latter is certainly the dominant firm-level event during the life of these options.

The unionization effect is given by the difference between the post-election change in the risk level of close winners and the post-election change in the risk level of close losers. This is equivalent to a difference-in-differences estimation approach, which also inherently controls for firm fixed effects that may affect the level of risk. More specifically, the causal effect of unionization on firm risk is defined as the following difference-in-differences:

$$Unionization\ Effect = \Delta Risk_{Close\ Winners} - \Delta Risk_{Close\ Losers}.$$

[12] Using average values also allows us to deal with the potential issue of missing observations since OptionMetrics does not compute the implied volatility surface if there are insufficient or unreliable option price data on a given trading day.

2.4 Summary Statistics

Table 1 presents summary statistics for our final sample of 586 election events.[13] Unions win 29% of all elections in our sample and the average vote share for unions is 42.1%. These statistics are in line with previous studies that examine elections at public firms with available stock or bond market data (e.g., Lee and Mas, 2012; Campello et al., 2018). On average, firms in our sample experience a slight drop in *ATM*, *LSKEW*, and *VRP* following a unionization election. Firms in our sample are larger, on average, than firms in Compustat or the NLRB election dataset. This is not surprising considering that we focus on large public firms with tradable options on their stock.

3. Empirical Strategy

Establishing the causal effect of unionization on firm risk is challenging given the significant identification problems in this context. In particular, the decision to establish a labor union might be correlated with omitted variables that also affect firm risk. For example, unobservable factors that lead to bad firm performance could exacerbate firm risk and at the same time encourage employees to unionize if they fear about their job security. This example also points to possible reverse causality problems should firm risk affect the employee decision to unionize.

To mitigate the impact of these endogeneity concerns, we examine secret-ballot labor union elections supported by NLRB and apply an RDD approach (DiNardo and Lee, 2004; Lee and Mas, 2012; Bradley et al., 2017; Campello et al., 2018). There are multiple advantages to this research design. First, the secret-ballot nature of the election leads to considerable ex-ante uncertainty over the outcome of the vote, particularly for close winners and losers. It also makes it more difficult for managers and employees to manipulate the vote share around the cutoff. So, we can expect the vote share probability density to be continuous around the 50% cutoff

[13] Our final sample includes multiple elections held at the same year at different establishments of the same firm consistent with prior work (Lee and Mas, 2012; Campello et al., 2018). All our results are robust to (i) keeping only the first election for each firm in a given year, (ii) removing firms that have more than one election in a given year, (iii) keeping only the first election when a firm has more than one election over a 3-year period, (iv) keeping only the first election when a firm has more than one election during the whole sample period, (v) removing firms that have more than one election during the whole sample period.

point. This is an important condition for implementing RDD as we explain and empirically verify below.

Furthermore, these elections typically lead to “permanent” treatment effects even for close winners and losers. DiNardo and Lee (2004) find that the chance of getting a certified union after an election vote below (above) the 50% threshold is virtually zero (100%) even for close elections. Close losers have a small chance of getting a certified union in the future but typically only through new elections. Close winners rarely have a union decertified with the vast majority maintaining their unions after three years. Thus, we should expect market participants to fully price the treatment effect even for close elections. In addition, by focusing on NLRB elections, which are the typical case anyway, we exclude unions that are voluntarily recognized by employers. The focus on NLRB elections biases in favor of finding a union effect, at least relative to voluntary recognition cases, since NLRB elections are more common when there is divergence of opinion between employees and managers on important matters (DiNardo and Lee, 2004). Thus, if there is a unionization effect on firm risk, we should be able to capture it.

The RDD approach allows us to capture the effect of treatment (i.e., unionization) on the outcome (i.e., option-implied risk) by exploiting local exogenous variation caused by close elections. Effectively, we capture the differential changes in firm risk across close winners and losers, which help infer the impact of unionization. The focus on close winners and losers is important in our setting since it allows us to deal with issues relating to the timing of the resolution of uncertainty caused by the election outcome. In particular, the pre-election option prices reflect market participants’ expectations about the election outcome. Therefore, one would expect the unionization effect to be fully priced for clear winners at some point prior to the election, whereas there should be no effect at all for clear losers. The actual timing of the pricing of the effect for clear winners depends on when investor beliefs about the outcome of the upcoming elections are updated, which is not observable.

Thus, a comparison of the changes in firm risk around elections between clear winners and clear losers would be inappropriate and could lead to incorrect conclusions. However, market participants’ expectations about the highly uncertain election outcome for close winners and losers should be relatively similar, therefore any changes to option prices prior to the event would be differenced out by our RDD model, which is equivalent to a difference-in-differences estimation. Thus, the RDD approach allows us to cleanly capture the change in firm risk caused by the election outcome.

As long as there is no self-selection between close winners and losers, the variation in outcome captured by the RDD approach is equivalent to that of a randomized experiment. Furthermore, the focus on close winners and losers means that there is no need for identification to include other covariates in the analysis, provided that the firms with close elections come from a homogenous group (Lee and Lemieux, 2010). Therefore, the validity of the RDD approach relies on two important conditions, that is, a continuous distribution of the forcing variable around the assignment threshold and insignificant differences in observable firm characteristics for close winners and losers before treatment.

To test for the first condition, we follow the procedure in McCrary (2008) and calculate the fitted density function of the vote share. Figure 1 plots the fitted density with a 95% confidence interval around it; the dots represent the observed density for each vote share bin. There is no evidence of a significant discontinuity in the forcing variable at the cutoff point. We also perform the manipulation test suggested by Cattaneo et al. (2020), which is based on a nonparametric density estimator that automatically adapts to boundary points (in our setting the 50% cutoff point), thus reducing boundary bias. This test further confirms the lack of evidence on manipulation since the robust bias-corrected test statistic is insignificant (T=0.38, p-value = 0.698).

Table 2 presents regression results on the continuity of both our risk measures (Panel A) and other observable firm characteristics (Panel B) in the pre-election period. In particular, Panel B presents results for a series of commonly regarded determinants of firm risk, such as firm size, market-to-book, sales, return-on-assets, leverage, cash holdings, capital expenditures and R&D expenditures. We report the estimates from global polynomial regressions of the following model:

$$Firm\_Characteristic_z = \alpha + \beta \times Unionization_{i,z,t} + \gamma_n \times \sum_{n=1}^{p} (V_i - 0.5)^n + F_s + Y_t + \varepsilon, \qquad (1)$$

where *i* indicates union elections, *p* the order of the polynomial function, *z* firms, *s* industries and *t* years. *Firm_Characteristic* is the level in observable firm characteristics for the pre-election period, *Unionization* is an indicator variable that takes the value of 1 when the vote share is above the 50% cutoff point and zero otherwise, *V* is the election vote share, whereas *F* and *Y* denote industry and year fixed effects, respectively. The tabulated results are based on polynomials of order 3; however, our findings are not sensitive to using other polynomial orders (unreported results).

All the estimates of the *Unionization* coefficient are statistically insignificant, suggesting no discontinuity at the cutoff point for any of these firm characteristics in the pre-election period. Overall, our results collectively suggest that the RDD approach is valid in our setting.

4. Results

4.1 Main Results

This section presents the main RDD results. To ease exposition, we first provide a graphical analysis of the relationship between the election vote share and changes in option-implied firm risk. The three plots included in Figure 2 present the results for the three risk measures we employ in this study, namely *ATM*, *LSKEW* and *VRP*. Each dot represents the conditional mean of the change in risk for each of the 20 equally sized bins of vote share. The solid curves represent the fitted quadratic polynomial estimates of changes in risk as a function of vote share and the dashed curves correspond to their 95% confidence intervals. Figure 2 clearly illustrates the lack of a significant discontinuity around the unionization threshold across all three risk measures. Hence, there is no significant differential change in investor perceptions over firm risk due to the unionization event. As a matter of fact, the average changes in firm risk across all bins (the full range of vote share) appear to be insignificantly different from zero.

Table 3 presents the estimates from global polynomial regressions using the following model:

$$\Delta Risk_{z,i} = \alpha + \beta \times Unionization_{i,z,t} + \gamma_n \times \sum_{n=1}^{p} (V_i - 0.5)^n + F_s + Y_t + \varepsilon, \qquad (2)$$

where the dependent variable *ΔRisk* is the change in firm risk centered around the tally date to capture the impact of the treatment effect. Everything else is as defined in Equation (1). Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW, and ΔVRP,* respectively. Given that the expression in Equation (2) is centered around the 50% vote share cutoff, the coefficient on *Unionization* (i.e., β) captures the direct treatment effect. In other words, β provides an estimate of the causal effect of unionization on the change in option-implied risk.

All coefficients are statistically insignificant suggesting that there is no causal unionization effect on the dimensions of firm risk examined here. For example, the unionization coefficient in Column 1 is -0.009 but insignificant (p-value = 0.386), indicating that a union election does not lead to a significant differential change in price risk between close winners and losers. Similarly, the insignificant coefficients on *Unionization* in Columns 2 and 3 (0.007, p-value = 0.472 and 0.028, p-value = 0.423, respectively) suggest that market participants do not modify their assessment of firms' tail or variance risk significantly differently between close winners and losers due to unionization. We note that, even though the global polynomial regressions use the entire population, the RDD estimates an average treatment effect that places more weight on observations around the threshold. Thus, our findings are relevant to firms experiencing close elections (strong local validity) but cannot be generalized to elections where there are clear winners or losers (weak external validity).

Even though global polynomial regressions may achieve greater precision because of their use of the entire population, they can also lead to estimation biases if the assumed functional forms are not valid for the whole range of data. Therefore, we also run nonparametric local linear regressions for firms close to the assignment threshold. We use the method suggested by Imbens and Kalyanaraman (2012) to identify the optimal bandwidth around the 50% cutoff that minimizes the estimation errors. We also report results based on bandwidths that represent 75% and 125% of the optimal bandwidth. Table 4 presents this analysis. For robustness, we estimate our results using both triangular (Panel A) and rectangular (Panel B) kernels. All reported coefficients on *Unionization* are insignificant, confirming our main finding that union elections do not significantly impact investor perceptions about firm risk. We verify the robustness of this finding by also running local linear regressions using the optimal bandwidth identified by the nonparametric approach suggested by Calonico et al. (2014), which provides robust bias-corrected estimators (unreported result).

Overall, the evidence reported in this section offers strong support to the conjecture that there is no causal effect of unionization on firms' price, tail, or variance risk.

4.2 Robustness Tests: Alternative Firm Risk Measurement

We run a series of tests to check the sensitivity of our findings to our benchmark choices relating to the measurement of changes in firm risk. As we explain in Section 2.3, in our main analysis we use a 10-trading day window around the election tally date to compute the average level of firm risk. We also set a minimum filter of 3 non-missing daily observations in each window and exclude from our calculations the tally date as well as the 2 trading days before and after it. To ensure that our benchmark results are not driven by these methodological choices, we relax these restrictions by: (i) using averages over 15-, 20- and 30-trading day windows, (ii) allowing data from the 2 trading days around the tally date to enter our calculations, and (iii) using averages over 5-trading day windows, including the 2 trading days around the tally date, without imposing the minimum filter of 3 non-missing daily observations. Table SA-1 in the Supplementary Appendix reports the corresponding results from these additional specifications; our findings remain qualitatively unchanged.

Another potential concern is that the computed shift in our firm-level risk measures might be driven by market-level events that could coincide with the election period and lead to market-wide shifts in risk. In the unlikely scenario of a differential impact of these market events on the option-implied firm risk across close winners and losers, our findings in support of a null result might be driven by the impact of these market events cancelling out the unionization effect. To address this potential issue, we calculate market-adjusted changes in option-implied firm risk by subtracting the market-level risk changes ($\Delta Risk_M$) from the firm-level ones ($\Delta Risk_{z,i}$). We are careful to align the market-level to the firm-level changes by only calculating market-level averages for trading days with non-missing firm-level observations. The findings reported in Table SA-2 of the Supplementary Appendix using the market-adjusted changes in firm risk confirm the benchmark null result.

It should be also noted that the magnitude of firm risk varies substantially in the cross-section. For example, small cap firms are typically characterized by higher levels of volatility compared to big cap firms. By using *changes* in risk ($\Delta Risk_{z,i}$), we account for this scale issue (firm fixed effect). However, the use of $\Delta Risk_{z,i}$ implies that we expect a potential shift in the *level* of firm risk. If, instead, the "true" risk shift was *proportional* (i.e., a percentage of its pre-unionization level), then our approach might yield a biased estimate of the unionization effect. To address this potential concern, we calculate the proportional change in firm risk ($\%\Delta Risk_{z,i}$) relative to its pre-election level and repeat our main analysis. Table SA-3 in the Supplementary Appendix reports these additional tests, corroborating our benchmark conclusions.

Prior literature reports unionization-led changes in corporate policies and activities that might result in the election outcome affecting differently investor perceptions about systematic and idiosyncratic firm risk. In Table SA-4 of the Supplementary Appendix, we repeat our main RDD analysis on the effect of unionization on firm risk but now focus on price risk, which we decompose into its systematic and unsystematic components, following the approach in Stilger et al. (2017) and Bali et al. (2019).

Specifically, starting from a single-factor market model, we write the risk-neutral variance for firm *i*, $\sigma^2_{RN,i}$, as:

$$\sigma^2_{RN,i} = \beta^2_{RN,i}\sigma^2_m + \sigma^2_{\varepsilon,i},$$

where $\beta_{RN,i}$ is the risk-neutral beta of firm *i*, σ^2_m is the risk-neutral market variance, and $\sigma^2_{\varepsilon,i}$ is the risk-neutral unsystematic (idiosyncratic) variance of firm *i*. Hence, $\beta^2_{RN,i}\sigma^2_m$ yields the systematic component of risk-neutral variance for firm *i*. Proxying risk-neutral volatility by *ATM*, we can define the systematic component of risk-neutral volatility for firm *i* on day *d* as:

$$ATM_{i,d,systematic} = \beta_i \, ATM_{m,d} \, ,$$

where $ATM_{m,d}$ is the *ATM* of the market on day *d*. Similarly, the risk-neutral unsystematic (idiosyncratic) volatility for firm *i* is defined as the square root of $\sigma^2_{\varepsilon,i}$. For robustness, following Stilger et al. (2017) and Bali et. (2019), we compute $ATM_{i,d,systematic}$ using either the risk-neutral beta (Models 1 and 2) or the physical beta (Models 3 and 4) of the firm. The risk-neutral beta is estimated by regressing $ATM_{i,d}$ on $ATM_{m,d}$ using a rolling window of 250 days prior to the election event. Alternatively, we estimate the physical beta for each firm using the single-factor market model and a rolling window of 250 days prior to the election event. In both cases, we drop the observations where a negative beta coefficient is estimated.

The coefficient for *Unionization* remains clearly insignificant across all 4 cases, which shows that the election outcome does not change investor perceptions over either systematic or idiosyncratic (unsystematic) price risk. Thus, we conclude that our main findings remain unchanged after decomposing price risk.

4.3 Robustness Tests: Alternative Definition of the Event Date

The event date in our main analysis is the election tally date, that is, the day the votes are counted and the result is typically announced. We consider this day to be the most important one since it marks the resolution of uncertainty over the voting outcome. For the majority of cases, the tally date coincides with the election date. However, in few instances, the election and tally dates differ. Hence, we repeat our benchmark tests using the election date as the event date and our results remain virtually the same (unreported result).

Every NLRB-supported union election case eventually closes when NLRB ratifies the election outcome. DiNardo and Lee (2004) mention that objections can be filed to the NLRB within seven days after the tally date. Lee and Mas (2012) find that the median time between the election date and NLRB case closure is 10 days in their post-1977 sample. An inspection of our data confirms that there is indeed a time lag between the tally date and the case closure date. Thus, in order to ensure that our support for a null result is not driven by a potential imprecision in the timing of the resolution of uncertainty regarding the election outcome, we repeat our benchmark tests using the case closure date as the main event date. The new estimates still support the lack of a unionization effect on firm risk (unreported result).

5. Additional Analyses

5.1 Long-term Unionization Effect

The election outcome is typically unambiguous and leads to a "permanent" treatment effect even for close winners and losers (DiNardo and Lee, 2004). Thus, market participants should price the effect rapidly. However, Lee and Mas (2012) find that it takes more than 15 months for the unionization effect to fully emerge in stock prices. They argue that the rarity of union election wins makes arbitrage strategies unprofitable and unattractive, thus leading to mispricing over a considerable number of months.

Though it is surprising that such a degree of mispricing might persist beyond the annual horizon,[14] we test whether there is a discernable unionization effect on firm risk at long horizons. Figure 3 presents 6 plots that illustrate the average $\Delta Risk_{z,i}$ values, computed separately for firms with union victories and losses. For each election, we define the change in option-implied firm risk relative to the pre-election period as the difference between a 10-day moving average of post-election daily risk values and the pre-election risk level. We compute

[14] The finding reported by Lee and Mas (2012) is not particularly strong since they do not identify a discontinuity in 2-year cumulative abnormal returns between close winners and losers (Figure VII). The reported effect appears to be driven by clear winners, which makes it suspect given the weak external validity of the RDD approach.

this 10-day moving average for up to 500 post-election trading days (2 years). This approach yields a continuous daily time-series of the change in firm risk. A visual inspection of the plots reveals no obvious pattern, with all risk changes for firms with union victories remaining insignificant over the 2-year period.

In Table SA-5 of the Supplementary Appendix, we also repeat our main RDD analysis using longer-horizon post-election windows to compute the shifts in risk levels. For example, we compare the level of firm risk computed using 30-day windows at 150 and 500 trading days post-election relative to its pre-election average value. The estimated differential change in risk remains insignificant not only between close winners and losers but also across the whole range of vote share observations.

Overall, we conclude that there is no evidence of a long-term unionization effect on firm risk.

5.2 Total Change in Firm Risk

An advantage of our empirical approach is that, by using option-implied risk measures calculated for a narrow window on either side of the tally date, we minimize the possibility of a confounding firm-level event affecting our estimates. However, using a narrow pre-election period means that the computed risk measures also reflect the uncertainty regarding the unionization election outcome, since the expirations of the options used span the tally date. This is particularly true for close winners and losers. Thus, our main analysis may underestimate the *total* change in firm risk due to unionization, since some of it may have already been incorporated in the risk measures computed during the narrow pre-election period.

We address this issue by comparing the post-election level of firm risk to its level before the petition filing date. As mentioned in Section 2.1, employees wishing to have a union election need to petition the NLRB. Typically, the election takes place within two months of successfully filing a petition. Since we utilize the 30-day Volatility Surface file, we opt for a pre-election window that ends 45 days prior to the filing date. In this way, we ensure that the risk measures computed in this pre-filing period cannot be affected by the potential future shift in the unionization status of the firm because the latter could only occur well after the expiry of the options used. In Table SA-6 of the Supplementary Appendix, we repeat our main analysis for the alternatively defined $\Delta Risk_{z,i}$ to capture the *total* change in firm risk. We still do not find a significant unionization effect on any of our risk measures. Hence, we conclude that the support of the null result in our study is not driven by the potential underestimation of the total change in firm risk.

5.3 Unionization Threat Effect

How good are market participants in assessing the probability of the outcome of a unionization election? Assuming they can accurately predict the election outcome, then the unionization effect on firm risk, if it existed, should be priced even at the pre-election window. Specifically, we would expect the full effect to be priced for clear winners, whereas there should be no effect at all for clear losers. Close winners and losers should experience only moderate effects given the highly uncertain outcome of their unionization election. On the other hand, if there is no significant unionization effect on firm risk, then the threat of a union establishment should result in no material change in investor perceptions about firm risk, even for subsequent clear winners. In other words, an insignificant result in a test examining the impact of unionization threat on firm risk would further confirm the null result we report in this study.

To capture the potential shift in market participants' assessment of firm risk as a result of the threat of unionization, we calculate the change in firm risk between the pre-election 10-trading day window and an equivalent petition pre-filing window. Figure 4 presents 3 plots, one for each risk measure, where the solid curves represent the fitted quadratic polynomial estimates of changes in pre-election risk relative to pre-filing risk as a function of vote share, and the dashed curves show their 95% confidence intervals. The changes remain largely insignificant across all measures and for the whole range of vote share. Hence, we conclude that market participants do not update their perception about firm risk because of the threat of unionization. We interpret this finding as further evidence that there is no unionization effect on firm risk.

5.4 Expected Union Bargaining Power

Our analysis so far captures average treatment effects for close winners and losers. However, one could argue that market participants price a unionization effect on firm risk only when the elected unions are expected to have significant bargaining power, and hence a pronounced impact on firm decision making. Thus, our evidence for a null result might be driven by the fact that we do not condition our analysis on market participants' perceptions about the elected unions' bargaining power. To examine this issue, we follow Bradley et al. (2017) and Campello et al. (2018), partitioning our sample based on whether the union election takes place in a state that has adopted Right-to-Work (RTW) laws or not. RTW laws allow employees to enjoy the benefits of collective bargaining without having to join a union or pay union fees. Therefore, unions have considerably less bargaining power in states with RTW laws compared to states without RTW legislation.

Table 5 reports the results of this subsample analysis. In particular, we repeat the tests presented in Table 3 but we present them separately for elections in RTW states (Panel A) and elections in non-RTW states (Panel B). The coefficient of *Unionization* remains insignificant for all measures of firm risk (Columns 1-3) across both panels (Panels A and B).

Thus, our support for the null result remains intact for elected unions that are expected to exert significant power and influence in their negotiations with management.

5.5 The Effect of Labor Strikes prior to Elections

Labor strikes are highly disruptive events that have significant negative economic impact on the employer (Schmidt and Berri, 2004). They tend to be rare events and their occurrence suggests substantial disagreement between managers and employees. Union elections in firms that have recently experienced strike action[15] could therefore lead to significant changes in investor perceptions about the firms' risk if they are perceived to be either an escalation of the manager-employee conflicts or a mechanism for reducing further disruptions. Arguably, our conclusions in favor of a null result might be driven by the fact that we do not condition our previous analysis on the effect of prior strike action on investors' perceptions about the impact of unionization on firm risk. The results reported in Table 6 address this issue.

In particular, we repeat the analysis of Table 3 separately for elections in firms that had at least one labor strike in the 5 years prior to the union election (Panel A) and for elections in firms with no strike action in the 5 years prior to the union election (Panel B). We still do not find a significant unionization effect on most of our risk measures across both subsamples. Hence, we conclude that the support of the null result in our study is not affected by the incorporation of the role of labor strikes.

[15] The National Labor Relations Act (NLRA) guarantees the statutory right of employees to strike, therefore, it is legal for employees to participate in strikes even without union representation.

5.6 The Effect of Financial Distress and Constraints

Campello et al. (2018) find that the cost of bankruptcy is higher in unionized firms since unionized employees receive special treatment in bankruptcy. This treatment leads to value losses for unsecured creditors when firms become unionized, especially for firms closer to bankruptcy. Even though Campello et al. (2018) show that unionization does not affect a firm's likelihood of entering financial distress, one might argue that the enhanced claims of unionized employees over the assets of the firm during bankruptcy as well as the associated increase in expected bankruptcy costs could lead to changes in investor perceptions over the firm's risk profile after unionization. In other words, our support for the null hypothesis so far might be driven by the fact that we have not conditioned our analysis on firms' financial status. We address this issue here. In particular, we classify the firms in our sample into financially distressed and financially healthy ones and repeat the analysis presented in Table 3, but now separately for each subsample.

Table 7 presents the results of this analysis using the modified Altman's z-score as our measure of financial distress. The coefficients of *Unionization* remain insignificant for both distressed (Panel A) and healthy (Panel B) firms and for all risk measures (Columns 1-3). We also repeat this analysis using the Ohlson O-score and Moody's credit ratings as alternative proxies for financial distress and our conclusions remain unchanged (see Table SA-7 in the Supplementary Appendix).

Recent work by Schmalz (2015) finds that even though the causal effect of unionization on firms' financial policies is zero on average, there is significant variation of this effect between financially constrained and unconstrained firms. In particular, he reports that financially unconstrained firms respond to unionization by increasing cash holdings and reducing leverage, thus increasing financial flexibility, whereas the opposite holds true for financially constrained firms. This behavior could lead to a systematic effect of unionization on firm risk across these different types of firms. Table 8 reports results from a subsample analysis of financially constrained and unconstrained firms. We classify firms as constrained or unconstrained using the Kaplan and Zingales (1997) (KZ) index. The coefficients of *Unionization* remain insignificant for both constrained (Panel A) and unconstrained (Panel B) firms across all risk measures (Columns 1-3). In Table SA-8 of the Supplementary Appendix, we repeat this analysis using alternative proxies for financial constraints, such as firm size and payout ratio, and the results remain qualitatively similar.

Collectively, the results in this section indicate that firm heterogeneity, in terms of financial distress and constraints, does not alter our main conclusion that there is no causal effect of unionization on firm risk.

6. Conclusions

This paper finds no evidence of a causal effect of labor unionization on firm risk. Using a sample of labor union elections, we employ a regression discontinuity design to study changes in option-implied measures of firm risk caused by election outcomes. The use of information from the options market helps us overcome several limitations in prior studies that relied on accounting, stock or bond market information and strengthens our argument that our setup is better suited for the investigation of a causal effect.

We examine the robustness of our conclusion by investigating the impact of unionization on different types of firm risk, that is, price, tail, and variance risk, as well as different definitions of these risk measures, i.e., levels, market-adjusted levels, proportional changes as well as systematic and unsystematic components. Given prior work arguing that the effect of unionization on firm value manifests itself over a 15-18 month period (Lee and Mas, 2012), we also estimate this effect for different windows, ranging from a 5-trading day window around the tally date to a 2-year one. We also allow for different assumptions about the timing of the resolution of uncertainty regarding the election outcome. Finally, we follow prior literature and examine the role of firm heterogeneity, in terms of union bargaining power, prior strike action and firm financial distress and constraints (Schmalz, 2015; Campello et al., 2018), with respect to the unionization effect on firm risk; our conclusion remains unchanged.

Overall, we contribute to the literature by providing new and novel evidence that helps address prior contradictory conclusions regarding the relationship between unionization and firm risk (Chen et al., 2011; Chen et al., 2012). Even though there are mixed findings on the impact of labor unionization on firm policies in general, we provide robust evidence that unionization has no direct causal effect on firm risk.

References

Abowd, J.M. (1989), The effect of wage bargains on the stock market value of the firm. American Economic Review 79, 774-800.

Abraham, K., and J. Medoff (1984), Length of service and layoffs in union and non-union work groups. Industrial and Labor Relations Review 38, 87-97.

Agrawal, A.K. (2012), Corporate governance objectives of labor union shareholders: Evidence from proxy voting. Review of Financial Studies 25, 187–226.

An, B.J., Ang, A., Bali, T.G., and N. Cakici (2014), The joint cross section of stocks and options. Journal of Finance 69, 2279-2337.

Bali, T.G., Hu, J., and S. Murray (2017), Option implied volatility, skewness and kurtosis and the cross-section of expected stock returns. Working Paper.

Berk, J.B., Stanton, R., and J. Zechner (2010), Human capital, bankruptcy, and capital structure. Journal of Finance 65, 891-926.

Bradley, D., Kim, I., and X. Tian (2017), Do unions affect innovation? Management Science 63, 2251-2271.

Bronars, S.G., and D.R. Deere (1991), The threat of unionization, the use of debt, and the preservation of shareholder wealth. Quarterly Journal of Economics 106, 231-254.

Calonico, S., Cattaneo, M.D., and R. Titiunik (2014), Robust nonparametric confidence intervals for regression-discontinuity designs. Econometrica 82, 2295-2326.

Campello, M., J. Qiu, J. Gao, and Y. Zhang (2018), Bankruptcy and the cost of organized labor: Evidence from union elections. Review of Financial Studies 31, 980-1013.

Cattaneo, M.D., M. Jansson, and X. Ma (2020), Simple local polynomial density estimators. Journal of the American Statistical Association 115, 1449-1455.

Chen, H.J., Kacperczyk, M., and H. Ortiz-Molina (2011), Labor unions, operating flexibility, and the cost of equity. Journal of Financial and Quantitative Analysis 46, 25-58.

Chen, H.J., Kacperczyk, M., and H. Ortiz-Molina (2012), Do nonfinancial stakeholders affect the pricing of risky debt? Evidence from unionized workers. Review of Finance 16, 347-383.

Connolly, R.A., B.T. Hirsch, and M. Hirschey (1986), Union rent seeking, intangible capital, and market value of firm. Review of Economics and Statistics 68, 567-577.

Cooper, I. (2006), Asset pricing implications of nonconvex adjustment costs and irreversibility of investment. Journal of Finance 61, 139-170.

Dasgupta, S., and K. Sengupta (1993), Sunk investment, bargaining and choice of capital structure. International Economic Review 34, 203-220.

Dessaint, O., Golubov, A., and P. Volpin (2017), Employment protection and takeovers. Journal of Financial Economics 125, 369-388.

DiNardo, J., and D.S. Lee (2004), Economic impacts of new unionization on private sector employers: 1984-2001. Quarterly Journal of Economics 119, 1383-1441.

Easley, D., O'Hara, M., and P.S. Srinivas (1998), Option volume and stock prices: Evidence on where informed traders trade. Journal of Finance 53, 431-465.

Ferguson, J.P. (2008), The eyes of the needles: A sequential model of union organizing drives, 1999-2004. Industrial and Labor Relations Review 62, 3-21.

Frandsen, B.H. (2020), The surprising impacts of unionization: Evidence from matched employer-employee data. Journal of Labor Economics, forthcoming.

Freeman, R.B. (1981), The effect of unionism on fringe benefits. Industrial and Labor Relations Review 34, 489-504.

Freeman, R.B., and J.L. Medoff (1984), What do unions do? Basic Books, NY.

Gamba, A., and A. Triantis (2008), The value of financial flexibility. Journal of Finance 63, 2263-2296.

Ge, L., T.C. Lin, and N.D. Pearson (2016), Why does the option to stock volume ratio predict stock returns? Journal of Financial Economics 120, 601-622.

He, J., X. Tian, H. Yang, and L. Zuo (2020), Asymmetric cost behavior and dividend policy. Journal of Accounting Research 58, 989-1021.

Hirsch, B.T. (1991), Union coverage and profitability among US firms. Review of Economics and Statistics 73, 69-77.

Hirsch, B.T. (2004), Reconsidering union wage effects: Surveying new evidence on an old topic. Journal of Labor Research 25, 233-266.

Holmes, T.J. (2006), Geographic spillover of unionism. Staff Report 368, Federal Reserve Bank of Minneapolis.

Huang, Q., Jiang, F., Lie, E., and T. Que (2017), The effect of labor unions on CEO compensation. Journal of Financial and Quantitative Analysis 52, 553-582.

Imbens, G.W., and K. Kalyanaraman (2012), Optimal bandwidth choice for the regression discontinuity estimator. Review of Economic Studies 79, 933-959.

Kaplan, S., and L. Zingales (1997), Do investment-cash flow sensitivities provide useful measures of financing constraints? Quarterly Journal of Economics 112, 169-215.

Kelly, B., Pastor, L., and P. Veronesi (2016), The price of political uncertainty: Theory and evidence from the option market. Journal of Finance 71, 2417-2480.

Lee, D.S., and T. Lemieux (2010), Regression discontinuity designs in economics. Journal of Economic Literature 48, 281-355.

Lee, D.S., and A. Mas (2012), Long-run impacts of unions on firms: New evidence from financial markets, 1961-1999. Quarterly Journal of Economics 127, 333-378.

Lie, E., and T. Que (2019), Union concessions following asset sales and takeovers. Journal of Financial and Quantitative Analysis 54, 393-424.

Matsa, D.A. (2010), Capital structure as a strategic variable: Evidence from collective bargaining. Journal of Finance 65, 1197-1232.

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

Myers, B., and A. Saretto (2016), Does capital structure affect the behavior of nonfinancial stakeholders? An empirical investigation into leverage and union strikes. Management Science 62, 3235-3253.

Novy-Marx, R. (2011), Operating leverage. Review of Finance 15, 103-134.

Pan, J., and A.M. Poteshman (2006), The information in option volume for future stock prices. Review of Financial Studies 19, 871-908.

Perotti, E., and K. Spier (1993), Capital structure as a bargaining tool: The role of leverage in contract renegotiation. American Economic Review 83, 1131-1141.

Qiu, Y., and T. Shen (2017), Organized labor and loan pricing: A regression discontinuity design analysis. Journal of Corporate Finance 43, 407-428.

Rosett, J.G. (2001), Equity risk and the labor stock: The case of union contracts. Journal of Accounting Research 39, 337-364.

Ruback, R.S., and M.B. Zimmerman (1984), Unionization and profitability: Evidence from the capital market. Journal of Political Economy 92, 1134-1157.

Schmalz, M.C. (2015), Unionization, cash, and leverage. Working Paper.

Schmidt, M.B., and D.J. Berri (2004), The impact of labor strikes on consumer demand: An application to professional sports. American Economic Review 94, 344-357.

Simintzi, E., Vig, V., and P. Volpin (2015), Labor protection and leverage. Review of Financial Studies 28, 561-591.

Stanfield, J., and R. Tumarkin (2018), Does the political power of nonfinancial stakeholders affect firm values? Evidence from labor unions. Journal of Financial and Quantitative Analysis 53, 1101-1133.

Stilger, P.S., Kostakis, A., and S.-H. Poon (2017), What does risk-neutral skewness tell us about future stock returns? Management Science 63, 1814-1834.

Tian, X., and W. Wang (2020), Hard marriage with heavy burdens: Organized labor as takeover deterrents. The Review of Corporate Finance Studies, forthcoming.

Wasi, N., and A. Flaaen (2015), Record linkage using Stata: Preprocessing, linking, and reviewing utilities. Stata Journal 15, 672-697.

Woods, K., K.J.K. Tan, and R. Faff (2019), Labor unions and corporate financial leverage: The bargaining device versus crowding-out hypotheses. Journal of Financial Intermediation 37, 28-44.

Xing, Y., X. Zhang, and R. Zhao (2010), What does the individual option volatility smirk tell us about future equity returns? Journal of Financial and Quantitative Analysis 45, 641-662.

Zhang, L. (2005), The value premium. Journal of Finance 60, 67-103.

Figure 1. Unionization elections: Density distribution of the union vote share

This figure plots the density distribution of the union vote shares following the procedure developed by McCrary (2008). The x-axis represents the share of votes cast in favor of unionization. The dots represent the observed density for each vote share bin. The thick solid curve represents the fitted density function of the vote share with a 95% confidence interval around it. Union elections data are from the NLRB over the period 1996-2011.

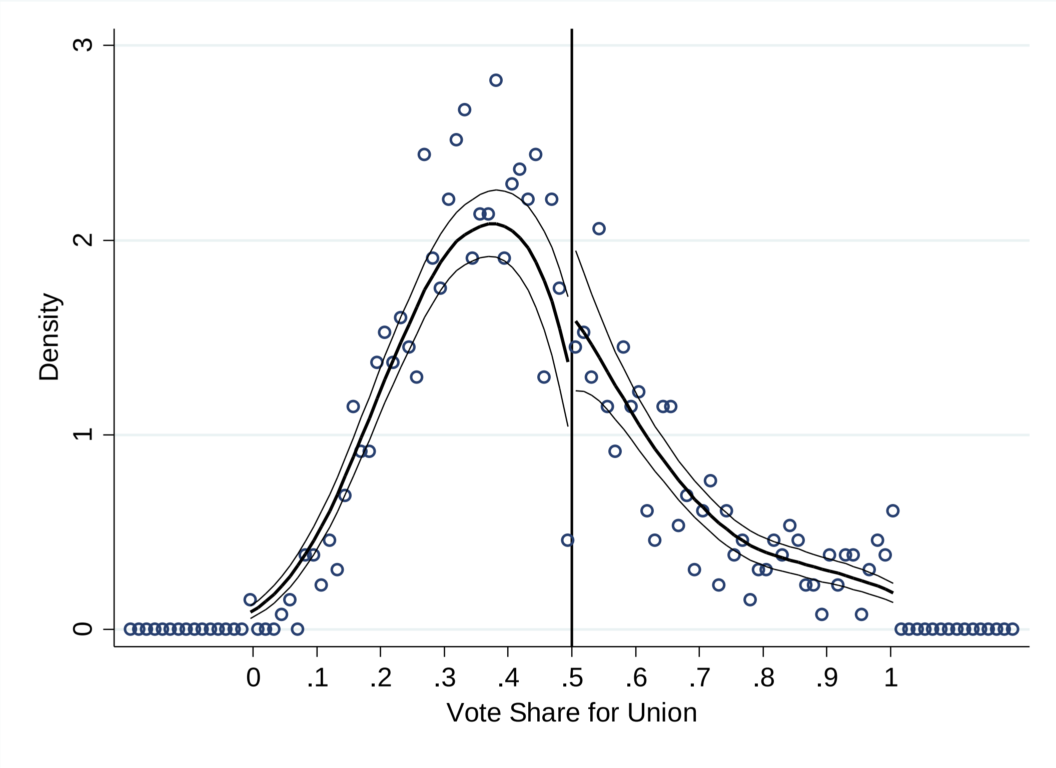

Figure 2. Changes in option-implied firm risk following a unionization election

This figure shows the changes in the average option-implied firm risk over 10-trading day windows around a unionization election. The three regression discontinuity plots present the results for the three risk measures we use in this study, namely *ATM*, *LSKEW* and *VRP*. Each dot represents the conditional mean of the change in risk for each of the 20 equally sized bins of vote share. Observations to the right of the 50% vertical line correspond to union wins. The solid curves represent the fitted quadratic polynomial estimates of changes in risk as a function of vote share and the dashed curves show the 95% confidence intervals. The discontinuity of the outcome variable at the 50% vote share threshold represents the estimated causal effect of unionization.

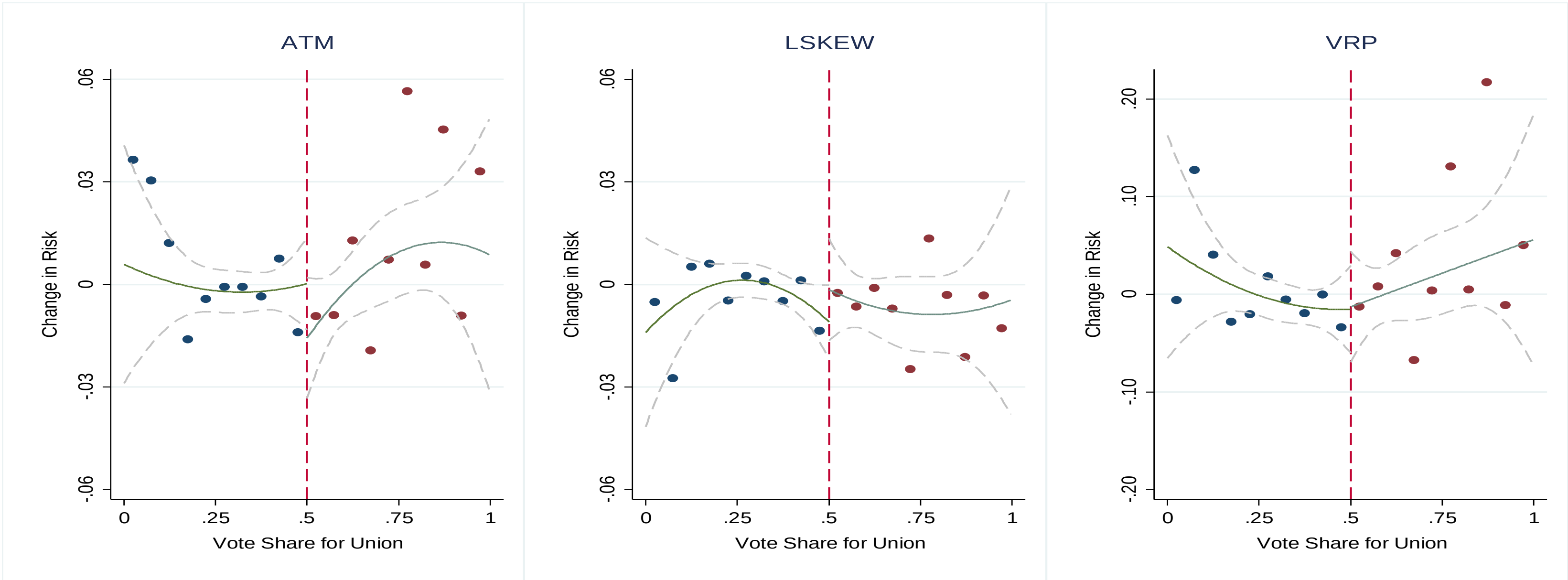

Figure 3. Long-term unionization effect

This figure shows the long-term effect of unionization on firm risk. We present six plots that capture the average *ΔRisk* estimated for all the option-implied risk measures (*ΔATM, ΔLSKEW,* and *ΔVRP)*, separately for firms with union victories and losses. For each election, we define the change in option-implied risk relative to the pre-election period as the difference between a 10-trading day moving average of post-election daily risk and the pre-election risk. We calculate the moving average for 500 trading days (2 years) post-election. The horizontal axis represents the number of days relative to the tally date.

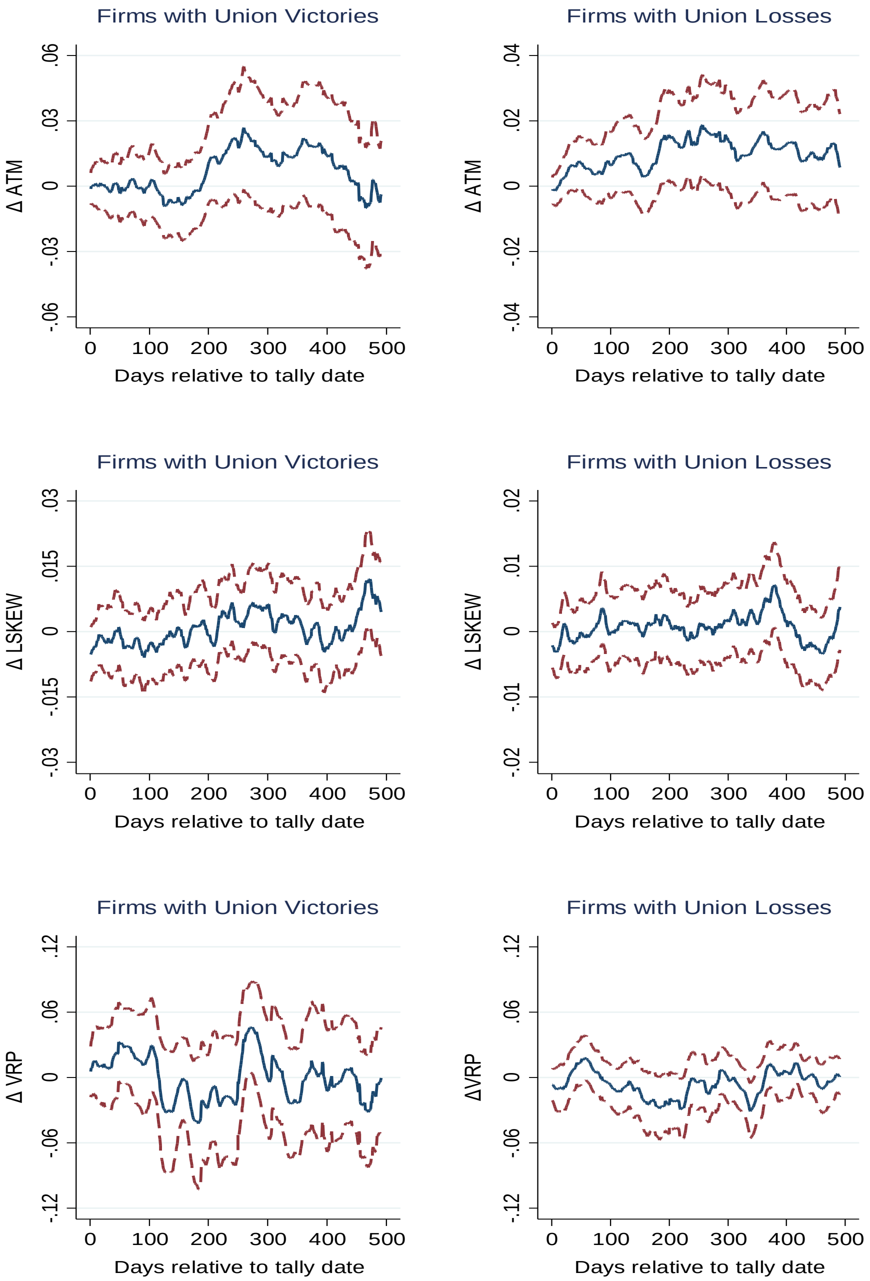

Figure 4. Unionization threat effect

This figure shows the changes in option-implied firm risk as a result of the threat of unionization. To capture the change in investors' perceptions of firm risk resulting from the increased threat of unionization post filing, we calculate the change in firm risk between the pre-election 10-trading day window and an equivalent petition pre-filing window. The three regression discontinuity plots present the results for the three risk measures we use in this study, namely *ATM*, *LSKEW*, and *VRP*. Each dot represents the conditional mean of the change in risk for each of the 20 equally sized bins of vote share. Observations to the right of the 50% vertical line correspond to union wins. The solid curves represent the fitted quadratic polynomial estimates of changes in risk as a function of vote share and the dashed curves show the 95% confidence intervals. The discontinuity of the outcome variable at the 50% vote share threshold represents the estimated causal effect of unionization.

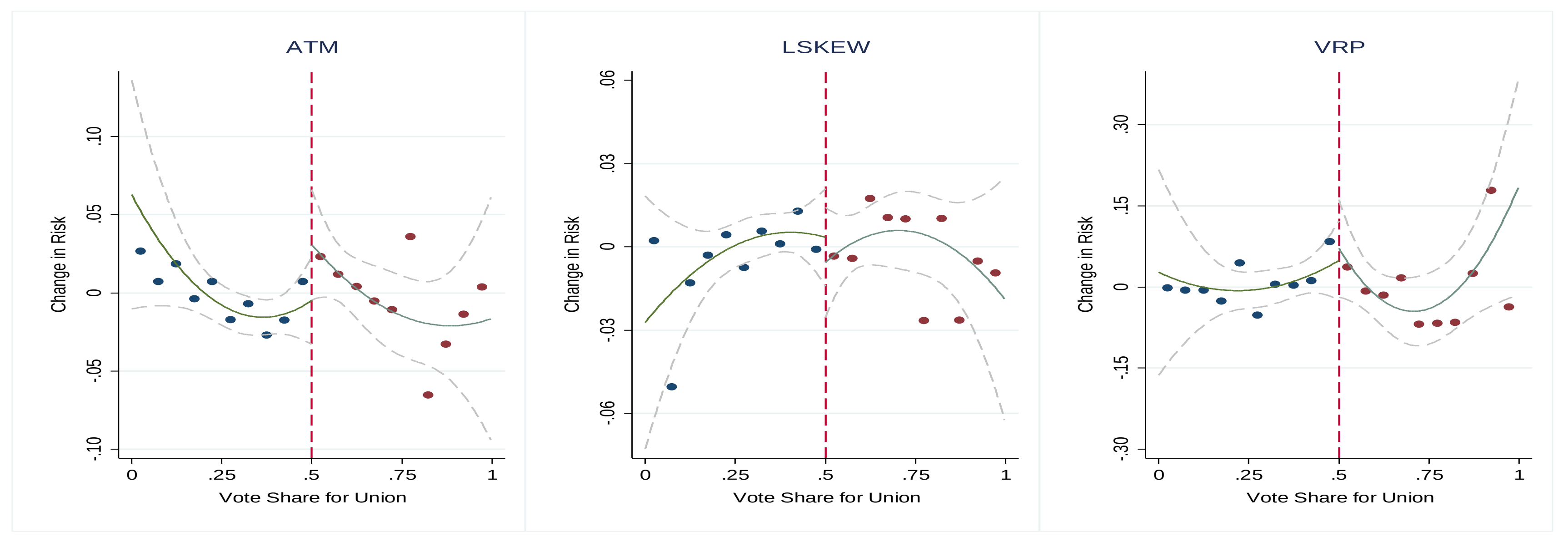

Table 1. Summary statistics

This table reports summary statistics for firm characteristics, labor union election variables, and option-implied risk measures used in our analysis. Our sample consists of 586 union elections covering the period 1996–2011. *Vote share for unions* is the number of votes in favor of unionization divided by the total number of votes in a given election. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. *ΔATM, ΔLSKEW,* and *ΔVRP* are measured as the difference between the average value of the option-implied risk measure in the 10 trading days before and after an election. We exclude the 5 trading days around and including the tally date from these calculations. *Size* is measured as the logarithm of the book value of total assets (at). The *market-to-book* ratio is defined as the book value of assets (at) plus the market value of common equity (prcc_f * csho) minus the book value of common equity (ceq), scaled by the book value of total assets (at). *Sales* is sales revenue (*sale*) scaled by the book value of assets (at). *ROA* is Net income (*ni*) scaled by the book value of total assets (at). *Leverage* is long-term debt (dltt) plus debt in current liabilities (dlc), scaled by total assets (at). *Cash holdings* is the ratio of cash and short-term investments (che) to total assets (at). *Capital expenditures* is the ratio of capital expenditures (capx) to total assets (at). *R&D expenditures* is the ratio of R&D expenses (xrd) to net sales (sale), and is set equal to zero when R&D expenses (xrd) are missing.

.

Variable	Mean	Median	Std. Dev.	Q1	Q4
Election variables:					
Number of valid votes	224.258	110.500	681.968	71.000	202.000
Vote share for unions	0.421	0.400	0.184	0.288	0.533
Unionization	0.290	0.000	0.454	0.000	1.000
Risk measures:					
ΔATM	-0.001	-0.003	0.048	-0.020	0.014
ΔLSKEW	-0.003	-0.002	0.039	-0.017	0.015
ΔVRP	-0.002	-0.003	0.156	-0.027	0.023
Firm characteristics:					
Size	8.735	8.917	1.437	7.631	9.878
Market-to-book	2.594	2.064	2.656	1.425	3.287
Sales	1.249	0.995	0.881	0.673	1.521
ROA	0.137	0.132	0.058	0.100	0.170
Leverage	0.328	0.344	0.160	0.199	0.432
Cash holdings	0.053	0.029	0.065	0.012	0.071
Capital expenditures	0.055	0.051	0.037	0.029	0.070
R&D expenditures	0.006	0.000	0.012	0.000	0.007

Table 2. Continuity of observable firm characteristics

This table presents the test results for the null hypothesis that there are no systematic pre-election differences in observable characteristics between firms in which unions barely win elections and firms in which unions barely lose elections. Panel A shows the results for the pre-election risk variables measured over one year prior to the election filing date. Panel B shows the results for other firm characteristics measured in the year before the election. We report the estimates from a global polynomial model. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level. All variables are defined in Table 1.

Panel A. Pre-election risk measures

Variables	*ATM* (1)	*LSKEW* (2)	*VRP* (3)
Unionization	0.025	0.001	-0.003
	(0.317)	(0.877)	(0.863)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	586	586	586

Panel B. Other pre-election characteristics

Variables	*Size* (1)	*Market-to-book* (2)	*Sales* (3)	*ROA* (4)	*Leverage* (5)	*Cash holdings* (6)	*Capital expenditures* (7)	*R&D expenditures* (8)
Unionization	-0.099	-0.542	-0.080	-0.010	-0.003	-0.000	-0.006	0.002
	(0.567)	(0.258)	(0.286)	(0.266)	(0.902)	(0.970)	(0.294)	(0.305)
Polynomial order	3	3	3	3	3	3	3	3
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	583	583	583	583	583	583	576	583

Table 3. Unionization and firm risk: Polynomial regression results

This table presents the RDD results for the effect of unionization on firm risk. We report the estimates from the global polynomial model specified in Equation (2). The dependent variable is the change in firm risk (*ΔRisk*) measured as the difference between the average value of the option-implied risk measure in the 10 trading days before and after the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

.

	Dependent variable: *ΔRisk*		
	ΔATM	*ΔLSKEW*	*ΔVRP*
Variables	(1)	(2)	(3)
Unionization	-0.009	0.007	0.028
	(0.386)	(0.472)	(0.423)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	586	586	586

Table 4. Unionization and firm risk: Local linear regression results

This table presents the RDD results for the effect of unionization on firm risk. We report the estimates from the local linear regressions for firms close to the assignment threshold using the optimal bandwidth suggested by Imbens and Kalyanaraman (2012). We also report results based on the 75% and 125% of the optimal bandwidth. We estimate our results using both triangular (Panel A) and rectangular (Panel B) kernels. The change in firm risk (*ΔRisk*) is measured as the difference between the average value of the option-implied risk measure in the 10 trading days before and after the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. The p-values in parentheses are based on robust standard errors clustered at the firm level.

Panel A. Triangular Kernel

	Dependent variable: *ΔRisk*		
	ΔATM	*ΔLSKEW*	*ΔVRP*
Variables	(1)	(2)	(3)
Optimal bandwidth			
Unionization	0.001	0.010	0.045
	(0.931)	(0.230)	(0.220)
Observations	335	458	322
75% of optimal bandwidth			
Unionization	0.006	0.011	0.067
	(0.671)	(0.224)	(0.118)
Observations	256	365	247
125% of optimal bandwidth			
Unionization	-0.001	0.007	0.041
	(0.936)	(0.319)	(0.204)
Observations	411	521	386

Panel B. Rectangular Kernel

	Dependent variable: *ΔRisk*		
	ΔATM	*ΔLSKEW*	*ΔVRP*
Variables	(1)	(2)	(3)
Optimal bandwidth			
Unionization	-0.003	0.007	0.034
	(0.755)	(0.431)	(0.373)
Observations	263	378	254
75% of optimal bandwidth			
Unionization	0.010	0.009	0.055
	(0.517)	(0.348)	(0.197)
Observations	206	281	197
125% of optimal bandwidth			
Unionization	0.000	0.009	0.042
	(0.969)	(0.259)	(0.221)
Observations	332	449	311

Table 5. Union bargaining power: The effect of Right-to-Work laws

This table presents the RDD results from global polynomial regressions for subsamples of firms located in states with or without Right-to-Work (RTW) laws, respectively. The change in firm risk (*ΔRisk*) is measured as the difference between the average value of the option-implied risk measure in the 10 trading days before and after the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. Panel A reports the results for firms in states with RTW laws and Panel B shows the results for firms in states without RTW laws. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

Panel A. States with RTW laws

.

	Dependent variable: *ΔRisk*		
	ΔATM	*ΔLSKEW*	*ΔVRP*
Variables	(1)	(2)	(3)
Unionization	0.013	0.006	0.118
	(0.605)	(0.773)	(0.266)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	183	183	183

Panel B. States without RTW laws

.

	Dependent variable: *ΔRisk*		
	ΔATM	*ΔLSKEW*	*ΔVRP*
Variables	(1)	(2)	(3)
Unionization	-0.014	0.013	0.007
	(0.299)	(0.303)	(0.859)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	394	394	394

Table 6. Firm heterogeneity: The effect of pre-election labor strikes

This table presents the RDD results from global polynomial regressions for subsamples of elections in firms with and without prior labor strikes. An election is defined as an "election in a firm with prior labor strikes" if there is at least one strike action in the firm in the 5 years preceding the election. We collect strike data from the U.S. Bureau of Labor Statistics (BLS) and the Federal Mediation and Conciliation Service (FMCS). The change in firm risk (*ΔRisk*) is measured as the difference between the average value of the option-implied risk measure in the 10 trading days before and after the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. Panel A reports the results for elections in firms with prior labor strikes and Panel B shows the results for elections in firms with no prior labor strikes. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

Panel A. Elections in firms with prior labor strikes

.

	Dependent variable: *ΔRisk*		
	ΔATM	*ΔLSKEW*	*ΔVRP*
Variables	(1)	(2)	(3)
Unionization	-0.030	0.016	0.028
	(0.092)	(0.270)	(0.619)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	200	200	200

Panel B. Elections in firms with no prior labor strikes

.

	Dependent variable: *ΔRisk*		
	ΔATM	*ΔLSKEW*	*ΔVRP*
Variables	(1)	(2)	(3)
Unionization	-0.000	0.002	0.025
	(0.996)	(0.869)	(0.636)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	386	386	386

Table 7. Firm heterogeneity: The effect of financial distress

This table presents the RDD results from global polynomial regressions for subsamples of firms with different levels of financial distress. We measure financial distress using the modified Altman's z-score (1.2*(wcap/at) + 1.4*(re/at) + 3.3*(ebit/at) + (sale/at) + 0.6*((prcc_f * csho)/(dltt + dlc))). For each year, we define firms with below (above)-median z-score as distressed (healthy). The change in firm risk (*ΔRisk*) is measured as the difference between the average value of the option-implied risk measure in the 10 trading days before and after the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. Panel A reports the results for distressed firms and Panel B shows the results for healthy firms. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

Panel A. Distressed firms

.

	Dependent variable: *ΔRisk*		
	ΔATM	*ΔLSKEW*	*ΔVRP*
Variables	(1)	(2)	(3)
Unionization	-0.017	0.008	0.067
	(0.440)	(0.598)	(0.286)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	258	258	258

Panel B. Healthy firms

.

	Dependent variable: *ΔRisk*		
	ΔATM	*ΔLSKEW*	*ΔVRP*
Variables	(1)	(2)	(3)
Unionization	0.001	-0.002	-0.032
	(0.954)	(0.887)	(0.540)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	263	263	263

Table 8. Firm heterogeneity: The effect of financial constraints

This table presents the RDD results from global polynomial regressions for subsamples of firms with different levels of financial constraints. We measure the degree of financial constraints using the Kaplan and Zingales (1997) (KZ) index. The KZ index is defined as −1.002 × Cash flow + 0.283 × Tobin's q + 3.139 × Debt – 39.368 × Dividends – 1.315 × Cash holdings. For each year, we define firms with above (below)-median score on the KZ index as constrained (unconstrained). The change in firm risk (*ΔRisk*) is measured as the difference between the average value of the option-implied risk measure in the 10 trading days before and after the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. Panel A reports the results for firms that are financially constrained, and Panel B shows the results for financially unconstrained firms. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

Panel A. Constrained firms

.

	Dependent variable: *ΔRisk*		
Variables	*ΔATM* (1)	*ΔLSKEW* (2)	*ΔVRP* (3)
Unionization	-0.013 (0.564)	0.011 (0.513)	0.062 (0.377)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	276	276	276

Panel B. Unconstrained firms

.

	Dependent variable: *ΔRisk*		
Variables	*ΔATM* (1)	*ΔLSKEW* (2)	*ΔVRP* (3)
Unionization	-0.007 (0.552)	0.011 (0.259)	-0.012 (0.728)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	275	275	275

Supplementary Appendix

Table SA-1. Unionization and firm risk: Alternative time windows

This table presents the RDD results for the effect of unionization on firm risk using alternative time windows. We report the estimates from the global polynomial model specified in Equation (2). In Models 1–3, the dependent variable is the change in firm risk (*ΔRisk*) measured as the difference between the average value of the option-implied risk measure in the 15, 20, or 30 trading days before and after the tally date, respectively. In Models 1–3, we exclude the 5 trading days around and including the tally date from these calculations. In Models 4 and 5, we measure the change in firm risk as the difference between the average value of the option-implied risk measure in either the 10 or 5 trading days before and after the tally date including the 2 trading days around that date. In Models 1–4, we set a minimum filter of 3 non-missing daily observations in each of the pre- and post-election windows, whereas in Model 5 we remove this filter. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. We report the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

Variables	15 days (1)	20 days (2)	30 days (3)	(-10,10) (4)	(-5,5) (5)
Dependent variable: ***ΔATM***					
Unionization	-0.005	-0.006	-0.013	-0.011	-0.010
	(0.652)	(0.584)	(0.255)	(0.232)	(0.218)
Dependent variable: ***ΔLSKEW***					
Unionization	0.006	0.006	0.003	0.004	-0.003
	(0.425)	(0.429)	(0.736)	(0.605)	(0.659)
Dependent variable: ***ΔVRP***					
Unionization	0.043	0.047	0.045	0.020	0.000
	(0.333)	(0.330)	(0.374)	(0.505)	(0.983)
Polynomial order	3	3	3	3	3
Industry fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	586	586	586	586	586

Table SA-2. Unionization and firm risk: Market-adjusted changes

This table presents the RDD results for the effect of unionization on market-adjusted firm risk. We report the estimates from the global polynomial model specified in Equation (2). The dependent variable is the change in option-implied firm risk in excess of the corresponding change in market risk (*ΔEXRisk*). The change in firm and market risk is measured as the difference between the average value of the corresponding option-implied risk measure in the 10 trading days before and after the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔEXATM, ΔEXLSKEW*, and *ΔEXVRP*, respectively. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

.

	Dependent variable: *ΔEXRisk*		
Variables	*ΔEXATM* (1)	*ΔEXLSKEW* (2)	*ΔEXVRP* (3)
Unionization	-0.010 (0.362)	0.006 (0.520)	0.027 (0.424)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	586	586	586

Table SA-3. Unionization and firm risk: Proportional changes

This table presents the RDD results for the effect of unionization on proportional changes in firm risk. We report the estimates from the global polynomial model specified in Equation (2). The dependent variable is the proportional post-election change in firm risk relative to its pre-election level (*%ΔRisk*). The post- (pre-) election level of option-implied risk is given by the average value of the corresponding measure in the 10 trading days after (before) the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *%ΔATM, %ΔLSKEW,* and *%ΔVRP,* respectively. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

.

	Dependent variable: *%ΔRisk*		
Variables	*%ΔATM* (1)	*%ΔLSKEW* (2)	*%ΔVRP* (3)
Unionization	-0.017 (0.464)	5.410 (0.231)	0.676 (0.795)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	586	586	586

Table SA-4. Unionization and firm risk: Systematic vs. unsystematic risk

This table presents the RDD results for the effect of unionization on the systematic and unsystematic components of firm risk. We decompose *ATM* into its systematic and unsystematic components using the approach in Bali et al. (2019). Specifically, we define the systematic component of *ATM* for firm *i* on day *d* as: $ATM_{i,d,systematic} = \beta_i \ ATM_{m,d}$, where β_i is either the risk-neutral beta of firm *i* (Models 1 and 2) or the physical beta of firm *i* (Models 3 and 4) and $ATM_{m,d}$ is the *ATM* of the market on day *d*. Following Stilger at al. (2017) and Bali et al. (2019), we estimate the risk-neutral beta for each firm *i*, by regressing $ATM_{i,d}$ on $ATM_{m,d}$ using a rolling window of 250 days prior to the election event. We estimate the physical beta for each firm *i* by regressing the stock returns for firm *i* on the market returns using a rolling window of 250 days prior to the election event. We drop the cases where the estimation approach yields a negative coefficient either for the risk-neutral or physical beta. We report the estimates from the global polynomial model specified in Equation (2) in the manuscript. The dependent variable is the change in firm risk (*ΔRisk*) measured as the difference between the average value of the option-implied risk measure in the 10 trading days before and after the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 and 2 present the coefficient estimates for *Unionization* when the dependent variable are *ΔATM systematic* and *ΔATM unsystematic*, respectively. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

	Dependent variable: *ΔRisk*			
Variables	*ΔATM systematic* (1)	*ΔATM unsystematic* (2)	*ΔATM systematic* (3)	*ΔATM unsystematic* (4)
Unionization	0.005	-0.014	-0.005	-0.003
	(0.723)	(0.439)	(0.296)	(0.778)
Polynomial order	3	3	3	3
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	454	454	573	573

Table SA-5. Unionization and firm risk: Long-term effect

This table presents the RDD results for the long-term effect of unionization on firm risk. We report the estimates from the global polynomial model specified in Equation (2). The dependent variable is the change in firm risk (*ΔRisk*) measured as the difference between the average value of the option-implied risk measure before and after the tally date. We compare the average level of firm risk computed using 30-day windows at 150 and 500 trading days post-election relative to the pre-election 30-day average. We exclude the 2 trading days before the tally date from the calculation of the pre-election average. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

.

	Dependent variable: *ΔRisk*		
Variables	*ΔATM* (1)	*ΔLSKEW* (2)	*ΔVRP* (3)
Post window: (150, 179)			
Unionization	-0.032 (0.232)	0.004 (0.730)	-0.004 (0.950)
Observations	562	562	562
Post window: (500, 529)			
Unionization	0.038 (0.266)	0.022 (0.113)	-0.011 (0.828)
Observations	501	501	501
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Table SA-6. Unionization and firm risk: Total change in firm risk

This table presents the RDD results for the effect of unionization on total change in firm risk. We report the estimates from the global polynomial model specified in Equation (2). The dependent variable is the change in firm risk (*ΔRisk*), measured as the difference between the average value of the option-implied risk measure in the 10 trading days after the tally date and the average value of the option-implied risk measure in the 10-day window that ends 45 days prior to the petition filing date. We exclude the 2 trading days after the tally date from the calculation of the post-election average value. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

.

	Dependent variable: *ΔRisk*		
Variables	*ΔATM* (1)	*ΔLSKEW* (2)	*ΔVRP* (3)
Unionization	0.023 (0.380)	-0.005 (0.730)	-0.003 (0.961)
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	555	555	555

Table SA-7. The effect of financial distress: Alternative proxies

This table presents the RDD results from global polynomial regressions for subsamples of firms with different levels of financial distress. We measure financial distress using Ohlson's O-score (–1.32 – 0.407 $\times$ size + 6.0 $\times$ liability ratio – 1.43 $\times$ working capital/total assets + 0.0757 $\times$ current liabilities/current assets – 1.72X – 2.37 $\times$ net income/total assets – 1.83 $\times$ funds from operations/total liabilities + 0.285Y – 0.521 $\times$ (net income(t) – net income(t−1)) / (|net income(t)| + |net income(t−1)|)), where X is an indicator for total liabilities being larger than total assets, and Y is an indicator for net losses in the past two years. For each year, we define firms with below (above)-median O-score as distressed (healthy). We also measure financial distress using credit ratings. We classify firms with a rating of "BBB-" or above as healthy and those with a rating below it as distressed. The change in firm risk (*ΔRisk*) is measured as the difference between the average value of the option-implied risk measure in the 10 trading days before and after the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. Panel A reports the results for distressed firms and Panel B shows the results for healthy firms. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

Panel A. Distressed firms

Variables	*ΔATM* (1)	*ΔLSKEW* (2)	*ΔVRP* (3)
O-score			
Unionization	-0.001	0.020	0.054
	(0.962)	(0.175)	(0.330)
Observations	261	261	261
Credit ratings			
Unionization	0.005	0.009	0.073
	(0.925)	(0.623)	(0.481)
Observations	133	133	133
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Panel B. Healthy firms

Variables	*ΔATM* (1)	*ΔLSKEW* (2)	*ΔVRP* (3)
O-score			
Unionization	-0.006	-0.010	-0.037
	(0.653)	(0.412)	(0.370)
Observations	260	260	260
Credit ratings			
Unionization	-0.009	0.015	0.061
	(0.284)	(0.125)	(0.130)
Observations	371	371	371
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Table SA-8. The effect of financial constraints: Alternative proxies

This table presents the RDD results from global polynomial regressions for subsamples of firms with different levels of financial constraints. We measure the degree of financial constraints using firm size (*Size*) and the payout ratio, respectively. For each year, we define firms with below (above)-median firm size or payout ratio as constrained (unconstrained). The change in firm risk (*ΔRisk*) is measured as the difference between the average value of the option-implied risk measure in the 10 trading days before and after the tally date. We exclude the 5 trading days around and including the tally date from these calculations. *Unionization* is a dummy variable that takes the value of 1 when vote share is above the 50% cutoff point and zero otherwise. Only the coefficients for *Unionization* are reported. Columns 1 to 3 present the coefficient estimates for *Unionization* when the dependent variable is *ΔATM, ΔLSKEW,* and *ΔVRP,* respectively. Panel A reports the results for financially constrained firms and Panel B shows the results for financially unconstrained firms. All regressions include industry and year fixed effects. The p-values in parentheses are based on robust standard errors clustered at the firm level.

Panel A. Constrained firms

.

Variables	*ΔATM* (1)	*ΔLSKEW* (2)	*ΔVRP* (3)
Size			
Unionization	-0.007	-0.009	0.021
	(0.736)	(0.585)	(0.753)
Observations	285	285	285
Payout ratio			
Unionization	-0.011	0.016	0.019
	(0.612)	(0.351)	(0.797)
Observations	283	283	283
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Panel B. Unconstrained firms

.

Variables	*ΔATM* (1)	*ΔLSKEW* (2)	*ΔVRP* (3)
Size			
Unionization	-0.008	0.021	0.042
	(0.427)	(0.032)	(0.306)
Observations	286	286	286
Payout ratio			
Unionization	-0.004	0.005	0.027
	(0.683)	(0.599)	(0.401)
Observations	284	284	284
Polynomial order	3	3	3
Industry fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes