Labor Unions and Workplace Safety*

Ling Li[†] Shawn Rohlin [‡] Perry Singleton [§]

May 26, 2020

Abstract

The authors examine the effect of labor unions on workplace safety. For identification, the authors exploit the timing and outcome of union elections, using establishments in which elections narrowly fail as a comparison group for establishments in which elections narrowly pass. Data on elections come from the National Labor Relations Board, and data on workplace safety come from the Occupational Safety and Health Administration. The results indicate that unionization had no detectable effect on accident case rates at the mean, but shifted downward the case rate distribution below 2 cases per 100 full-time equivalent workers annually. The downward shift is most evident among larger bargaining units and manufacturing establishments. The results at the higher end of case rate distribution are inconclusive.

Keywords: labor unions, workplace safety, occupational safety, OSHA
JEL Codes: J28, J51, J81

Corresponding Author: Perry Singleton; Syracuse University; 426 Eggers Hall; Syracuse, NY 13244; psinglet@syr.edu

*For helpful comments and suggestions, the authors would like to thank Gary Engelhardt, Brigham Frandsen, Barry Hirsch, Hugo Jales, Jeffrey Kubik, and conference participants at the Annual Meeting of the Society of Labor Economists. The authors would also like to thank Jeanette Walters-Marquez for providing data from the Federal Mediation and Conciliation Service.

[†]University of Wisconsin - Parkside, Department of Economics

[‡]Kent State University, Department of Economics, and the Center for Entrepreneurship and Business Innovation

[§]Syracuse University, Department of Economics, and the Center for Policy Research

Workers form labor unions to bargain over wages, employment, and working conditions. One facet of working conditions - and a focus of labor union efforts - is workplace safety. At a systemic level, labor unions were key proponents in establishing the Occupational Safety and Health Administration (OSHA) and the Mining Safety and Health Administration, which regulate and promote workplace safety in the United States (Schurman et al., 1998). At the establishment level, labor unions induce employers to maintain safe workplaces, educate workers about workplace hazards, influence the stringency of regulatory oversight, and develop safety-related innovations through economies of scale (Morantz, 2009). Despite these safety-enhancing activities, the empirical literature on a union safety effect is inconclusive, and many studies find that unions are associated with more non-fatal injuries (Donado, 2015). Thus, the effect of labor unions on workplace safety remains an open question.

To contribute to the literature, we attempt to identify the causal effect of unionization on workplace safety. Following several studies (DiNardo and Lee, 2004; Frandsen, 2012, 2014; Lee and Mas, 2012; Sojourner et al., 2015; Sojourner and Yang, 2015), we identify the effect using the regression discontinuity model that exploits the timing and outcome of union elections, whereby establishments in which elections narrowly fail serve as a comparison group for establishments in which elections narrowly pass. The identification assumption is that establishments just above and below the cutoff for a successful union election are comparable with respect to observable and unobservable characteristics so that any difference in workplace safety at the cutoff is attributable to unionization. Data on union elections come from the U.S. National Labor Relations Board (NLRB), and the data on occupational safety come from the Occupational Safety and Health Administration (OSHA), specifically the OSHA Data Initiative (ODI). These data report the rate of cases involving days away from work, job restrictions, and job transfers (DART) per 100 full-time equivalent workers annually.

This study contributes uniquely to the literature on labor unions and workplace

safety. First, to our knowledge, we are the first to identify the effect of unionization on workplace safety by exploiting close union elections. The most relevant study to ours is by Sojourner and Yang (2015), who use a similar strategy to examine the effect of unionization on regulatory enforcement through OSHA. The empirical strategy addresses the concern that more dangerous establishments are more likely to unionize, which would bias against finding a union safety effect (Donado, 2015). Second, the data on workplace safety is unique to the literature. The data allow analyses of multiple industries, whereas several related studies focus on specific eras and industries. Finally, we examine heterogeneous effects across the case rate distribution by the size of the bargaining unit and industry.

1 Background

1.1 Labor Unions and Workplace Safety

The economic framework of labor unions provides insights on why and how unions affect workplace safety. In *What Do Unions Do?*, Freeman and Medoff (1984) describe two facets of labor unions. First, unions serve as a collective voice. With legal protections, unionized workers may be more willing to express their preferences for workplace safety without fear of retaliation, and labor unions may be more effective at aggregating, communicating, and promoting worker preferences in negotiations with management. Through collective voice, both workers and management could benefit. For example, labor unions could address coordination failures among workers and reduce information asymmetries between workers and management

Second, workers form labor unions to create or capture monopoly rents, which benefits unionized workers at others' expense (Farber, 1986). Similar to wages, labor unions may bid up workplace safety, imposing new costs to the firm. The costs could be both direct - for example, the cost of new safety-enhancing technology or equipment - as well as indirect

through lost productivity. Labor unions may also bid up wages in lieu of safety (Donado, 2015), though this tradeoff does not necessarily require monopoly power through collective bargaining.

In practice, labor unions engage in numerous activities to improve workplace safety. As Morantz (2009) notes, labor unions induce employers to maintain safe workplaces, educate workers about workplace hazards, influence the stringency of regulatory oversight, and develop safety-related innovations through economies of scale. For example, agreements to provide safety information directly from employers to workers are more common among unionized workers (Viscusi, 1979), and unionization appears to increase the rate of OSHA inspection (Sojourner and Yang, 2015; Weil, 1991).

Despite the numerous activities of labor unions to improve workplace safety, their causal effect is difficult to identify empirically. First, more dangerous establishments may be more likely to unionize. This could arise from the direct effect of poor working conditions on unionization as well as the indirect effect of other factors that affect both working conditions and unionization, such as management quality. Second, workplace accidents are difficult to measure objectively, and unionization may itself affect the propensity to report an accident. For example, unionized establishments may be more likely to accommodate workers following an accident, thereby increasing the reported case rate. Also, unionized establishments may be more likely to report an accident due to an employer's tendency to underreport in the absence of unionization, a union's tendency to overreport, or both. Both empirical complications - referred to as selection bias and reporting bias, respectively - work against finding a union safety effect.

Due to these empirical difficulties, the empirical evidence on the union safety effect is inconclusive. Stated above, most studies find that unions are associated with more non-fatal injuries (Donado, 2015). A few studies find that unions improve workplace safety, but these findings pertain to specific eras and industries. For example, Boal (2009) examines turn-of-the-century coal mining, exploiting within-mine variation in union status, and

Morantz (2013) focuses on mining-related injuries and fatalities in the 1970s and 1980s, exploiting mostly across-mine variation in union status.

1.2 Union Elections

In the US, workers typically form unions through elections.[1] Elections are facilitated by the National Labor Relations Board (NLRB), established in 1935 to enforce collective bargaining laws. To hold an election, organizers must first demonstrate at least 30 percent support for a union election among eligible workers. This is achieved by petitions or authorization cards. If successful, the NLRB determines the size and scope of the bargaining unit and the time and location of the election. The election is conducted by secret ballot, and a successful election requires a simple majority. If an election is successful, employers must bargain "in good faith" with the union during contract negotiations.

Based on the criterion for a successful union election, DiNardo and Lee (2004) develop a framework for union bargaining power. In their framework, bargaining power is a function of the share of voters who favor unionization. In a baseline case, where union elections are permitted, but none occur, bargaining power increases monotonically with the vote share. If an election occurs, bargaining power increases independent of the election outcome, referred as the indirect effect of unionization. If the election is successful, bargaining power increases further, referred as the direct effect of unionization. As DiNardo and Lee (2004) note, because a successful election requires a simple majority, bargaining power increases discontinuously at the 50-percent vote share.

2 Empirical Strategy

To identify the effect of labor unions on workplace safety, the empirical strategy exploits close union elections, using establishments in which an election narrowly failed as a

[1] An employer may independently recognize a labor union, forgoing a union election.

comparison group for establishments in which an election narrowly passed. The identification is similar to related studies, including DiNardo and Lee (2004); Frandsen (2012, 2014); Lee and Mas (2012); Sojourner et al. (2015); and Sojourner and Yang (2015).

Specifically, the strategy identifies $E[Y(W=1) - Y(W=0)|X=0]$, where W is an indicator of union status, Y is an outcome variable that is a function of union status, and X is the vote share relative to the 50-percent cutoff.[2] The key identification assumptions are that $E[Y(1)|X]$ and $E[Y(0)|X]$ are smooth at $X=0$ and that $W=1$ and $W=0$ for all establishments above and below the threshold, respectively. If so, the discontinuity in W at the cutoff may generate a discontinuity in Y, which reflects the causal effect of the former on the latter.

Estimation and inference can be accomplished through parametric polynomial regression. The regression model has the following form:

$$Y_i = \alpha + \beta W_i + F(X_i) + W_i G(X_i) + \gamma Z_i + \varepsilon_i \tag{1}$$

The variable Y_i is a measure of workplace safety following the union election, W_i is an indicator of a successful union election, X_i is the vote share relative to the cutoff, and Z_i is a vector of election and establishment characteristics.[3] $F(.)$ and $G(.)$ are polynomial functions of the vote share. By interacting $G(X_i)$ with W_i, the model allows for separate conditional expectation functions above and below the cutoff. The error term ε_i is robust to heteroskedasticity.[4]

The coefficient of interest is β. This measures the discontinuity in workplace safety at the cutoff and, given the identification assumptions, is interpreted as the conditional

[2]To impose symmetry in the vote share distribution regardless of the number of vote cast, an amount equal to 0.5 divided by the number of votes cast is subtracted from the vote share if the number of votes cast is even (DiNardo and Lee, 2004).

[3]Because the conditional distribution of covariates Z_i is assumed continuous at the cutoff, including them in equation (1) does not affect the identification strategy. Their inclusion, nonetheless, may reduce small sample bias and improve the precision of the estimates (Imbens and Lemieux, 2008).

[4]Discussed below, some establishments have multiple observations of occupational safety in different calendar years. In the empirical analysis, the standard errors are clustered at the election level.

average treatment effect of unionization. Because the effect is identified locally, estimation utilizes observations only within a symmetric bandwidth around the cutoff. The empirical analysis considers both first-order polynomials with a narrow bandwidth and second-order polynomials with a wide bandwidth.

The effect of unionization on workplace safety may differ across the case-rate distribution. For example, unions may focus their safety-enhancing efforts on high case-rate establishments, affecting only the right tail of the case-rate distribution. The effect could also differ by establishment characteristics - for example, industry - that are systematically correlated with workplace safety. To estimate distributional effects, the outcome variable is replaced with an indicator function $1(Y_i \leq y)$, and β is replaced with β_y, which measures the discontinuity of the conditional cumulative density function evaluated at y (Frandsen et al., 2012).

3 Data and Sample

3.1 Union Elections: National Labor Relations Board

Data on union elections come from the NLRB. One source of data is compiled by the AFL-CIO, which contains elections held from 1965 to 1998.[5] Another source is an online data repository, *www.data.gov*, which contains annual NLRB files from 1999 to 2010.[6] Data from both sources include an establishment's name, address, and industry, as well as the number of eligible voters, valid votes cast, and votes for and against unionization.

Combined, the data contain 45,582 elections from 1991 to 2010.[7] These years

[5]The AFL-CIO is the American Federation of Labor and the Congress of Industrial Organizations. The data are available to download from John-Paul Ferguson at https://github.com/jpfergongithub/nlrb_old_rcases.

[6]The data files are labeled by calendar years, but the file name does not necessarily correspond with tally year of the elections within the file. After pooling the files, observations were deleted if they appeared to be duplicates or were petitions that were withdrawn or dismissed cases. For more details of the NLRB files, see the Appendix.

[7]There were 455 elections that were omitted due to a missing or invalid vote share in favor of unionization.

coincide with the years of data on workplace safety described below. The first data source contains 21,917 elections from 1991 to 1998, and the second contains 23,665 elections from 1999 to 2010.[8] The annual number of elections decreased over time, from 2,855 in 1991 to 1,644 in 2010.[9] To ensure uncertainty in the election outcome, the data are restricted to elections with at least 20 valid votes, leaving 24,758 elections from 1991 to 2010. This restriction is similarly imposed in related studies, including DiNardo and Lee (2004), Lee and Mas (2012), Frandsen (2014), and Sojourner et al. (2015).

Table 1 provides summary statistics of the elections. The average number of votes cast is 97.27, and the share of elections that pass is 46.77 percent. The greatest share of the elections is in manufacturing (28.94 percent), followed by health services (19.63 percent) and transportation (16.86 percent). A greater share of elections occurred in the Northeast and Midwest, compared to the South and West.

3.2 Workplace Safety: OSHA Data Initiative

Data on workplace safety come from the ODI. The ODI was part of OSHA's Site Specific Targeting (SST), which was designed to better target more dangerous establishments for an inspection. To do so, the ODI first collected accident case rates directly from employers at the establishment level, and the SST plan used these data to target high case rate establishments for inspection.[10] The data were collected in annual cycles covering years 1996 to 2011. Eligible establishments were identified from a business registry compiled by Dun & Bradstreet, and the scope of the data collection changed from year to year by industry and establishment size, with a goal to sample targeted establishments at least once every three years (Johnson et al., 2017). Also, some states did not participate in the ODI. For example,

[8]Given the available data, the year is based on the date of the election using the files from 1991 to 1998 and based on the date of vote tally using the files from 1999 to 2010. Using a subset of data that report both dates, the year of the vote and year of the vote tally are the same for 93 percent of cases.

[9]The number of elections is lower in 1999 compared to 1998 and 2000 - 1,686 compared to 2,761 and 2,867, respectively - suggesting some missing data in that year.

[10]Using the ODI data, Li and Singleton (2019) exploit the SST plan to identify the effect of workplace inspections on worker safety.

in 2010, ODI data were not collected in Alaska, Oregon, South Carolina, Washington, and Wyoming. Construction was generally excluded from data collection, and some industries cycled in and out.[11] The scope was limited to establishments with at least 60 employees in 1996, at least 50 employees in 1997, at least 40 employees from 1998 to 2009, and at least 20 from 2010 onwards. In some cases, establishments with a case rate exceeding a cutoff in one year were more likely to appear in the ODI the following year. [12] For these reasons, the ODI data are not representative of all business establishments and thus not directly comparable across collection cycles.[13]

The ODI data on workplace safety come specifically from OSHA's Form 300. This form is provided by OSHA to employers to log workplace accidents and injuries. In general, employers with 10 or more full-time employees are required to complete the form. Cases are logged separately involving death, days away from work, job restrictions or transfers, and medical attention beyond first aid. Based on these logs, the ODI reports accident case rates per 100 full-time equivalent workers annually. The total case rate (TCR) includes all four cases, and a second rate includes only cases involving days away from work, job restrictions, and job transfers (DART).[14] The initial focus of the empirical analysis will be on the DART case rate, which is arguably more objective and thus reported more accurately than the TCR.[15]

Unfortunately, the ODI data are not suited to examine extreme values of the case-rate distribution. As OSHA notes, recording errors may exist for a small percentage of

[11] As Li and Singleton (2019) note, dairy farms were covered in 1998, but not in 2000, and ornamental nurseries were covered in 2000, but not in 1998.

[12] For example, establishments observed in 1996 were significantly more likely to be observed in 1997 if their case rate involving days away from work, job restrictions, and job transfers exceeded seven. From 2004 to 2005, this cutoff was six.

[13] Because the sampling frame changed from cycle to cycle, matched ODI observations are not directly comparable across calendar years or analysis periods. This prevents a straightforward event-study analysis, which compares changes in the mean case rate before and after the union election.

[14] In calendar years 2002 to 2011, the ODI reports a third case rate that includes only days away from work.

[15] For this reason, to study the effect of OSHA inspections on workplace safety, Johnson et al. (2017) focus on a third case rate which includes only cases involving days away from work. We do not focus this case rate because these data are only available for calendar years 2002 to 2011, which limits the number of observations and thus statistical precision.

establishments, and establishments with the highest rates are not accurate in absolute terms. For this reason, when noted, the data are trimmed or winsorized to address extreme values in the case-rate distribution.

The NLRB elections are matched to each year of the ODI based on the establishment name and address. More details of the data matching procedures are provided in the Appendix. Of the 24,758 union elections, 6,976 have at least one match to the ODI across all the available years of data. Although the match rate may seem low, the ODI is restricted by industry, establishment size, injury rate, and state participation. Also, elections closer to 1991 and 2010 were less likely to match to the ODI than elections in the intervening years, as the ODI data span from 1996 to 2011. A more advantageous scenario for matching, for example, would be union elections in 2004, in manufacturing, in states that participate. Among the 263 elections in this case, 185 have at least one match to the ODI, a match rate of 70.3 percent.

Table 1 provides summary statistics of elections with and without a match. The number of valid votes is greater among elections with a match, which is consistent with the ODI excluding establishments with fewer employees, though the valid votes cast reported in the NLRB pertain only to the bargaining unit and may not include all employees of an establishment. Elections with a match are also less likely to have passed: 40.25 percent versus 49.37 percent. Regarding industry and geography, elections with a match are more likely to be in manufacturing and health services, compared to transportation, and more likely to be in the Midwest, compared to the Northeast, South, and West.

A single election may match to multiple ODI records in different years. Among the 6,976 elections with at least one match to the ODI, there are 38,004 total matches, and 19,318 matches specifically from five calendar years before the election to five calendar years after. During this window, 17.06 percent of the 6,976 elections have no matches, 24.68 percent have one match, 15.05 percent have two matches, and 43.21 percent have three to eleven matches. Because establishments that reported high case rates in one year were more

likely to appear in the ODI the following year, more matches are associated with higher case rates.

Figure 1 illustrates the ODI match rate each year relative to the year of the election. The match rates are calculated using only calendar years for which ODI data are available.[16] As shown, the match rate is highest in the year of the election, when the establishment is known to exist. In that year, the match rate is 11.73 percent. The match rate gradually declines with years before and after the election, which is consistent with establishment formation and dissolution, respectively.

4 Support for Identification Strategy

Similar to related studies, we provide auxiliary analysis to support the identification strategy, with details provided in the Appendix. First, we confirm that union activity increases following a successful union election. This is achieved by matching the election data from 1999 to 2010 to "notices of bargaining" data in years 1997 to 2016 from the Federal Mediation and Conciliation Service (FMCS).[17] A notice is required to initiate, terminate, or modify a labor contract and is therefore an indicator of union activity.[18] Using these data, we show that a successful union election is associated with a spike in union activity in the calendar year of the election and the year after (Appendix Figure 1) and that the spike in union activity occurs discontinuously at the cutoff for a successful union election (Appendix Figure 2 and Appendix Table 1).

Second, we test for discontinuities in the vote share distribution at the cutoff for a successful union election. This addresses the concern for non-random sorting, which occurs when the vote share is manipulated at the margin of victory to alter the election outcome. We find graphical evidence that suggests too few narrow election victories (Appendix Figure

[16]For example, elections tallied in 1999 were not used to calculate the ODI match rates in periods -4 and -5, which correspond to calendar years 1995 and 1994, respectively.

[17]DiNardo and Lee (2004) similarly match union election data to FMCS data.

[18]The NLRB elections are matched to FMCS records by establishment name and address.

3), but the McCrary (2008) test fails to reject continuity at the 5 percent level. Nonetheless, one strategy to address excess or missing density at the cutoff is to focus on union elections with many votes cast, where vote share manipulation to affect the election outcome is not only more difficult, but more likely to be uncertain locally at the cutoff for a successful union election (Lee, 2008).

Finally, we estimate discontinuities in the conditional mean function of establishment and election characteristics, which also addresses the concern for non-random sorting near the cutoff. Specifically, we estimate discontinuities in eligible employees, valid votes cast, industry (manufacturing and health services), and whether an establishment had any match to the ODI three calendar years before the election (Appendix Figure 4 and Appendix Table 1). The ODI match rate is constructed only before the election since unionization may affect the accident case rate and, due to ODI targeting, a high case rate in one year increases the likelihood of an ODI match in the next year. We find that the discontinuity estimates are generally small relative to the conditional mean near the cutoff, and none of the discontinuity estimates are statistically significant.

A separate concern is that unionization may affect selection into the ODI after an election, which could bias the estimated effect of unionization on workplace safety. Specifically, if unionization affects the case rate, this change may shift the case rate across the cutoff for ODI targeting in the subsequent year. To examine this further, we estimate the discontinuity in whether an establishment had any match to the ODI three calendar years after the election and find that the discontinuity estimates are positive, but small and statistically insignificant (Appendix Figure 4 and Appendix Table 1). We therefore conclude that endogenous selection due to ODI targeting should not threaten the identification strategy.

5 Results for Workplace Safety

5.1 Mean Effects

To identify the direct effect of unionization on workplace safety, the empirical strategy focuses on establishments in which elections narrowly pass or fail. The causal effect is measured by the discontinuity in workplace safety at the cutoff for a successful election. Discontinuity analysis is presented both before and after the election. The pre-election analysis helps to assess non-random sorting at the cutoff, but it is important to note that establishments that match to the ODI before the election are not directly comparable to those that match after the election, as the ODI changed from year to year.[19] To improve statistical precision, the data are pooled across one to three years before and after the election for the pre- and post-election analysis, respectively. There are two reasons for restricting analysis shortly after the election. First, establishments in which an election narrowly failed may have a subsequent election, which would increasingly contaminate the comparison group with treatment. Second, unionization may effect establishment survival, and this effect may differ by workplace safety. While DiNardo and Lee (2004) and Freeman and Kleiner (1999) find no effects of unionization on firm survival, Brown and Heywood (2006) and Frandsen (2014) suggest unionization decreases firm survival. In Frandsen (2014), the effect on survival is evident only three years after an election.

To evaluate discontinuities in the average DART rate, the first panels of Figures 2 and 3 plot the conditional mean of the DART rate across 20 mutually exclusive bins. Following Frandsen (2014), the markers are shaded to indicate the relative number of observations within each bin.[20] Figure 2 corresponds to pre-election, and Figure 3 corresponds

[19] Compared to establishment-by-ODI matches pre-election, matches post-election are 2.21 percentage points less likely to correspond to an election that passed, 2.58 percentage points more likely to be in manufacturing, 3.29 percentage points less likely to be in the Northeast, and 3.21 percentage points more likely to be in the Midwest.

[20] If the number of bin observations exceeds 660, the marker is shaded grey scale 0, which is black. For every 40 fewer observations, the marker is shaded one additional point on the grey scale. For example, if the number of observations is between 620 and 660, the marker is shaded grey scale 1.

to post-election. In both periods, the average DART rate increases slightly with the vote share in favor of unionization, suggesting that support for unionization is greater among more dangerous establishments. Importantly, there is no apparent discontinuity in mean workplace safety at the cutoff in either period.

Using equation (1), Table 2 presents discontinuity estimates pre-election, and Table 3 presents discontinuity estimates post-election. The first row presents estimates for the average treatment effect, and the columns correspond to discontinuity estimates from a single model, which vary by bandwidth, polynomial order, and the exclusion or inclusion of covariates. Given the available data, covariates include industry by calendar year fixed effects, state by calendar fixed effects, and the natural log of valid votes cast.

Consistent with Figure 3, the estimates do not indicate a sizeable discontinuity in the DART rate pre- or post-election. Although the discontinuity estimates post-election are generally negative, suggesting an improvement in workplace safety, they are less than 0.5 in absolute value per 100 full-time equivalent workers annually, compared to a mean near the cutoff of approximately 8, and none of the estimates are statistically significant. Within each model, the estimate is smaller post-election relative to pre-election, suggesting an improvement in workplace safety. None of the pre versus post differences, however, are statistically significant. Regarding non-random sorting, all the estimates are positive before the election, suggesting that, if anything, non-random sorting generates a positive bias in the estimated effect of unionization on the DART rate, so that the effect of unionization on workplace safety would be underestimated.

5.2 Distributional Effects

To evaluate discontinuities in the DART rate distribution, Figure 4 illustrates the cumulative density function (CDF) of the DART before and after the election separately for the five percentage point bins just above and below the cutoff. Before the election, the CDF is lower above the cutoff, up to a DART of 17 per 100 full-time equivalent workers annually.

This is consistent with the first panel of Figure 2, where the average DART is greater in the bin above the cutoff compared to the bin below the cutoff. According to the first row of Table 2, however, the discontinuity estimates at the cutoff are small and statistically insignificant. After the election, the CDF is instead greater above the cutoff, specifically at a DART less than 3. This suggests a downward shift DART rate distribution following a successful union election. This is consistent with the first row of Table 3, where the discontinuity estimates are negative, but remain statistically insignificant.

To further evaluate discontinuities in the DART rate distribution, the other panels in Figures 2 and 3 plot the CDF of the DART evaluated at integers from 0 to 4. The only apparent discontinuities appear post-election for DART<1 and DART< 2 per 100 full-time equivalent workers annually, consistent with Figure 4. For example, the share with DART<1 appears to decrease from the left towards the cutoff, then increases discontinuously at the cutoff, suggesting an improvement in workplace safety. In contrast, the estimates pre-election appear noisier across bins, reflecting fewer observations in the pre-election period.

Using equation (1), Table 2 presents discontinuity estimates pre-election, and Table 3 presents discontinuity estimates post-election. Discontinuity estimates are reported at each integer up to and including DART<10. Consistent with Figures 2 and 3, the discontinuity estimates are positive post-election for DART<1 and DART<2, which is robust across specifications. Moreover, many estimates are statistically significant at the five percent level based on the pointwise standard errors reported in paretheses. In contrast, the discontinuity estimates pre-election for DART<1 and DART< 2 are generally negative and statistically insignificant, though the standard errors are larger due to fewer observations. Nonetheless, in some cases, the difference in estimates post- and pre-election within the same model are statistically significant. For example, in column (3) for DART<1, the discontinuity estimate is -1.78 percentage points pre-election and 7.07 percentage points post-election. The difference of 8.847 percentage points is statistically significant at the 5 percent level.

The other effects are inconclusive. Post-election for DART<3 and above, the es-

timates are both positive and negative, and most are smaller in magnitude compared to those for DART<1 and DART< 2. None of the estimates are statistically significant. Pre-election, all estimates are negative, and several are large relative to the mean, particularly at the higher end of the case-rate distribution. None of the estimates are statistically significant, nor are they robust across specifications. This is consistent with Figure 3, where the estimated shares appear too noisy for inference.

Because multiple hypothesis tests are conducted using the same sample and model, one concern is that some estimates will be statistically significant even if the null is true. To address this concern, we calculate sup-t critical values for 90 and 95 percent confidence (Olea and Plagborg-Moller, 2019). The sup-t approach considers all estimates within the same sample and model to be a random vector, and the set of confidence intervals based on the sup-t critical value for 95 percent confidence contains the entire vector in 95 percent of samples. To calculate the sup-t critical values, we first estimate the variance-covariance matrix of the discontinuity estimates within each sample and model by boostrapping.[21] We then draw randomly from a multivariate normal distribution with the estimated variance-covariance matrix.[22] The sup-t critical value for 95 percent confidence is the supremum t-value such that at least 95 percent of the realized vectors lie within the set of confidence intervals.

The sup-t critical values are reported at the bottom of Table 3. The sup-t critical values, combined with the pointwise estimates and standard errors, yield the sup-t confidence intervals. In all cases, the confidence intervals include zero, so none of the estimates are statistically significant using this more restrictive criterion.

Taken together, the results suggest that unionization may improve workplace safety, particularly by shifting the DART rate distribution downwards at a rate less than 2. The effects are statistically significantly based on pointwise standard errors, but statistically insignificant when addressing multiple hypotheses. One possibility is that the effect of unions

[21]For each sample and model, we bootstrap the sample 1,000 times.

[22]For each sample and model, the number of draws equals the sample size.

on workplace safety varies by establishment characteristics, and establishments with low DART rates differ systematically from other establishments. In Table 4, summary statistics of establishments are reported separately by DART rate less than 2 per 100 full-time equivalent workers annually. In general, establishments with a lower DART rate have fewer eligible voters and votes cast. They are also more likely to be in construction and manufacturing, and less likely to be in health services. Thus, the results could reflect that unions target their workplace safety efforts at establishments with fewer votes, establishments in manufacturing, or both, or that their efforts at these establishments are more effective.

5.3 Votes Cast

The discontinuity estimates may differ by votes cast in the election. First, the causal effect of unions on workplace safety may depend on the size of the bargaining unit. Stated above, unions could target their workplace safety efforts on larger or smaller bargaining units, or their efforts may be more or less effective. Second, the bias due to non-random sorting may be smaller among larger bargaining units. Among larger bargaining units, vote share manipulation to affect the election outcome is not only more difficult, but the election outcome is more uncertain locally near the cutoff (Lee, 2008). For these reasons, it is important to consider whether the baseline results are evident among larger bargaining units.

To examine heterogeneous effects, we focus on establishments with 70 votes or more, splitting the sample about evenly. We first replicate Appendix Table 1 to confirm both the first stage in union activity and smoothness in observable characteristics (not shown). We then replicate Figure 3 using post-election data from periods 1 to 3, and the results are illustrated in Figure 5. As shown, the discontinuity at the cutoff is more pronounced among larger bargaining units. This is confirmed in Table 5, which presents discontinuity estimates using equation (1). In all specifications, the downward shift in DART$<$1 and DART$<$ 2 is larger than the shift among all establishments reported in Table 2. Morever, in some cases,

the discontinuity estimates are statistically significant using the sup-t critical values. For DART<3 and above, the results are inconclusive due to both smaller estimates and larger standard errors. Additionally, a downward shift is not evident among smaller bargaining units (Appendix Table 2).

5.4 Manufacturing

The discontinuity estimates may also differ by industry. One reason is that most unions are industry or occupation specific, and some unions may be more effective than others at affecting workplace safety. Another reason is that accidents and injuries differ by industry and occupation, and some accidents and injuries may be more affected by union activity than others.

To examine heterogeneous effects, we focus on establishments in manufacturing, the single largest two-digit industry in the data. Again, we first replicate Appendix Table 1 to confirm both the first stage in union activity and smoothness in observable characteristics (not shown). We then replicate Figure 3 using post-election data from periods 1 to 3, and the results are illustrated in Figure 6. As shown, the discontinuity at the cutoff is more pronounced among establishments in manufacturing. This is confirmed in Table 6, which presents discontinuity estimates using equation (1). In all specifications, the downward shift in DART<1 and DART< 2 is larger than the shift among all establishments reported in Table 2. Morever, in some cases, the discontinuity estimates are statistically significant using the sup-t critical values. Similarly, for DART<3 and above, the results are inconclusive. Additionally, a downward shift is not evident among non-manufacturing establishments (Appendix Table 3).

5.5 Robustness Tests

Taken together, the results suggest that unionization shifted down the DART rate distribution below 2 per 100 full-time equivalent workers annually, and this downward shift is most evidence among larger bargaining units and manufacturing establishments. We consider several robustness tests for these baseline results.

First, we additionally control for workplace safety before the election, since some establishments have ODI matches both before and after the election. Specifically, we include the average DART rate during the five years before the election. If an establishment has no matches, this variable equals zero. To control no matches, we include a set of indicator variables for the number of matches, which ranges from zero to five.[23] We find that the baseline results in Table 3 are robust to controls for pre-election safety. This is consistent with the identification assumption that establishments below the cutoff are comparable to establishments above the cutoff, including with respect to workplace safety.

Second, we restrict the analysis to establishments that had only one election during an eleven year a window spanning five calendar years before the one election and five years after.[24] This addresses the concern that establishments in which an election fails may have a subsequent election that is successful, contaminating the comparison group with treatment. Again, we find that the baseline results are robust with the restricted sample. In fact, the discontinuity estimates for DART<1 and DART<2 are slightly larger than the estimates in Table 3, which is consistent with contamination.

Third, we extend the analysis period from one to three years after the election to one to five years after.[25] Again, we find that the baseline results are robust, and the discontinuity estimates are more statistically significant due in part to more observations.

Fourth, we examine the sensitivity of the results to the matching algorithm linking

[23] In specifications (1) and (2) of Table 3, for example, 45.84 percent of establishments have no DART matches pre-election.

[24] In specifications (1) and (2) of Table 3, for example, the sample size decreases from 3,328 to 2,515.

[25] In specifications (1) and (2) of Table 3, for example, the sample size increases from 3,328 to 5,500.

the election data from the NLRB to the workplace safety data from the ODI. Specifically, we omit elections that matched to the ODI using the least restrictive criterion, which uses the first six digits of the establishment name, the first six digits of the street, full city, and state.[26] In this case, the downward shift is even more apparent, evident from DART=0 to DART<3.

Finally, using the ODI data, we examine the effect of unionization on the TCR, a broader measure of accidents than the DART rate. In addition to cases involving days away from work, job restrictions, and job transfers, the TCR includes cases involving death and medical attention beyond first aid. Workplace deaths are relatively rare - there were 5,190 workplace deaths in 2017 (Department of Labor, 2018) - so the major difference between the DART and the TCR are cases involving medical attention beyond first aid. We find that the results for the TCR are qualitatively similar to those for the DART. For TCR<2, the discontinuity estimates are generally negative pre-election and positive post-election, suggesting an improvement in workplace safety. In the post-election period, the point estimates are relatively larger and statistically significant across specifications. For TCR<3 and above, the discontinuity estimates are smaller and statistically insignificant both pre- and post-election.

6 Conclusion

Labor unions engage in numerous activities to improve workplace safety. To identify their causal effect, this study exploits close union elections, using establishments in which elections narrowly fail as a comparison group for establishments in which elections narrowly pass. According to the results, unionization had a negative but small and statistically insignificant effect on the mean DART rate, which was approximately 8 per 100 full-time equivalent workers annually both pre- and post-election. The effect of unionization on workplace safety is more evident when examining distributional effects. In particular,

[26] In specifications (1) and (2) of Table 3, for example, the sample size decreases from 3,328 to 2,697.

unionization shifted the DART rate distribution downward below a DART rate of 2, an improvement in workplace safety. For example, in years one to three following a union election, the share of establishments with a DART rate less than 2 increased discontinuously at the cutoff for a successful union election, with estimates ranging from 5.41 percentage points to 9.68 percentage points. Additionally, the downward shift in the case-rate distribution is most evident among larger bargaining units and manufacturing establishments. As Morantz (2009) notes, larger bargaining units may differ by their ability to form health and safety committees, conduct inspections independently, and increase and enforce regulatory compliance.

We consider several robustness tests for the baseline results. These include controlling for pre-election workplace safety, restricting the sample based on multiple elections, expanding the analysis period beyond three years, and testing the sensitivity of the results to the data matching algorithm. We also note that the baseline results likely reflect real improvements in workplace safety, rather than changes in reporting. This is because employers are likely to underreport accidents in the absence of unionization, and unions are likely to overreport, both of which would bias against finding a union safety effect.

This study contributes uniquely to the literature on labor unions and workplace safety. First, we identify the effect of labor unions on workplace safety by exploiting narrow union elections. This addresses the concern that more dangerous establishments are more likely to unionize, which biases against finding a union safety effect (Donado, 2015). Second, the ODI data is unique to the literature on unions and workplace safety. This allows analyses of multiple industries, whereas several related studies focus on specific eras and industries. Finally, we find heterogeneous effects across the case rate distribution by the size of the bargaining unit and industry. Understanding the causal mechanisms for these heterogeneous effects is an important direction for future research.

Appendix

Data and Sample

The empirical analysis utilizes data from multiple sources. Data on union elections come from the National Labor Relations Board (NLRB) and the American Federation of Labor and the Congress of Industrial Organizations (AFL-CIO); data on union activity come from the Federal Mediation and Conciliation Service (FMCS); and data on occupational safety come from the Occupational Safety and Health Administration (OSHA), specifically the OSHA Data Initiative (ODI). These data are matched at the establishment level.

The NLRB reports the establishment name, address, and industry, as well as the number of eligible voters, valid votes cast, and votes for and against unionization. The NLRB data are first restricted to closed cases. To match the NLRB data to the other data, the establishment name and address were standardized. For the establishment name, all the special characters and common words, such as company, limited, and corporation, were deleted. If the listed formal name and the case name differed, or if the establishment is doing business as (DBA) under a different name, both names are retained and used for matching. For the street address, all special characters and numbers for floor, suite, and room were deleted. Common words, such as street, avenue, and road, were replaced with their respective abbreviations. To standardize and clean the city name, each name was best matched probabilistically to an exhaustive list of all city names in the US, compiled by the US Census Bureau. City names without a perfect match were checked manually for typos.

The NLRB were matched to the ODI and the FMCS. The ODI reports accident case rates, measured annually per 100 full-time equivalent workers. The FMCS data indicate whether an establishment filed a "notice of bargaining," an indicator of union activity. The establishment name and street address from the FMCS data and the ODI data were standardized using the same method of standardizing NLRB data.

The matching procedure utilized the establishment name, street address, city, state, and zip code. The NLRB was matched to the FMCS and ODI in several stages. In the first and most restrictive stage, the data were matched exactly on the establishment name, street, city, and state. If a record contained multiple establishment names, the matching procedure was repeated for each name until a successful match, if any. In the second stage, the data were matched exactly on the establishment name, zip code, city, and state. In the third stage, the data were matched exactly based on the first six letters of the establishment name and address within the same city and state. In the fourth and least restrictive stage, the data were matched exactly on the first six digits of the establishment name, the first six digits of the street, full city, and state. If an FMCS or ODI record successfully matched in one stage, the matched record was removed from matching in subsequent stages.

Support for Identification Strategy

Union Activity

The empirical strategy assumes that bargaining power of workers increases discontinuously at the 50-percent cutoff following the union election. To support this assumption, the election data from 1999 to 2010 are matched to "notices of bargaining" data in years 1997 to 2016 from the Federal Mediation and Conciliation Service (FMCS).[27] A notice is required to initiate, terminate, or modify a labor contract and is therefore an indicator of union activity. The NLRB elections are matched to FMCS records by establishment name and address.

Using the FMCS data on notices of bargaining, Appendix Figure 1 plots the FMCS match percent by calendar year before and after the union election, separately by the election outcome. Given the calendar years of the FMCS data, the sample is restricted to elections that occurred in 1999 and after, and the FMCS match rate only reflects periods for which data are available. Among establishments in which the unionization won, the match rate increases sharply in the calendar year of the election and the year after, then returns to its pre-existing trend. Among establishments in which unionization failed, the match rate remains relatively unchanged compared to the pre-existing trend. These results confirm that union activity increases following a successful election.

To examine union activity at the cutoff, Appendix Figure 2 plots the FMCS match rate in periods 0 or 1 across 20 non-overlapping bins of five percentage point each. Periods 0 and 1 had the greatest increase in union activity following a successful union election (Appendix Figure 1), and the number of bins equals the minimum votes in the sample so that establishments with the minimum votes can be represented in each bin (DiNardo and Lee, 2004). Given the calendar years of the FMCS data, the sample is restricted to elections that occurred from 1999 to 2009. The figure confirms that union activity increases

[27] DiNardo and Lee (2004) similarly match union election data to FMCS data.

discontinuously at the cutoff following a union election.

The first row of Appendix Table 1 presents discontinuity estimates in the FMCS match rate in periods 0 or 1 using equation (1). Each figure corresponds to a discontinuity estimate from a single model. The rows correspond to outcome variables, and the columns correspond to different bandwidths and polynomial orders, which range from 0.15 to 0.25 and from 1 to 2, respectively. The first row presents discontinuity estimates for the FMCS match rate. As shown, the match rate increases discontinuously by more than 30 percentage points, which is statistically significant at the one percent level and robust to the bandwidth and polynomial order.

The FMCS match rates raise two issues. First, the FMCS match rate is not 100 percent among establishments with a successful union election. One possible reason is that the match of the NLRB data to the FMCS data is imperfect, leading to false-negatives. Another possible reason is that a successful union election may not necessarily lead to a labor contract. Second, the FMCS match rate is not zero among establishments with a failed union election. Again, one possible reason is that the match of the NLRB data to the FMCS data is imperfect, although false-positives seem less likely than false-negatives. Another reason is that a single establishment may have multiple bargaining units, and the bargaining unit associated with the union election may be different from the bargaining unit associated with a FMCS match.

Density of the Vote Share

A threat to identification is non-random sorting at the cutoff, which occurs when the vote share is manipulated at the margin of victory to alter the election outcome. The problem for identification is that manipulation for or against a successful election may correlated with workplace safety so that the discontinuity in workplace safety at the cutoff may be biased relative to the true causal effect of unionization.

Non-random sorting due to vote share manipulation may lead to excess or missing

density at the cutoff, so a discontinuity in the density of the vote share may be evidence of non-random sorting (McCrary, 2008). To illustrate the vote share distribution, the top panel of Appendix Figure 3 plots the vote share density distribution across 20 non-overlapping bins of five percentage point each. The figure indeed suggests manipulation: the density generally increases from the right towards the cutoff, but decreases just above the cutoff, suggesting too few narrow election successes. This finding is consistent with an earlier study by Frandsen (2014), who also finds too few narrow election successes.[28]

A test for continuity at the cutoff is developed by McCrary (2008), which is based on the local linear density estimator developed by Cheng (1994) and Cheng et al. (1993). Using a uniform kernel, the test fails to reject continuity at the 5 percent level (p-value: 0.0696), though the test statistic is only marginally insignificant.

Manipulating the vote share may become increasingly more difficult as the number of votes increases. The second and third panels of Appendix Figure 3 illustrate vote share densities among elections with at least 50 and 100 votes, respectively. Although both figures indicate missing density just above the cutoff, the McCrary (2008) test fails to reject continuity. The failure to reject may be partially due to a smaller sample, which is reduced by approximately 50 percent and 75 percent when examining elections with at least 50 votes and 100 votes, respectively.

There are two potential strategies to address excess or missing density at the cutoff. The first is to focus on union elections with many votes cast, where vote share manipulation to affect the election outcome is not only more difficult, but more likely to be unexpected locally at the cutoff (Lee, 2008). As McCrary (2008) notes, excess or missing density does not necessarily invalidate identification if manipulation occurs at random. The second strategy is to evaluate non-random sorting directly with respect to observable characteristics and workplace safety pre-election.

[28]A key difference between between the studies is that Frandsen (2014) uses elections from 1980 to 2009, whereas this study uses elections from 1991 to 2010. This study also uses only establishments with union elections that match to the ODI, which are not representative of all establishments with union elections.

Establishment and Election Characteristics

Non-random sorting may also lead to discontinuities in the conditional mean functions of observable characteristics (Lee, 2008). To evaluate discontinuities in election characteristics graphically, the first row of Appendix Figure 4 plots the conditional mean of eligible employees and valid votes cast across 20 mutually exclusive bins.[29] As shown, the number of eligible employees and valid votes cast increases and then decreases with the vote share, but there is no apparent discontinuity in either measure at the cutoff.

The second and third rows of Appendix Table 1 present discontinuity estimates in eligible employees and valid votes using equation (1). As shown, the discontinuity estimates are generally small in magnitude relative to the mean near the cutoff, and all of the estimates are statistically insignificant. The largest estimates appear in the second column, with a bandwidth of 0.20 and a second-order polynomial. In that column, the discontinuity estimates are -5.83 and -3.07 for eligible employees and valid votes, respectively, relative to the mean near the cutoff of 116.98 and 102.90.[30]

To evaluate discontinuities in industry, the second row of Appendix Figure 4 plots the share of establishments in manufacturing and health services across 20 mutually exclusive bins. As shown, the share in manufacturing increases and then decreases with the vote share, whereas the share in health services generally increases, except for a sharp decrease at the highest bins. Importantly, neither industry share exhibits a discontinuity at the cutoff.

The fourth and fifth rows of Appendix Table 1 presents discontinuity estimates in the industry share using equation (1). In this case, all the estimates are small and statistically insignificant regardless of the bandwidth or polynomial order. These results suggest that there is no non-random sorting around the cutoff with respect to industry.

Finally, the last panel of Appendix Figure 4 plots the share of establishments with

[29]When examining discontinuities in the number of eligible employees and votes cast, one extreme outlier is omitted from the analysis. For this outlier, the number of eligible employees and votes case is 17,195 and 15,471, respectively. The next highest values are 7,000 and 4,589, respectively.

[30]The means near the cutoff are calculated among elections within a bandwidth of 10 percentage points from the cutoff.

any match to the ODI.

Appendix Table 1: Discontinuity Estimates in Union Activity and Establishment and Election Characteristics

Outcome Variable	Mean	(1)	(2)	(3)
FMCS	21.63	35.00***	32.47***	32.97***
	(0.70)	(2.54)	(3.40)	(3.01)
Employees eligible (number)	116.98	2.412	-5.834	-1.360
	(1.92)	(6.733)	(9.099)	(7.883)
Valid votes (number)	102.90	3.059	-3.070	1.535
	(1.64)	(5.626)	(7.633)	(6.600)
Manufacturing	32.39	-0.802	0.807	-0.682
	(0.54)	(1.779)	(2.375)	(2.100)
Health services	20.30	0.884	0.261	1.127
	(0.46)	(1.582)	(2.117)	(1.865)
ODI Match, Periods -3 to -1	18.07	-0.423	-0.870	0.947
	(0.56)	(1.888)	(2.523)	(2.226)
ODI Match, Periods 1 to 3	19.22	1.25	0.640	1.89
	(0.48)	(1.621)	(2.174)	(1.913)
Polynomial		1	2	2
Bandwidth		0.15	0.20	0.25
Observations		11,150	14,374	17,187

The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. Each column corresponds to discontinuity estimates for one dependent variable. The FMCS is an indicator for union activity. Due to the dates of the FMCS data, the sample for these regressions is restricted to elections in 1999 to 2009. For employees eligible and valid votes, one outlier is dropped from the sample. The mean is calculated with a bandwidth of 10 percentage points above and below the cutoff. Standard errors are in parentheses and clustered by election. Estimates are in percentage points unless otherwise noted. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

Appendix Table 2: Discontinuities in DART, Periods 1 to 3, 20≤Votes<70

	Mean	(1)	(2)	(3)	(4)	(5)	(6)
DART (rate)	7.93	0.0247	0.0256	-0.563	-0.637	-0.106	0.102
	(0.22)	(0.876)	(1.031)	(1.124)	(1.291)	(1.020)	(1.182)
DART=0	8.35	2.971	2.335	3.449	2.423	3.271	3.761
	(0.83)	(3.344)	(3.582)	(4.819)	(5.088)	(4.159)	(4.320)
DART<1	10.32	1.024	0.569	1.038	-1.828	1.702	1.581
	(0.91)	(3.678)	(4.135)	(5.276)	(5.821)	(4.535)	(4.937)
DART<2	16.70	1.404	0.547	0.768	-1.929	2.228	0.926
	(1.12)	(4.423)	(4.812)	(6.263)	(6.605)	(5.396)	(5.734)
DART<3	25.31	0.905	0.438	3.210	1.943	4.072	1.691
	(1.30)	(5.331)	(5.710)	(7.450)	(7.906)	(6.428)	(6.728)
DART<4	32.32	-0.750	-1.555	1.066	-1.195	0.879	-0.970
	(1.40)	(5.816)	(6.265)	(8.124)	(8.500)	(7.033)	(7.413)
DART<5	38.96	-0.499	0.482	-0.271	-0.342	0.480	-0.195
	(1.46)	(5.988)	(6.643)	(8.359)	(8.936)	(7.249)	(7.790)
DART<6	47.04	-8.830	-8.783	-7.900	-10.60	-7.620	-8.524
	(1.50)	(5.979)	(6.624)	(8.299)	(8.746)	(7.213)	(7.645)
DART<7	55.57	-1.793	-1.432	1.243	1.736	0.405	1.166
	(1.49)	(5.951)	(6.671)	(8.135)	(8.652)	(7.129)	(7.658)
DART<8	61.49	-1.085	-0.399	-0.898	-0.116	1.913	2.459
	(1.46)	(5.766)	(6.604)	(7.740)	(8.436)	(6.855)	(7.550)
DART<9	66.34	-0.127	-0.952	2.318	1.633	2.698	2.339
	(1.42)	(5.721)	(6.719)	(7.608)	(8.417)	(6.766)	(7.568)
DART<10	70.47	-1.627	-1.836	2.160	2.484	0.308	0.335
	(1.37)	(5.563)	(6.801)	(7.420)	(8.431)	(6.576)	(7.591)
Covariates		No	Yes	No	Yes	No	Yes
Polynomial		1	1	2	2	2	2
Bandwidth		0.15	0.15	0.20	0.20	0.25	0.25
Observations		1,586	1,586	1,990	1,990	2,385	2,385

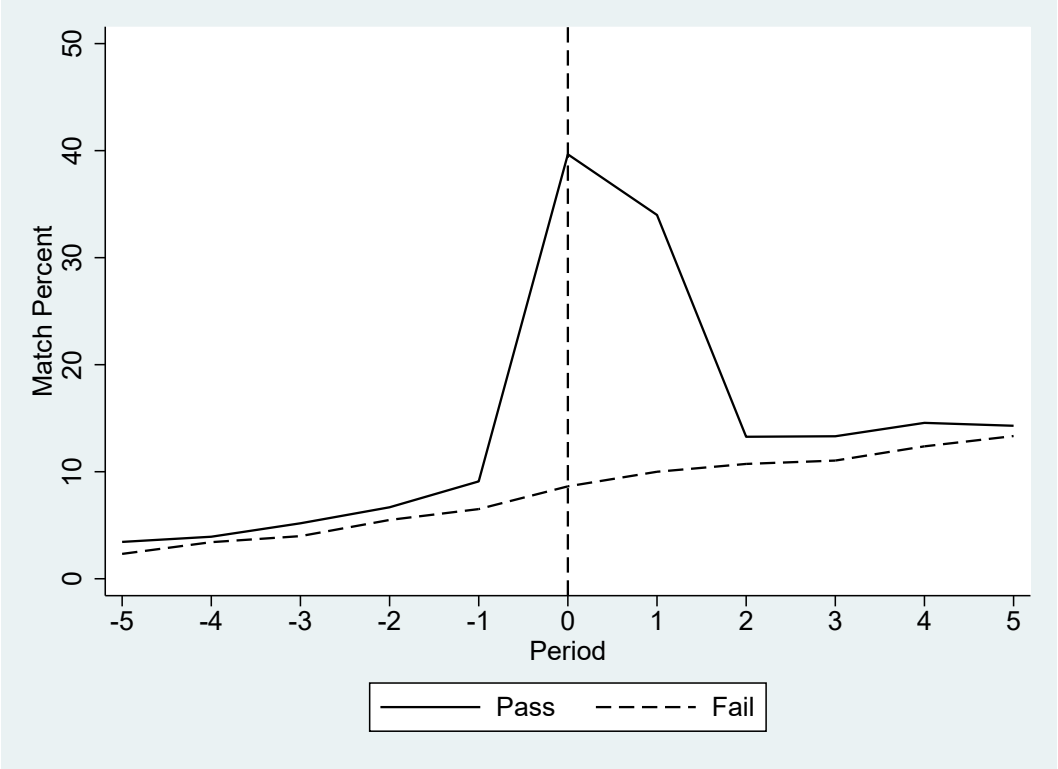
The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. To examine effects among smaller bargaining units, the sample is further restricted to elections with valid votes greater than or equal to 20 and less than 70. Observations are establishment-by-ODI match. The mean is calculated with a bandwidth of 10 percentage points above and below the cutoff. Standard errors are in parentheses and clustered by election. Estimates are in percentage points unless otherwise noted. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

Appendix Table 3: Discontinuities in DART, Periods 1 to 3, Not Manufacturing

	Mean	(1)	(2)	(3)	(4)	(5)	(6)
DART (rate)	8.18	0.735	0.858	1.552	1.438	0.694	0.642
	(0.22)	(0.935)	(1.037)	(1.224)	(1.299)	(1.126)	(1.188)
DART=0	5.35	2.084	3.346	3.242	3.446	2.569	2.886
	(0.69)	(3.156)	(3.341)	(4.359)	(4.541)	(3.901)	(3.972)
DART<1	7.41	0.640	3.370	1.187	3.106	1.649	2.592
	(0.80)	(3.513)	(3.881)	(4.868)	(5.337)	(4.332)	(4.568)
DART<2	12.85	1.406	6.241	0.611	5.863	1.201	4.338
	(1.03)	(4.424)	(4.948)	(6.115)	(6.751)	(5.410)	(5.718)
DART<3	20.73	1.247	6.583	2.143	7.581	1.231	3.808
	(1.24)	(5.101)	(5.918)	(6.969)	(7.914)	(6.156)	(6.707)
DART<4	27.67	-2.011	2.630	-2.447	2.173	-3.854	-1.573
	(1.37)	(5.630)	(6.740)	(7.602)	(9.016)	(6.753)	(7.604)
DART<5	35.27	-4.254	-1.097	-8.622	-4.412	-6.237	-3.860
	(1.46)	(6.037)	(7.065)	(8.133)	(9.426)	(7.232)	(7.897)
DART<6	43.71	-9.702	-9.307	-13.09	-13.01	-10.84	-12.11
	(1.52)	(6.259)	(7.041)	(8.390)	(9.158)	(7.479)	(7.791)
DART<7	51.59	-2.203	-4.102	-5.348	-5.954	-2.983	-5.443
	(1.53)	(6.278)	(6.902)	(8.339)	(8.887)	(7.461)	(7.639)
DART<8	57.04	-4.588	-6.342	-8.531	-8.910	-4.659	-6.225
	(1.52)	(6.188)	(6.986)	(8.082)	(8.725)	(7.304)	(7.666)
DART<9	62.01	-3.937	-6.989	-6.783	-8.301	-4.105	-6.223
	(1.49)	(6.114)	(7.018)	(7.928)	(8.698)	(7.197)	(7.615)
DART<10	67.92	-5.175	-7.240	-8.620	-10.12	-6.449	-7.912
	(1.43)	(5.906)	(7.371)	(7.656)	(9.091)	(6.942)	(7.808)
Covariates		No	Yes	No	Yes	No	Yes
Polynomial		1	1	2	2	2	2
Bandwidth		0.15	0.15	0.20	0.20	0.25	0.25
Observations		1,539	1,539	1,923	1,923	2,274	2,274

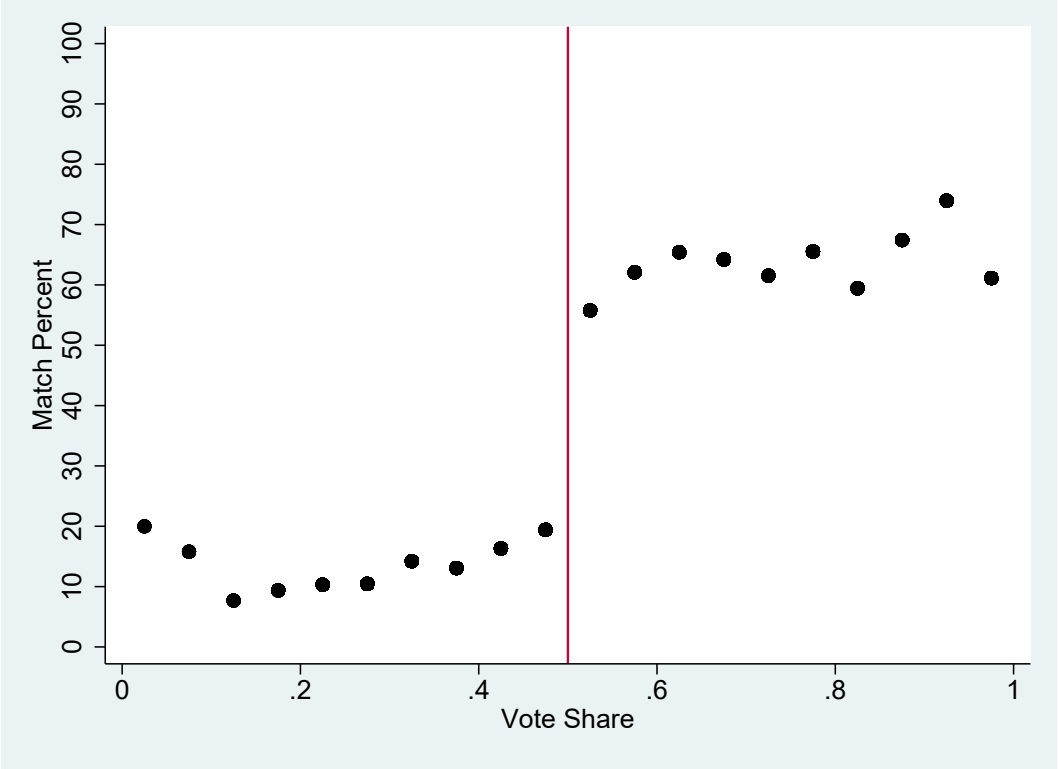
The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. To examine effects by industry, the sample is further restricted to establishments not in manufacturing. Observations are establishment-by-ODI match. The mean is calculated with a bandwidth of 10 percentage points above and below the cutoff. Standard errors are in parentheses and clustered by election. Estimates are in percentage points unless otherwise noted. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

Appendix Figure 1: FMCS Match by Period



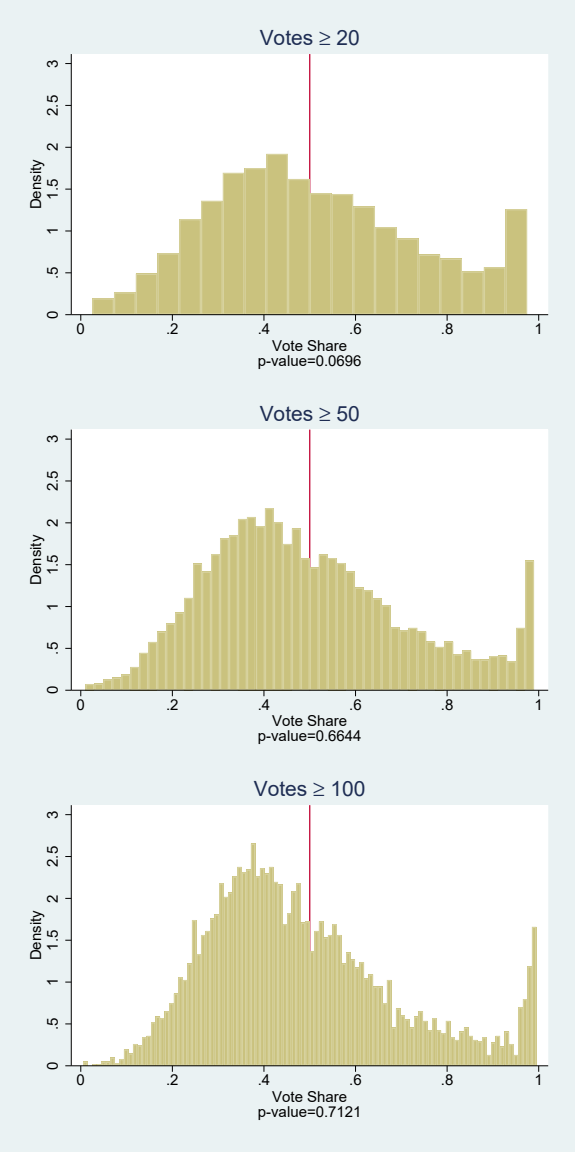
The sample is derived from union elections contained in the NLRB, file years 1999 to 2009, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. The periods correspond to calendar years relative to the calendar year of the election. The match rates are calculated using only calendar years for which ODI data are available.

Appendix Figure 2: FMCS Match by Vote Share



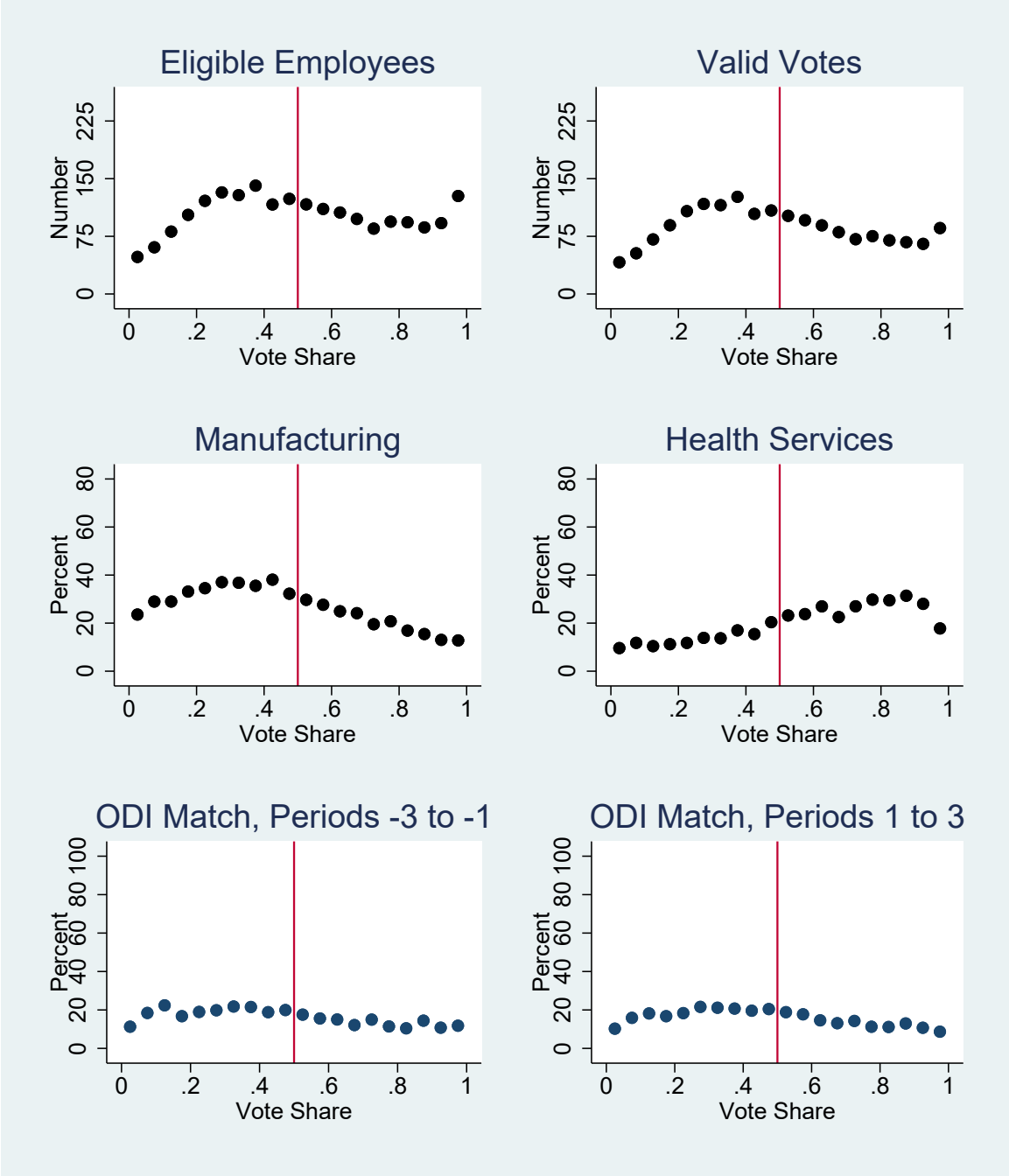
The figure illustrates the match rate of union elections contained in the NRLB to notices of bargining filed with the FMCS in periods zero or one across bins of five percentage points. The sample is derived from union elections contained in the NLRB, file years 1999 to 2009. The sample is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization.

Appendix Figure 3: Distribution of Vote Share



The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. In each panel, the number of bins is equal to the minimum number of votes cast. To impose symmetry in the vote share distribution regardless of the number of vote cast, an amount equal to 0.5 divided by the number of votes cast is subtracted from the vote share if the number of votes cast is even (DiNardo and Lee, 2004). The p-value comes from the McCrary (2008) test for continuity at the cutoff.

Appendix Figure 4: Discontinuities in Eligible Employees, Valid Votes Cast, Industry, and ODI Match



The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization.

References

Boal, W. (2009). The effect of unionism on accidents in u.s. coal mining, 1897-1929. *Industrial Relations*, 48(1):97–120.

Brown, M. and Heywood, J. (2006). Investigating the cause of death: Industrial relations and plant closures in australia. *ILR Review*, 59(4):593–612.

Cheng, M.-Y. (1994). On boundary effects of smooth curve estimators. Unpublished paper.

Cheng, M.-Y., Fan, J., and Marron, J. (1993). Minimax efficiency of local polynomial fit estimators at boundaries. Unpublished paper.

Department of Labor, U. S. (2018). National census of fatal occupational injuries in 2017. *News Release, USDL-18-1978*.

DiNardo, J. and Lee, D. (2004). Economic impacts of new unionization on private sector employers: 1984-2001. *Quarterly Journal of Economics*, 119(4):1383–1442.

Donado, A. (2015). Why do unionized workers have more nonfatal occupational injuries? *ILR Review*, 68(1):153–183.

Farber, H. (1986). The analysis of union behavior. In Ashenfelter, O. and Layard, R., editors, *Handbook of Labor Economics*, volume 2, pages 1039–1089. Elsevier.

Frandsen, B. (2012). Why unions still matter: The effects of unionization on the distribution of earnings. Unpublished paper.

Frandsen, B. (2014). The surprising impacts of unionization: Evidence from matched employer-employee data. Unpublished paper.

Frandsen, B., Frolich, M., and Melly, B. (2012). Quantile treatment effects in the regression discontinuity design. *Journal of Econometrics*, 168(2):382–395.

Freeman, R. and Kleiner, M. (1999). Do unions make enterprises insolvent? *ILR Review*, 52(4):510–527.

Freeman, R. and Medoff, J. (1984). *What Do Unions Do?* Basic Books, New York, NY.

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

Johnson, M., Levin, D., and Toffel, M. (2017). Improving regulatory effectiveness through better targeting: Evidence from osha. Unpublished paper.

Lee, D. (2008). Randomized experiments from non-random selection in u.s. house races. *Journal of Econometrics*, 142(2):675–697.

Lee, D. and Mas, A. (2012). Long-run impacts of unions on firms: New evidence from financial markets, 1961-1999. *Quarterly Journal of Economics*, 127(1):333–378.

Li, L. and Singleton, P. (2019). The effect of workplace inspection on worker safety. *ILR Review*, 72(3):718–748.

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

Morantz, A. (2009). The elusive union safety effect: Toward a new empirical research agenda. In Eaton, A., editor, *Labor and Employment Relations Association, 61st Proceedings*, pages 130–146.

Morantz, A. (2013). Coal mine safety: Do unions make a difference? *ILR Review*, 66:88–116.

Olea, J. L. M. and Plagborg-Moller, M. (2019). Simultaneous confidence bands: Theory, implementation, and an application to SVARs. *Journal of Applied Econometrics*, 34(1):1–17.

Schurman, S., Weil, D., Landsbergis, P., and Israel, B. (1998). The role of unions and collective bargaining in preventing work-related disability. In Thomason, T., Jr., J. B.,

and Hyatt, D., editors, *New Approaches to Disability in the Workplace*, pages 121–54. Industrial Relations Research Association.

Sojourner, A., Town, R., Grabowski, D., Chen, M., and Frandsen, B. (2015). Impacts of unionization on quality and productivity: Regression discontinuity evidence from nursing homes. *ILR Review*, 64(4):771–806.

Sojourner, A. and Yang, J. (2015). Effects of unionization on workplace-safety enforcement: Regression-discontinuity evidence. *IZA Discussion Paper 9610*.

Viscusi, K. (1979). *Employment Hazard: An Investigation of Market Performance*. Harvard University Press, Cambridge, MA.

Weil, D. (1991). Enforcing osha: the role of labor unions. *Industrial Relations*, 30(1):20–36.

Table 1: Summary Statistics of Union Elections by Match to ODI

ODI Match	All	Yes	No
Employees eligible	114.0	139.0	104.1
	(1.381)	(2.696)	(1.598)
Valid votes	97.27	122.7	87.13
	(1.139)	(2.252)	(1.309)
Pass	46.77	40.25	49.37
	(0.319)	(0.587)	(0.378)
Manufacturing	28.94	47.91	21.38
	(0.290)	(0.598)	(0.310)
Transportation	16.86	9.389	19.84
	(0.239)	(0.349)	(0.301)
Health services	19.63	21.44	18.91
	(0.254)	(0.491)	(0.296)
Other	34.57	21.26	39.87
	(0.304)	(0.490)	(0.370)
Northeast	27.08	25.73	27.61
	(0.284)	(0.523)	(0.338)
Midwest	29.77	37.28	26.78
	(0.292)	(0.579)	(0.335)
South	22.41	20.83	23.03
	(0.266)	(0.486)	(0.318)
West	20.74	16.16	22.57
	(0.259)	(0.441)	(0.316)
Elections	24,758	6,976	17,782

The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. The second and third columns present summary statistics separately by whether the union election is matched to any observations in the OSHA Data Initiative (ODI), file years 1996 to 2011. Standard errors are in parentheses.

Table 2: Discontinuities in DART, Periods -3 to -1

	Mean	(1)	(2)	(3)	(4)	(5)	(6)
DART (rate)	7.95	0.303	0.504	0.615	1.157	0.605	0.872
	(0.17)	(0.769)	(0.831)	(1.013)	(1.070)	(0.892)	(0.904)
DART=0	5.15	-2.848	-2.695	-4.319	-5.958*	-2.944	-2.446
	(0.58)	(2.301)	(2.623)	(3.214)	(3.556)	(2.850)	(3.113)
DART<1	8.46	-1.176	-1.715	-1.779	-5.071	-0.686	-0.899
	(0.72)	(2.727)	(3.107)	(3.676)	(4.083)	(3.271)	(3.592)
DART<2	14.15	-0.255	-1.498	0.0620	-3.967	-1.171	-1.531
	(0.91)	(3.573)	(4.122)	(4.739)	(5.158)	(4.225)	(4.576)
DART<3	23.63	-0.605	-1.235	-0.680	-3.745	-2.567	-1.885
	(1.11)	(4.436)	(4.971)	(5.854)	(6.227)	(5.239)	(5.519)
DART<4	32.36	-4.483	-3.812	-5.986	-6.880	-7.447	-6.442
	(1.22)	(4.808)	(5.306)	(6.334)	(6.752)	(5.656)	(5.896)
DART<5	39.68	-3.377	-3.478	-6.292	-7.514	-6.650	-6.988
	(1.27)	(5.126)	(5.588)	(6.761)	(7.126)	(6.037)	(6.234)
DART<6	47.39	-4.716	-3.683	-10.08	-11.31	-9.817	-10.57
	(1.30)	(5.312)	(5.835)	(6.992)	(7.426)	(6.224)	(6.502)
DART<7	53.49	-2.538	-1.657	-5.402	-8.179	-5.775	-7.639
	(1.30)	(5.398)	(5.850)	(7.129)	(7.551)	(6.347)	(6.586)
DART<8	60.12	-5.002	-3.856	-9.231	-10.67	-7.822	-8.334
	(1.27)	(5.432)	(5.804)	(7.172)	(7.495)	(6.398)	(6.626)
DART<9	65.54	-1.935	-1.834	-5.042	-7.106	-4.300	-5.478
	(1.24)	(5.253)	(5.593)	(6.937)	(7.219)	(6.168)	(6.286)
DART<10	70.01	-1.794	-3.093	-4.121	-6.721	-2.985	-5.361
	(1.19)	(5.055)	(5.328)	(6.719)	(6.932)	(5.949)	(6.029)
Covariates		No	Yes	No	Yes	No	Yes
Polynomial		1	1	2	2	2	2
Bandwidth		0.15	0.15	0.20	0.20	0.25	0.25
Observations		2,166	2,166	2,786	2,786	3,307	3,307

The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. Observations are establishment-by-ODI match. The mean is calculated with a bandwidth of 10 percentage points above and below the cutoff. Standard errors are in parentheses and clustered by election. Estimates are in percentage points unless otherwise noted. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

Table 3: Discontinuities in DART, Periods 1 to 3

	Mean	(1)	(2)	(3)	(4)	(5)	(6)
DART (rate)	8.03	0.130	-0.344	0.116	-0.403	-0.201	-0.515
	(0.15)	(0.661)	(0.690)	(0.869)	(0.891)	(0.783)	(0.794)
DART=0	6.17	3.421	3.223	4.809*	4.188	4.346*	4.237*
	(0.50)	(2.104)	(2.071)	(2.883)	(2.821)	(2.565)	(2.489)
DART<1	8.99	4.846*	5.247**	7.068**	6.791*	6.399**	6.862**
	(0.60)	(2.497)	(2.606)	(3.444)	(3.527)	(3.035)	(3.100)
DART<2	15.72	5.406*	6.597**	7.839*	9.684**	7.004*	8.462**
	(0.76)	(3.148)	(3.298)	(4.322)	(4.385)	(3.800)	(3.884)
DART<3	24.36	3.830	5.845	6.621	8.653*	5.981	7.371
	(0.89)	(3.730)	(3.905)	(5.041)	(5.179)	(4.451)	(4.563)
DART<4	31.87	0.512	1.911	2.289	2.941	1.619	2.327
	(0.97)	(4.136)	(4.365)	(5.595)	(5.795)	(4.948)	(5.082)
DART<5	39.77	-0.749	1.218	-1.704	0.875	0.110	1.420
	(1.02)	(4.324)	(4.535)	(5.853)	(5.992)	(5.172)	(5.217)
DART<6	47.46	-4.610	-3.552	-5.797	-4.619	-3.674	-3.143
	(1.04)	(4.431)	(4.616)	(6.008)	(6.036)	(5.303)	(5.281)
DART<7	55.02	2.615	3.884	2.924	4.920	3.529	4.104
	(1.04)	(4.365)	(4.542)	(5.862)	(5.867)	(5.198)	(5.181)
DART<8	60.57	0.265	1.551	-1.614	0.429	0.824	1.228
	(1.02)	(4.249)	(4.521)	(5.645)	(5.765)	(5.034)	(5.118)
DART<9	66.13	0.144	0.317	-1.045	-0.462	1.138	0.899
	(0.99)	(4.157)	(4.441)	(5.494)	(5.583)	(4.915)	(4.975)
DART<10	70.99	-0.448	0.939	-1.591	0.00957	-0.0126	0.277
	(0.95)	(4.048)	(4.450)	(5.333)	(5.582)	(4.773)	(4.947)
Covariates		No	Yes	No	Yes	No	Yes
Polynomial		1	1	2	2	2	2
Bandwidth		0.15	0.15	0.20	0.20	0.25	0.25
sup-t 90		2.45	2.46	2.36	2.39	2.36	2.41
sup-t 95		2.72	2.59	2.65	2.72	2.65	2.70
Observations		3,328	3,328	4,231	4,231	5,025	5,025

The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. Observations are establishment-by-ODI match. The mean is calculated with a bandwidth of 10 percentage points above and below the cutoff. Standard errors are in parentheses and clustered by election. Estimates are in percentage points unless otherwise noted. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

Table 4: Summary Statistics of Establishments by DART Post-Election

DART	DART<2	DART≥2	Difference
Employees eligible	127.6	148.9	-21.3***
	(5.72)	(2.81)	(6.37)
Valid votes	113.2	133.3	-20.1***
	(4.83)	(2.52)	(5.44)
Pass	35.79	39.94	-4.15***
	(1.49)	(0.66)	(1.63)
Manufacturing	61.88	50.56	11.32***
	(1.51)	(0.67)	(1.65)
Transportation	7.08	8.26	-1.18
	(0.80)	(0.37)	(0.88)
Health services	10.77	22.71	-11.94***
	(0.97)	(0.56)	(1.12)
Other	20.27	18.47	1.80
	(1.25)	(0.52)	(1.36)
Northeast	26.38	26.16	0.22
	(1.37)	(0.59)	(1.49)
Midwest	35.11	39.81	-4.70***
	(1.49)	(0.66)	(1.63)
South	24.15	20.63	3.52***
	(1.33)	(0.54)	(1.44)
West	14.35	13.4	0.95
	(1.09)	(0.46)	(1.18)
Observations	1,031	5,566	

The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. Observations are establishment-by-ODI match. The mean is calculated with a bandwidth of 10 percentage points above and below the cutoff. Standard errors are in parentheses and clustered by election. Estimates are in percents unless otherwise noted. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

Table 5: Discontinuities in DART, Periods 1 to 3, Votes ≥70

	Mean	(1)	(2)	(3)	(4)	(5)	(6)
DART (rate)	8.12	0.422	-0.103	1.008	0.0462	-0.0783	-0.540
	(0.21)	(1.008)	(1.074)	(1.343)	(1.367)	(1.211)	(1.194)
DART=0	4.12	3.465	4.788	5.289	6.591*	4.824	6.337*
	(0.58)	(2.626)	(3.102)	(3.506)	(3.989)	(3.159)	(3.499)
DART<1	7.74	8.445**	11.06***	11.75**	14.53***	10.42**	14.01***
	(0.78)	(3.504)	(4.180)	(4.774)	(5.357)	(4.247)	(4.672)
DART<2	14.80	9.333**	12.39**	13.60**	17.73**	11.27**	15.71**
	(1.03)	(4.592)	(5.336)	(6.215)	(6.946)	(5.515)	(6.102)
DART<3	23.47	6.689	11.56*	9.171	14.74*	7.701	11.78*
	(1.23)	(5.313)	(6.157)	(7.051)	(7.926)	(6.313)	(7.020)
DART<4	31.46	1.704	4.062	2.701	4.777	2.233	4.182
	(1.35)	(5.913)	(6.750)	(7.837)	(8.704)	(7.038)	(7.719)
DART<5	40.54	-0.886	-0.672	-3.844	-0.704	-0.353	-0.233
	(1.42)	(6.272)	(6.832)	(8.304)	(8.712)	(7.430)	(7.732)
DART<6	47.86	-1.095	-3.015	-5.588	-3.602	-1.026	-1.652
	(1.45)	(6.592)	(7.197)	(8.787)	(9.078)	(7.867)	(8.054)
DART<7	54.50	6.147	2.934	2.829	4.504	5.455	3.366
	(1.44)	(6.426)	(6.798)	(8.510)	(8.579)	(7.638)	(7.610)
DART<8	59.71	0.642	-2.339	-4.255	-2.956	-1.513	-3.751
	(1.42)	(6.284)	(6.805)	(8.238)	(8.562)	(7.424)	(7.564)
DART<9	65.94	-0.345	-4.329	-5.330	-5.129	-1.472	-3.391
	(1.37)	(6.106)	(6.503)	(7.974)	(8.042)	(7.208)	(7.222)
DART<10	71.49	0.252	-1.980	-5.777	-3.680	-1.200	-1.954
	(1.31)	(5.951)	(6.495)	(7.708)	(8.118)	(6.991)	(7.220)
Covariates		No	Yes	No	Yes	No	Yes
Polynomial		1	1	2	2	2	2
Bandwidth		0.15	0.15	0.20	0.20	0.25	0.25
sup-t 90		2.42	2.38	2.40	2.35	2.41	2.40
sup-t 95		2.72	2.61	2.68	2.65	2.68	2.70
Observations		1,742	1,742	2,241	2,241	2,640	2,640

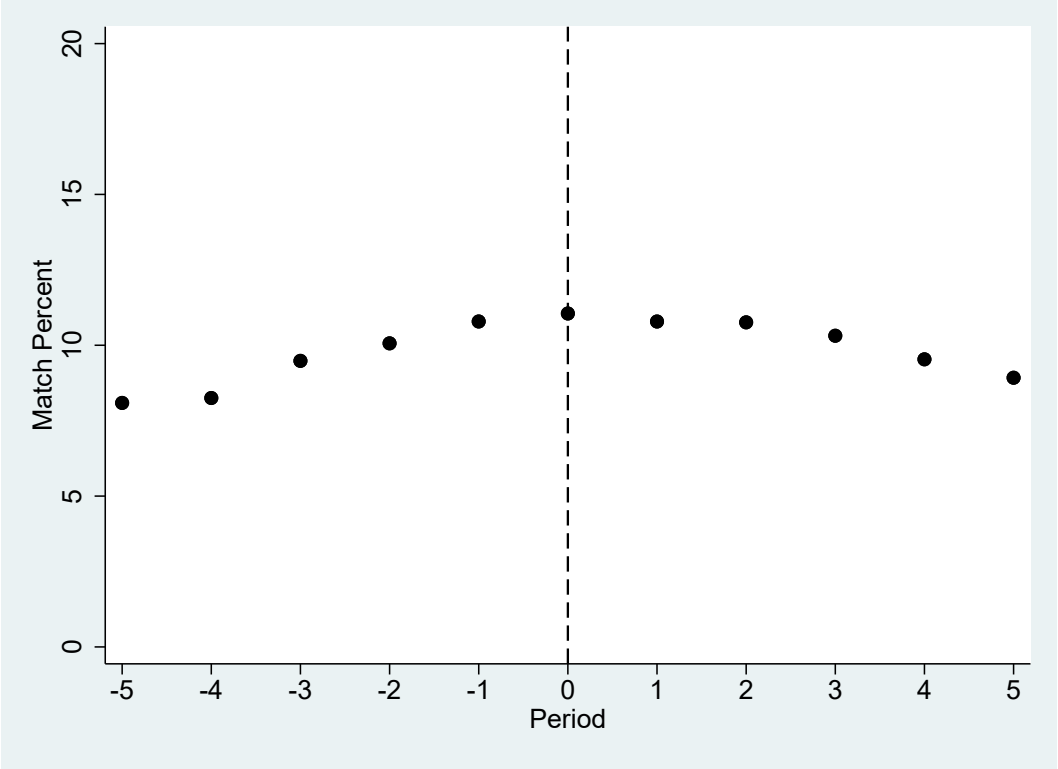
The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. To examine effects among larger bargaining units, the sample is further restricted to elections with valid votes greater than or equal to 70. Observations are establishment-by-ODI match. The mean is calculated with a bandwidth of 10 percentage points above and below the cutoff. Standard errors are in parentheses and clustered by election. Estimates are in percentage points unless otherwise noted. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

Table 6: Discontinuities in DART, Periods 1 to 3, Manufacturing

	Mean	(1)	(2)	(3)	(4)	(5)	(6)
DART (rate)	7.63	-0.639	-0.982	-1.564	-2.194*	-1.450	-1.812
	(0.21)	(0.925)	(1.031)	(1.194)	(1.286)	(1.065)	(1.135)
DART=0	6.87	4.943*	6.103**	6.465*	7.577*	6.579*	7.331**
	(0.72)	(2.848)	(3.042)	(3.809)	(4.055)	(3.417)	(3.612)
DART<1	10.35	9.345**	12.04***	13.26***	14.42***	11.80***	14.56***
	(0.87)	(3.646)	(4.019)	(5.008)	(5.252)	(4.397)	(4.623)
DART<2	18.19	9.669**	9.607*	15.16**	14.96**	13.18**	15.54***
	(1.10)	(4.547)	(5.125)	(6.202)	(6.624)	(5.467)	(5.922)
DART<3	27.49	7.674	8.451	12.16*	13.20*	12.06*	14.66**
	(1.27)	(5.432)	(6.069)	(7.328)	(7.954)	(6.472)	(6.975)
DART<4	35.49	4.837	2.077	8.904	5.824	9.316	6.907
	(1.36)	(6.054)	(6.549)	(8.260)	(8.784)	(7.266)	(7.633)
DART<5	43.65	4.561	2.249	7.159	6.507	8.545	5.738
	(1.41)	(6.187)	(6.777)	(8.478)	(9.146)	(7.421)	(7.888)
DART<6	50.69	1.834	0.0891	3.259	3.200	5.570	3.575
	(1.42)	(6.282)	(6.964)	(8.646)	(9.347)	(7.544)	(8.043)
DART<7	57.96	8.849	7.170	12.99	14.55	12.20*	10.68
	(1.40)	(6.006)	(6.825)	(8.125)	(8.934)	(7.141)	(7.721)
DART<8	63.62	6.643	4.817	6.958	8.837	8.460	6.375
	(1.37)	(5.817)	(6.715)	(7.843)	(8.753)	(6.905)	(7.610)
DART<9	69.68	6.419	3.065	7.226	6.847	9.563	6.022
	(1.31)	(5.567)	(6.398)	(7.478)	(8.269)	(6.587)	(7.212)
DART<10	73.65	5.754	4.267	7.199	9.270	8.645	6.867
	(1.25)	(5.520)	(6.375)	(7.370)	(8.199)	(6.506)	(7.135)
Covariates		No	Yes	No	Yes	No	Yes
Polynomial		1	1	2	2	2	2
Bandwidth		0.15	0.15	0.20	0.20	0.25	0.25
sup-t 90		2.39	2.17	2.39	2.42	2.41	2.40
sup-t 95		2.66	2.61	2.72	2.64	2.68	2.66
Observations		1,789	1,789	2,308	2,308	2,751	2,751

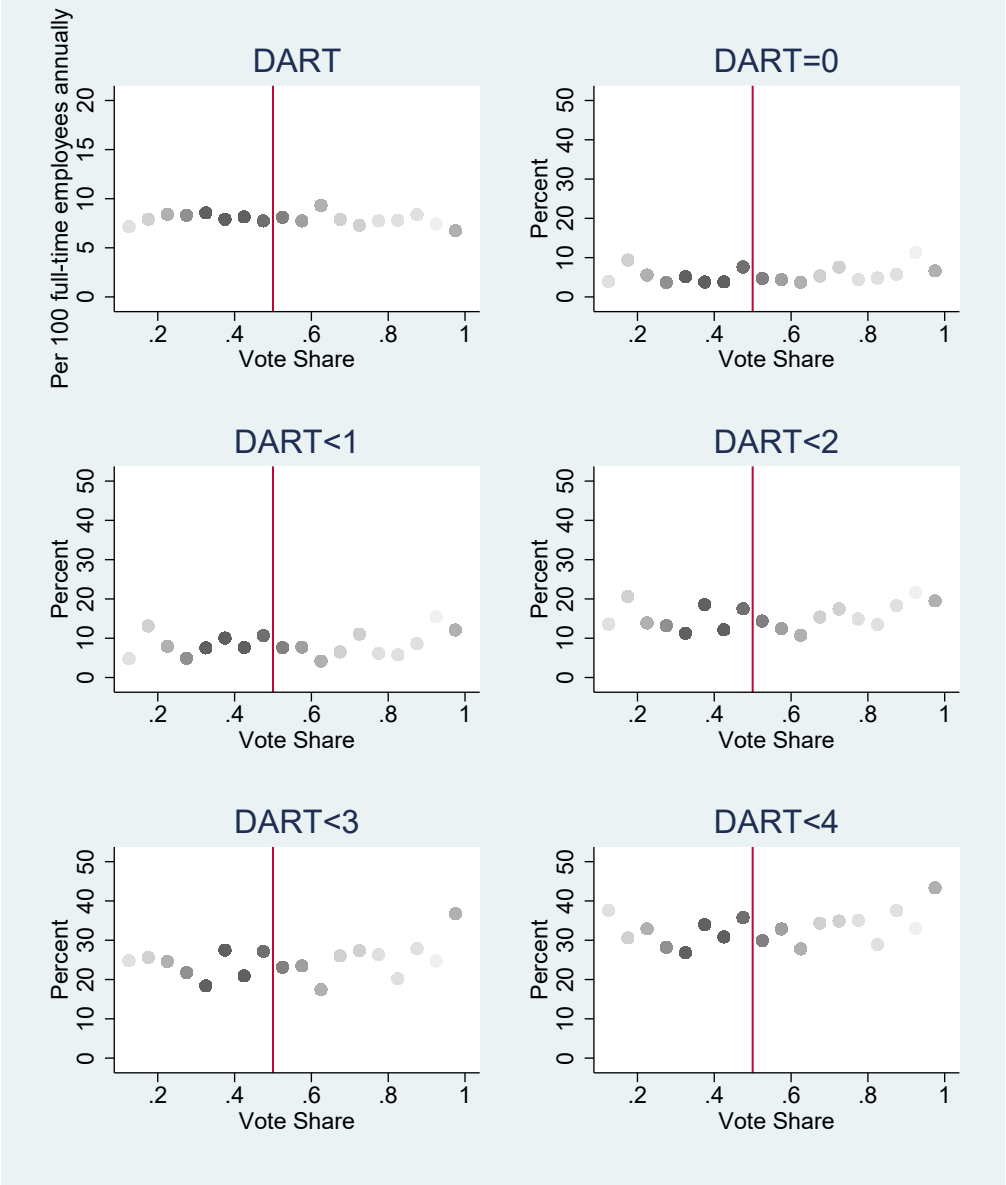
The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. To examine effects by industry, the sample is further restricted to establishments in manufacturing. Observations are establishment-by-ODI match. The mean is calculated with a bandwidth of 10 percentage points above and below the cutoff. Standard errors are in parentheses and clustered by election. Estimates are in percentage points unless otherwise noted. ***, **, and * indicate significance at the one, five, and ten percent levels, respectively.

Figure 1: ODI Match Rate by Period



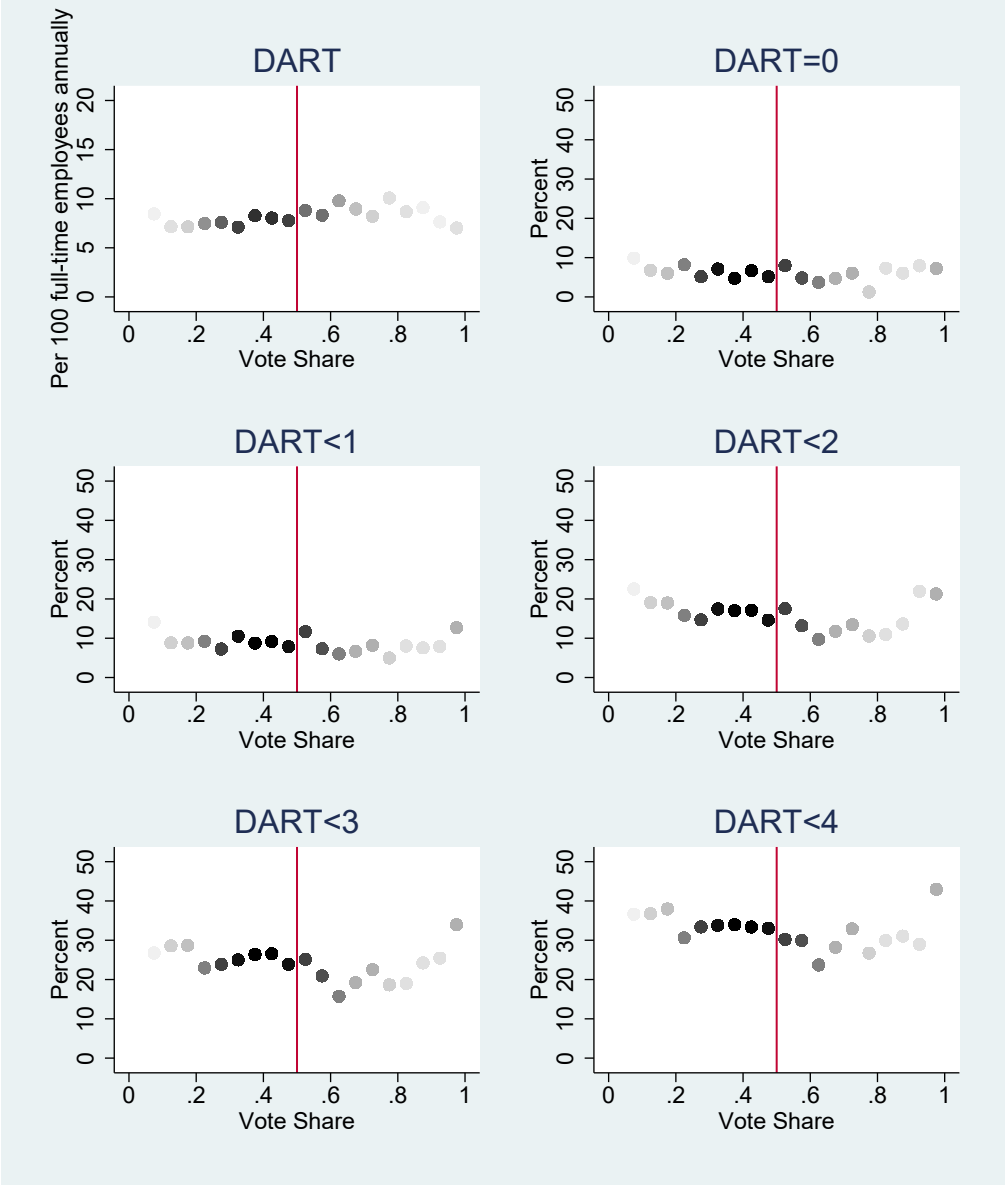
The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. The periods correspond to calendar years relative to the calendar year of the election. The match rates are calculated using only calendar years for which ODI data are available.

Figure 2: Discontinuities in DART, Periods -3 to -1



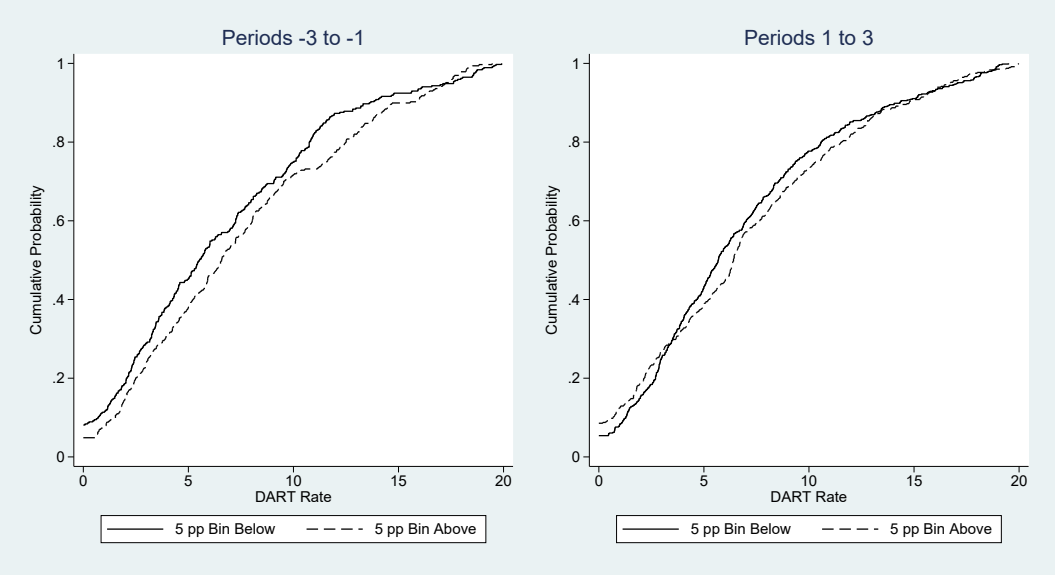
The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. The markers are shaded to indicate the relative number of observations within each bin. If the number of bin observations exceeds 660, the marker is shaded grey scale 0, which is black. For every 40 fewer observations, the marker is shaded one additional point on the grey scale. If the number of observations is between 620 and 660, the marker is shaded grey scale 1.

Figure 3: Discontinuities in DART, Periods 1 to 3



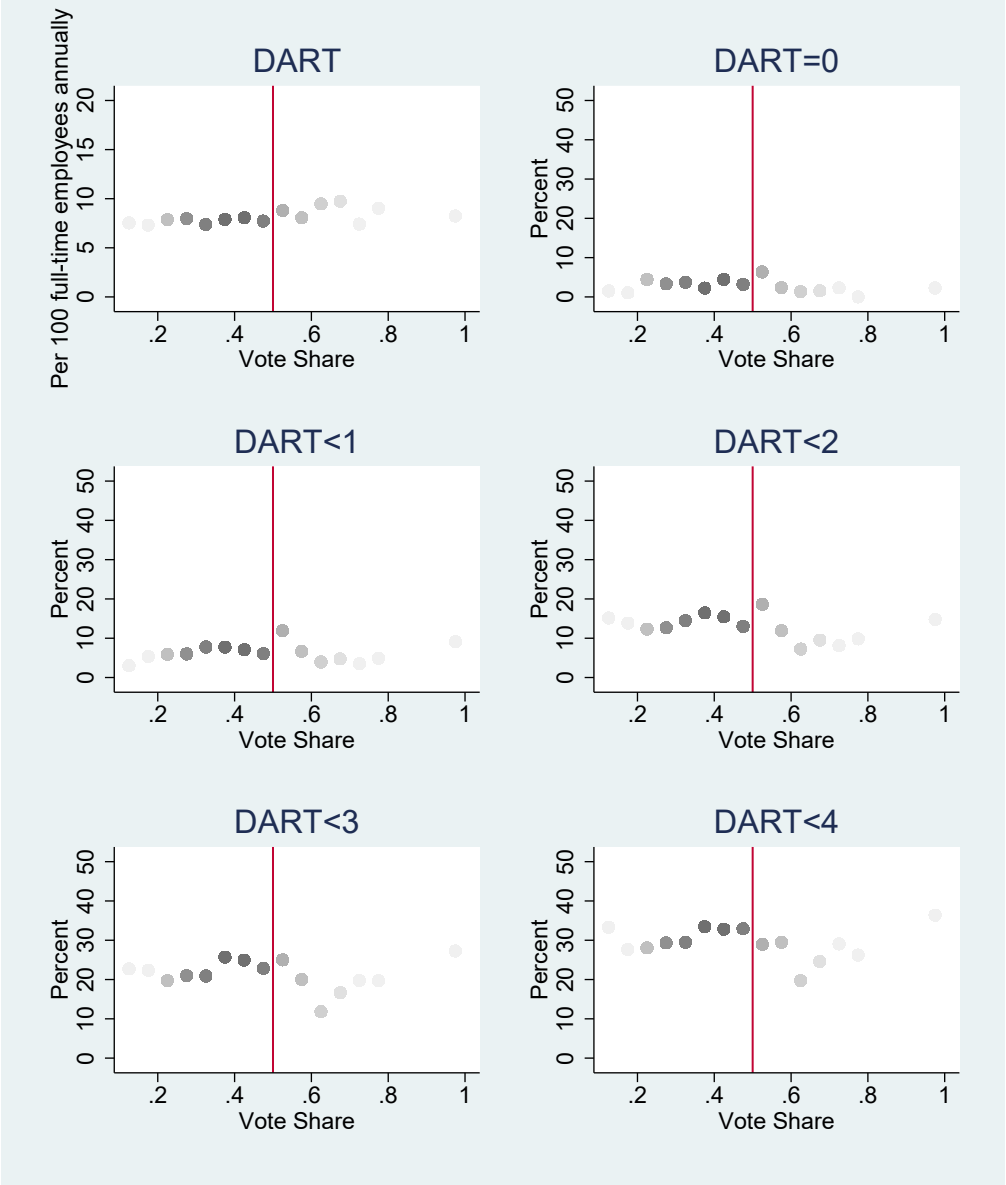
The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. The markers are shaded to indicate the relative number of observations within each bin. If the number of bin observations exceeds 660, the marker is shaded grey scale 0, which is black. For every 40 fewer observations, the marker is shaded one additional point on the grey scale. If the number of observations is between 620 and 660, the marker is shaded grey scale 1.

Figure 4: Cumulative Density Function of DART



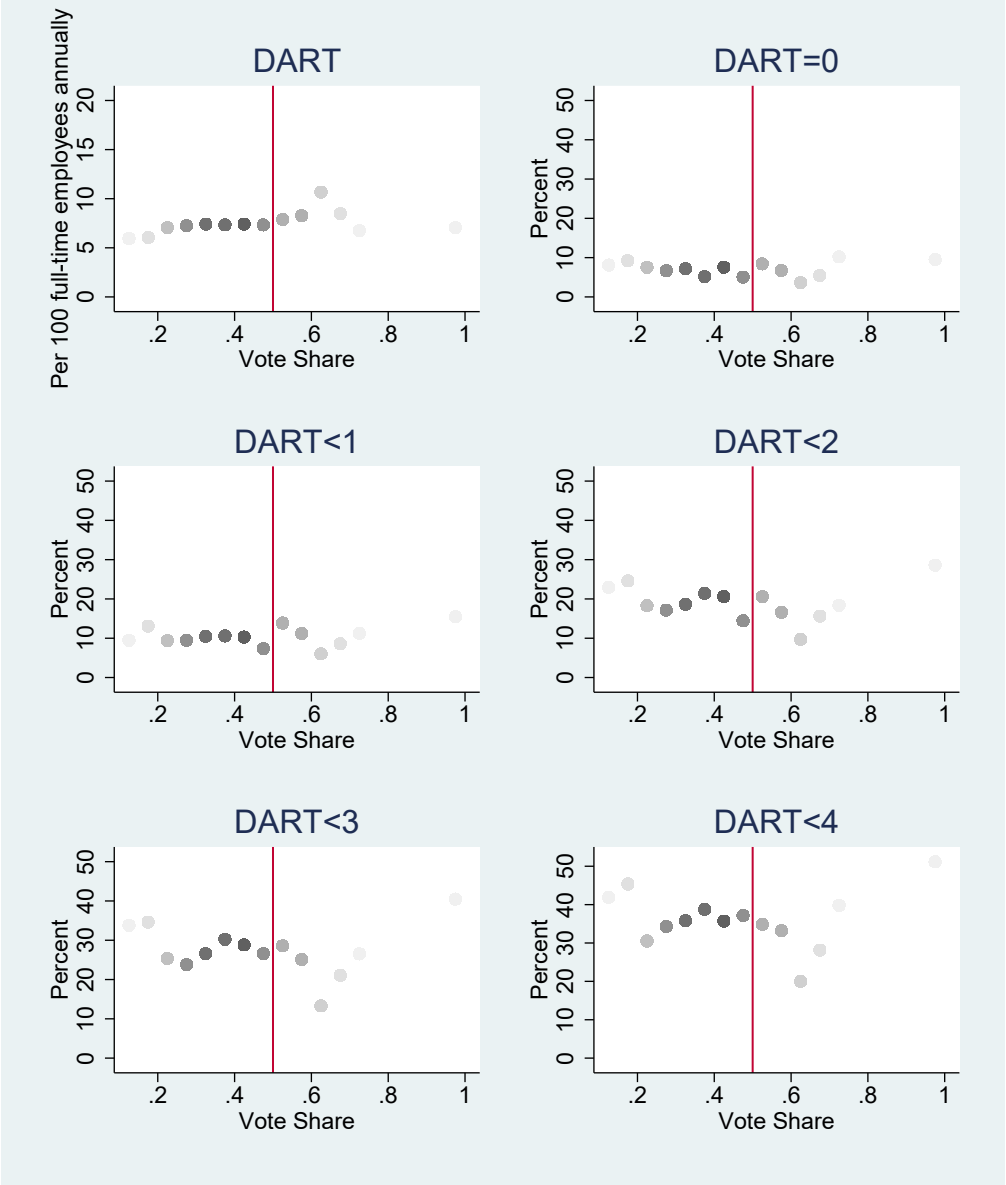
The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. The sample is also restricted to DART less than 20, which corresponds approximately to the 95th percentile. The 5 percentage point bin below corresponds to a vote share greater than or equal 45 and less than 50, and the 5 percentage point bin above corresponds to a vote share greater than or equal to 50 and less than 55.

Figure 5: Discontinuities in DART, Periods 1 to 3, Votes ≥70



The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. The markers are shaded to indicate the relative number of observations within each bin. If the number of bin observations exceeds 660, the marker is shaded grey scale 0, which is black. For every 40 fewer observations, the marker is shaded one additional point on the grey scale. If the number of observations is between 620 and 660, the marker is shaded grey scale 1.

Figure 6: Discontinuities in DART, Periods 1 to 3, Manufacturing



The sample is derived from union elections contained in the NLRB, file years 1991 to 2010, and is restricted to elections with at least 20 valid votes and a valid vote share in favor of unionization. The markers are shaded to indicate the relative number of observations within each bin. If the number of bin observations exceeds 660, the marker is shaded grey scale 0, which is black. For every 40 fewer observations, the marker is shaded one additional point on the grey scale. If the number of observations is between 620 and 660, the marker is shaded grey scale 1.