Effects of Unionization on Workplace Safety:
Regression Discontinuity Evidence from OSHA's Enforcement Data

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"The empirical literature on the relationship between unionization and workplace safety presents a curious puzzle. On the one hand, scholars have documented numerous ways unions help to promote safe work practices…. Yet most empirical studies of the relationship between unionization and important safety outcomes, such as injuries and fatalities, have failed to find statistically significant evidence of a `union safety effect.'" (Morantz, 2012)

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The empirical literature on union effects on OSH within firms struggles with two primary obstacles to credibly estimating the effect of unionization on workplace safety. First, unionized employees may be more likely to *report* occupational risks to OSHA, inducing greater rates of inspection and citation of unionized firms for violations than occurs in otherwise similar nonunion firms. This is a kind of measurement error in commonly-used workplace safety outcomes that is positively correlated with unionization. It would lead to a spurious negative association between unionization and safety. Second, less safe industries and less safe firms within industry may be more likely to unionize than more safe ones. This selection bias also would generate a negative association between unionization and safety.

This study will address both of these obstacles and deliver evidence about the effect of unionization on workplace safety. To overcome the first obstacle, the primary outcome will be a measure of workplace fatalities, which are subject to less measurement error and differential reporting than are less severe occupational injuries and illnesses. To overcome the second obstacle, I will use a regression discontinuity design (RDD) comparing establishments where unions just won National Labor Relations Board (NLRB) union-certification elections to establishments where unions just lost such elections (DiNardo &

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Lee, 2004; Frandsen, 2012; Sojourner, Town, Grabowski, Chen, & Frandsen, 2013). Rather than comparing union to nonunion establishments generally and relying on statistical controls and untestable identifying assumptions vulnerable to selection bias, I will restrict attention only to establishments where employees indicated an interest in unionizing such that the NLRB held a union-certification election. At the time of the election, establishments where the union just won are very similar to establishments where the union just lost. After the election, unions are certified as collective bargaining agents in the former set of establishments but not in the latter set. Around the 50% vote-share threshold, this generates quasi-random assignment of unionization to establishments and overcomes the selection problem.

Frandsen (2013) raises concerns about the validity of RDD designs using NLRB elections due to potential post-election, legal manipulation in very close elections. He suggests two basic ways to address this and concludes that these methods provide the best available design to estimate certification effects. The current study uses his suggestions. First, rather than using post-election levels as the outcome, we use changes in levels (post-election level minus pre-election level) as the outcome. This adjusts for any pre-election differences across the threshold and combines both difference-in-difference and RDD logic. Second, he suggests assessing results' sensitivity to deletion of cases where a very small number of votes could change the election outcome because these type of elections are most vulnerable to post-election manipulation.

I will study whether the two sets of establishments experienced different changes in their measure of occupational fatality rates post-election. Any difference can be interpreted as the effect of union certification. I will also study whether the establishments experienced different numbers of occupational fatalities and other observable characteristics *prior* to the election. This falsification exercise allows testing of a key identifying assumption of RDD.

Design and Data

Sample

The population is all U.S. private-sector establishments on the margin of unionization between 1962 and 2009 as measured by experiencing at least one NLRB certification election during this period. For each of the 253,449 elections with valid election month and year, the establishment name, city, state, 2-digit standard industrial classification (SIC) code, date of

election, and the numbers of employees voting in favor of unionization and against unionization are observed. In some cases, establishment street address is available.[2]

Because any establishment may have multiple NLRB elections, I construct longitudinal unique establishment identifiers using a fuzzy-matching algorithm. Across the set of establishments in the same state, city, and industry, the algorithm links establishments with similar names and addresses. This identifies 212,101unique establishments represented in the 253,677 elections with valid election month and year.[3]

To measure workplace safety, I use occupational fatalities because these are relatively well-measured. I draw on the OSHA enforcement database, which records occupational fatalities in U.S. private sector establishments back to 1970 (U.S. Department of Labor, 2014). This period included 103,768 accidents, of which 48,275 involved 55,058 fatalities. For each accident, the establishment's name, address, city, state, and SIC code are observed, as well as the accident date, number of workers killed, and other details. The database also includes records from all OSHA inspections, not just those triggered by accidents. It includes the same kind of establishment information. We will focus only on OSHA records from establishments that experienced NLRB elections.

NLRB and OSHA records are linked at the establishment level using fuzzy-matching based on establishment name, address, city, state, and industry. For each OSHA record, we look for a match among all the NLRB election records. Using the NLRB-based establishment identifiers, this yields a longitudinal database of all NLRB elections and all of their OSHA enforcement data, including reported occupational fatalities. This produces links to 135,366 OSHA records at 38,047 unique establishments that underwent NLRB certification elections. This implies that 18.0% of such establishments are linked to any OSHA record. Of these, 1,450 records in 1,154 unique establishments involve fatal accidents.

Design

[2] I integrate two databases that compile and standardize NLRB election records: one from Holmes (2006) that includes elections during 1977 to 1999 and which includes many establishments' street address and a second provided by Hank Farber covering 1962 to 2009 but lacking any street addresses.

[3] The algorithm requires the researcher to choose weights to value matching characters and penalize mismatches across records. Parameters were chosen by choosing different values and inspecting samples of the results by hand to assess which delivered the best quality matches. To assess sensitivity of final estimates to these nuisance parameters, I use two alternative algorithm parameterizations to generate two alternative sets of establishment identifiers. Results will be re-estimated using these alternative identifier sets.

For a given outcome (Y_i), the main treatment of interest is whether the union won the election or not, indicated by D_i. The forcing variable is the election's pro-union vote-share, which is centered at zero and adjusted to provide a support invariant to the number of voters (DiNardo & Lee, 2004). The basic model is:

$$Y_i = D_i\tau + f(X_i) + W_i\delta + U_i$$

Unionization depends deterministically on vote share, $D_i = 1[X_i > 0]$ and f is assumed to be continuous at 0. The causal effect of unionization near the certification threshold is identified by τ under the following continuity assumption on unobservable influences (U_i):

$$lim_{x\uparrow 0} E[U_i|W_i, X_i = x] = lim_{x\downarrow 0} E[U_i|W_i, X_i = x]$$

This condition means that no unobserved factor influences the outcome in a discontinuous manner across the election victory threshold. The only factor that shifts discontinuously at the threshold is unionization (D) so any observed differences in outcomes across the threshold after the election can be attributed to the causal effect of unionization.

The main regression discontinuity analysis will focus on NLRB elections that meet the following criteria:

1) *At least 20 individuals voted*: a vote-total-floor minimizes the risk that the exact outcome could be manipulated by the company, the union, or workers, which would somewhat undermine the quasi-randomization across the vote-share threshold (Frandsen, 2012).

2) *Election occurs after 1970:* ensures post-election fatalities may be observed in OSHA data.

3) *First election observed in an establishment*: Considering multiple elections for the same establishment raises a number of conceptual questions about whether an establishment should be considered as treated (union wins) or control (union loses). Focusing on only the first election in each establishment sidesteps these thorny issues. This election is termed the establishment's *focal election* (Sojourner, Town, Grabowski, Chen, & Frandsen, 2013).

Another issue raised by multiple elections is the possibility that unions or management learn enough through recently-past elections to manipulate the outcome of the election in such a way as to introduce systematic differences across the threshold in unobservables and, thereby, to invalidate the identifying assumption. This concern diminishes as the time between elections extends. Therefore, using the NLRB data between 1966 and 1970, we will also exclude any establishments that experienced an NLRB election regardless of outcome in the 5 years immediately prior to the focal election.

4) *No evidence of prior unionization:* Using the NLRB data back to 1962, we exclude all establishments where a union was certified prior to the focal election. This clarifies the interpretation of the treatment as a contrast between establishments with no unions certified as bargaining agents and any union so certified (Sojourner, Town, Grabowski, Chen, & Frandsen, 2013).

Filtering on criteria 1 and 2 reduces the number of unique establishments and focal elections to 103,918. These account for 77,979 OSHA records in 20,872 unique establishments including 938 fatal accidents in 739 unique establishments. After implementing criteria 3 and 4, the number of unique firms shrinks to 94,430. Among these, 93,309 have a recorded number of eligible voters (bargaining-unit size) no greater than the recorded number of total votes. This is our analytic sample.

Measures

The effect of establishment unionization on fatality rates will be analyzed in the cross-section. Assume that each establishment-*i* had exactly one NLRB election. To measure post-election fatality rate at each establishment, we construct the following variable:

$$Y_i^{post} \equiv \frac{(\#\ post\text{-}election\ fatalities)_i * 100{,}000}{[2013 - (election\ year)_i] * [2.43 * (\#\ in\ bargaining\ unit)_i]}$$

The key information is the number of post-election occupational fatalities observed at each establishment across all post-election years, which is measured directly from the matched NLRB-OSHA panel constructed above. Because there should be no systematic differences in the size of the establishments or the timing of elections across the 50% vote-share threshold, we could analyze this outcome directly. However, occupational fatality rates are

conventionally measured per 100,000 employees per year and measuring union effects in similar units will facilitate quality checking and interpretation. Dividing by the number of years of OSHA data available post-NLRB-election, (2013 – year of establishment's NLRB election), yields each establishment's occupational fatalities per year. Dividing through by the number of employees in the NLRB bargaining unit, that is the number eligible to vote in the election, adjusts for differences in establishment size.[4] To convert from the establishment's number of employees in the bargaining unit to the establishment's number of employees, we use a scaling factor of 2.43 based on auxiliary data.[5] Then, we multiply times 100,000. This is our measure of each establishment's post-election fatality rate per 100,000 employees per year.[6] Analogously, we construct an establishment-level pre-election fatality rate (Y_i^{pre}) using years between the election date and the first year of the OSHA data (1970) to count the number of years of pre-election observation and the number of pre-election, rather than post-election, fatal injuries observed in the numerator. Finally, we compute an establishment level change in fatality rate for use as our primary outcome ($\Delta Y_i = Y_i^{post} - Y_i^{pre}$).[7]

Table 1 provides summary statistics on change in fatality rate. Across the 93,309 establishments with focal NLRB elections, changes in fatal injury rate between post- and pre-election is average 0.12 per 100,000 employees per year with standard deviation 3.01. Pre-

[4] We cannot use OSHA variables to measure the number of workers per establishment because many establishments with NLRB elections never show up in the OSHA data. If unionization causes firms to shrink headcount or to go out of business, this study's estimates will be biased towards a negative effect of unionization on occupational fatality risk. Unionized firms will have less occupational fatalities partly because they have less employees and we lack a good measure of the number of post-election employees to adjust for this directly. However, Frandsen (2013) uses a very similar sample and design as the current study to study the question of union impacts on head count directly. He estimates a positive, though imprecise, effect of unionization on head count. Available evidence also suggests that unionization does not drive establishments out of business (Freeman & Kleiner, 1999; Sojourner, Town, Grabowski, Chen, & Frandsen, 2013).

[5] Frandsen (2013) linked NLRB election data to the Census Longitudinal Business Database over a similar period, which gave him a measure of each establishment's number of employees. He reports that the average number of voters is 93 and the average number of employees is 254. He does not report turn-out rates. In our, very-similar sample, among focal elections with more than 20 voters, the average turnout rate ≡ #voters/#eligible = 0.89. So, putting these facts together, we compute the scaling factor as #employees/#eligible = #employees*(turnout rate/#voters) = 254 *(.89/93) = 2.43.

[6] This is certainly not a perfect measure of occupational fatalities at each establishment. For years that they both exist, the OSHA enforcement data contain many fewer reported fatal injuries than the Census of Fatal Occupational Injuries (CFOI), which was developed to do a better job than OSHA enforcement data was doing. However, it is impossible to link CFOI data to establishment or firm identities so impossible to use in a RDD. The general implications of using a noisy outcome measure has been extensively studied. This issue will be discussed further in the context of the results.

[7] In robustness analysis, we use the same approach to measure pre-, post-, and change in establishment non-fatal injury rates requiring hospitalization and those not requiring hospitalization. Total injury rates are the computed using the sum of all three injury types: 1) fatal, 2) non-fatal requiring hospitalization, and 3) non-fatal and not requiring hospitalization.

election fatal injury rate will be used as a conditioning variable and for falsification testing. Its mean is 0.04 per 100,000 employees per year with standard deviation 1.42.

In all these models, covariates fixed at the time of the election (W_i) which may also influence the likelihood of post-election fatalities can be included in order to increase precision. Under RDD assumptions, they are not necessary to eliminate bias although they can help do so if these assumptions do not hold. I assembled a rich set of conditioning variables that help explain variation in establishment occupational safety. The most basic is a set of indicators of the establishment's broad industry, 1-digit SIC (Table 10). We also construct a measure of each establishment's narrow industry (2-digit) occupational fatality risk.[8] Across establishments in our sample, the average narrow-industry risk level is 0.06 fatalities per thousand FTE with standard deviation of 0.08 (Table 1), equivalent to 6.0 fatalities per 100,000 FTE per year. The average annual occupational fatality rate in the U.S. economy broadly fell from 5.0 per 100,000 FTE in 1992 to 4.2 in 2002. So, the establishments that have union elections tend to be from slightly riskier-than-average industries. Including both of these types of industrial variables compares outcomes in establishments in the same broad industry while controlling for differences in narrow-industry risk.

The fatal injury risk measure that I construct from the OSHA data has a far lower average than expected based on the sources above: 0.04 per 100,000 employees per year in the pre-election period and 0.15 in the post-election period. Further, this upward trend goes against what we know we know occurred in the overall economy, where fatality rates have fallen across these decades. This evidence is consistent with under-reporting of fatalities to OSHA, which improved somewhat over time. My measure of post-election rates is only about 2.5% of what is expected given average narrow-industry rates. This suggests real caution is warranted when interpreting effects on fatal rates, especially the magnitude of effects.

Within narrow industry, each establishment's own pre-election history of OSHA enforcement records (or absence thereof) should contain information about its idiosyncratic occupational injury risk. Pre-election OSHA enforcement actions are likely to be predictive

[8] The occupational fatality rate for each SIC2 industry each year is measured by the ratio of a) fatal occupational injuries from the Bureau of Labor Statistics (BLS) Census of Fatal Occupational Injuries per b) thousands of employees in the industry from the BLS Current Employment Statistics. To reduce measurement error, eleven annual rates, from 1992 to 2002, are averaged within SIC2 industry. This is based on all establishments in the U.S. economy, not just those in our sample and derived from sources completely outside the OSHA data.

of post-election injury risk so including measures of these experiences may also reduce bias and improve precision. For each establishment and pre-election inspection, OSHA assigns a number of current violations, initial penalty, current penalty, and failure-to-abate (FTA) penalty amount for each of 5 types of violations: Serious, Willful, Repeated, Other, and Unclassified. Appendix Figure 3 contains an example of the kind of data used. Because Other and Unclassified violations are very rare, I focus only on Serious, Willful, and Repeated violations. By dividing through by the appropriate number of pre-election years, I derive establishment-level measures of violation rates per pre-election year. Similarly, I construct measures of average annual penalties of 9 types, {serious, willful, repeated}x{initial, current, FTA}. A similar process gives to post-election and overall measures for each establishment. Appendix Table 11 provides summary statistics for the level of annualized violations and penalties across establishments in the sample.[9] This is rich measurement but these variables are highly collinear. Entering them all together as predictors in a regression might reduce precision.

To aggregate the information in the complete OSHA-inspection histories of each establishment and reduce multi-collinearity, I use factor analysis on pre-election measures to extract the single latent factor that explains the most variance in each set of measures. I use this approach to construct an index of each establishment's pre-election history of OSHA violations and an index of its history of OSHA penalties.[10] The pre-election index variables that are used as control variables are summarized in the bottom panel of Table 1. Establishment-level changes in these indices will be used as alternative outcomes and are summarized in the middle panel.

Finally, each OSHA inspection record contains a measure of whether a union representative was present during the inspection. From this, I construct a measure of the share of pre-election and post-election inspections that were attended by a union representative as well as a measure of the change in this share. In the pre-election period, an average of 1 percent of inspections were attended by union representatives. Because the sample is constructed to try to focus on establishments without union representation in the pre-election

[9] Column 1 presents overall establishment averages (SD) without respect to election date. Column 2 presents pre-election statistics and Column 3 presents post-election statistics.

[10] Column 4 of Table 11 reports the scoring coefficients used to aggregate the measures into each index. For instance, each establishment's pre-election violation index is measured as 0.52*(number of serious violations per pre-election year)+0.227*(number of willful violations per pre-election year)+0.258*(number of repeated violations per pre-election year). These same coefficients are used to score the post-election index. The penalty index is based on the first latent factor from analysis of the 9 pre-election penalty measures.

period, it is no surprise the share is so low.[11] This variable is useful for two purposes. First, the pre-election level can be used as a control variable that might pick up differences in unionization across establishments that are missed via the NLRB data. Second, by analyzing the effect of certification on the change in share of inspections attended by union representatives, we can get empirical verification of whether certification drives unionization, whether the RD design appears valid, and a measure of the effect of unionization on employee exercise of a federal right to representation during OSHA inspections.

Analysis

Assessing validity

We present evidence from two falsification tests of the validity of the RD identifying assumption, no pre-election discontinuity across the 50% vote-share threshold. First, Figure 1 presents a histogram of binned vote-shares across the sample of establishment NLRB elections. Most elections are close, giving a large share of the sample close to the threshold. However, we reject the null of no discontinuity in the density of vote shares across the threshold (t=4.137) (McCrary, 2008), consistent with concern about possible post-election manipulation (Frandsen, 2013). Second, we test for discontinuity across the threshold in the distribution of pre-election observables. I implement the test with a seemingly-unrelated regression model (Lee & Lemieux (2010): Section 4.4.2). Outcomes are pre-election fatality rate, violations index, penalty index, union-representative share, total number of focal NLRB-election votes, and narrow industry fatality rate. A piecewise linear function of vote-share that allows for different intercepts and slopes on either side of the certification threshold predicts. The set of establishments with vote-shares within a given bandwidth of the threshold (h) is the sample. Table 2 presents the results. The first column of results uses only establishments with vote shares within 0.10 of the threshold, those with 40-60% vote share, and presents the estimated discontinuity coefficients for each outcome with standard errors. Vote-share coefficients are not displayed. After estimation, a test is performed of the joint null hypothesis that, for all outcomes, there is no discontinuity across the threshold. Results are presented in the bottom row at p-values. For bandwidths 0.10, 0.20, and 0.30, the joint null is not rejected at the conventional 5% significance level. At data from establishments with less-close elections are included, the null is rejected due to a discontinuity in the total

[11] A union representative might be present if the employees organized outside the NLRB process.

number of votes. For the analysis, I will focus attention on elections within the 0.2 bandwidth where the assumptions appear valid and analyze results' sensitivity to alternative bandwidths.

Results

To start the analysis of effects, consider Figure 2(a). It plots the average change in establishment fatality rates among establishments in each of 20 vote-share bins of width 5%. There is not evidence of a discontinuity across the threshold in the average change in fatality rates. This change depends on pre-election levels (Figure 4(a)) and any post-election levels (Figure 5(a)). In inspecting these figures, no discontinuity is apparent in levels or changes.

To obtain an estimate and enable a statistical test, I estimate the effect of union certification on change in fatality rate among establishments with NLRB election vote shares within 0.2 of the certification threshold (pro-union vote share between 30% and 70%) controlling for a piece-wise linear function of vote share. The coefficient on the "Union certified" indicator measures the effect of union certification on the outcome. As displayed in specification (1) of Table 3, the estimated effect is 0.012 per 100,000 employees per year with a standard error of (0.031). This implies that a 95% confidence interval on the effect of union certification on post-election fatalities of [-0.060, 0.072]. The average post-election fatality rate across establishments in our sample is 0.15. The interval rules out effects with magnitude larger than half the base rate.

In specification (2) of Table 3, the establishment's pre-election violation index, penalty index, and union-representative share are added as controls. Each of these predictors has a strong relationship with changes in fatality rates but, most importantly, the estimated union certification effect is stable. Finally, the broad industry indicators and narrow industry fatal risk measure are added in specification (3). Again, the estimate remains very stable despite the fact that average narrow-industry fatality rate is a strong predictor. This stability suggests that the union certification "treatment" is uncorrelated with all observable measures of risk, as predicted by a valid RD design and similar to a randomly-assigned treatment. It is also consistent with an outcome that is just noise.[12]

Heterogeneity of effects?

[12] The OSHA-based fatality rate outcome measure's strong relationship with predictors, particularly with narrow-industry fatality rate which is derived from independent sources (COFI and CES) suggests that the outcome measure constructed here is not just noise, that it contains information about establishment fatality rates. However, the fact that narrow-industry levels predicts changes in OSHA-based rates bears further inquiry.

Effects of unionization may differ between industries with different levels on inherent risk. In riskier industries, occupational safety may be more of an issue in labor-management relations and this may change the effect of union certification. To explore this possibility, we divide the sample into quartiles based on the occupational fatality risk experience of all establishments nationally in the same 2-digit SIC code. The risk level topping the first (second) (third) quartile is 0.025 (0.035) (0.078) fatal occupational injuries per 100,000 employees. Table 4 presents estimates analogous to those in Table 3, column (3) but within each narrow-industry risk quartile. For each risk quartile, the effect of union certification is not significant and the point estimates are small and not significant.

Robustness

I assess the robustness of the main result. In all the results presented here, I use the richest specification, (3). However, for compactness, only the estimated coefficient (SE) on the union certification effect is presented in each case.

First, I vary the bandwidth from the baseline case of 0.2. Column (1) of Table 5 presents the estimate based on a bandwidth of 0.1, looking only at establishments with elections with pro-union vote shares between 40% and 60%. The estimate is -0.0261 (0.043). Column (2) reproduces the baseline result from Table 3 for comparison. Column 3 (4) (5) uses bandwidth 0.3 (0.4) (0.5). In every case, the effect of union certification is not significant.

Second, I look at effects on occupational injury outcomes other than fatal injury rate.[13] Column (2), (3), and (4) of Table 6 present the estimated effect of union certification on the risk of injuries requiring hospitalization, injuries not requiring hospitalization, and total injuries, respectively. In every case, the effect of union certification is not significant.

Third, as mentioned earlier, a recent working paper reports evidence that, in very close elections, post-election legal maneuvering may undermine the key identifying assumption of the RDD (Frandsen, 2013). In the elections with the narrowest margins of victory (the smallest difference between the number of pro-union votes cast and the number of pro-union votes necessary for the union to win certification), incentives for manipulation are strongest and there is compelling evidence that management and unions are able to manipulate final vote counts in the elections with the narrowest margins of victory. In fact,

[13] In all these and subsequent analyses, the baseline bandwidth of 0.2 is used.

evidence from the McCrary test is consistent with this kind of violation in our data. Analyzing changes is one way of dealing with this issue. Here, I present evidence using a second way. I use a donut regression discontinuity design, which involves excluding cases very close to the threshold (Barreca, Guldi, Lindo, & Waddell, 2011), which are most likely to reflect manipulation. In this case, the exclusion is made on the basis of margin of victory (MOV) rather than vote share because the ease of manipulation depends on the former rather than the latter. Column (1) of Table 7 excludes establishments where the election outcome would be changed if 1 vote switched sides. Moving right-ward across the table's columns, progressively more establishments are excluded with progressively larger margins of victory. The estimated effects of union certification are never significant.[14]

Our analysis is dependent on the algorithm that we used to measure records that belong to the same establishment within and across the NLRB and OSHA datasets. Within state and SIC2 code, the matching algorithm penalizes mismatched string values in establishment name, address, and city and matches with quality above a given threshold are retained. We constructed two alternative measures of records belonging to the same establishment by varying the threshold up and down. Each defines a somewhat different set of establishments with associated NLRB and OSHA records. Consequently, all variables defined at the establishment level vary somewhat. Table 8 presents estimates based on these two alternative penalization weights, along with our baseline estimate (NLRB_id2). Results are quite stable.

In conclusion, our evidence on the effect of union certification on occupational fatality rates is consistent with two interpretations. First, there is little effect. Second, our measure of the outcome is very noisy and the estimated effect suffers from attenuation bias. Future work will develop evidence to try to disentangle these stories.

Alternative outcomes

We also analyze the effect of union certification on establishment's interactions with OSHA enforcement. These outcomes are more reliable because the data are very complete and accurate with respect to OSHA enforcement actions. Consider the results reported in Table 9. Column (1) reports that union certification is not significantly associated with increased violations rates, although the point estimate is positive. However, it does cause a

[14] Sample size drops rapidly because we are losing most of the small elections; if they have a large MOV, they must not be close. If they are close, they must have a small MOV.

positive change in penalties (Column (2)), the magnitude of which – 0.238 – is about a sixth of a standard deviation of the sample's distribution of changes in the penalty index. Finally, union certification causes a significant increase in the establishment's share of OSHA inspections performed in the presence of a union representative. This discontinuity shows up very clearly in Figure 2(d). The magnitude of the change, a 5.5 percent increase, may seem small. However, recall that only 18 percent of establishments with NLRB elections had any linked OSHA inspections. So 82 percent of the sample has a 0 share pre-election, post-election, and for its change. Scaling the 5.5 percent effect estimate up by a factor of 5.6 (=1/(1-.82)) yields an estimated effect of 30.5 percent effect among those with any inspections.

Limitations

First, because occupational fatalities are extreme events and thankfully rare, the outcome has less variance across establishments than nonfatal occupational accidents does. An outcome with little variance makes it difficult to generate power to detect effects and helps explain why an exclusive focus on occupational fatalities has been rare in the research literature. For this reason, it is essential to have a large sample. The ability to analyze all establishments with NLRB elections nationally over many decades is particularly useful here. However, the OSHA enforcement data may provide only a weak proxy true fatality rates and the linking process may weaken this further. Further work is needed to assess the value of this proxy.

Second, this design is most informative about the effects of the weakest unions, those that barely win elections. Further, NLRB certification of a union is not the same as persistent unionization, as only about of NLRB-certified bargaining units ever negotiate and sign first contracts. The largest studies that relied on NLRB regression discontinuity designs have found null effects on the outcomes they measured, including firm average wage, employment, survival, revenue, and profits (DiNardo & Lee, 2004; Lee & Mas, 2012).

Conclusion

Union certification increases the participation of worker representatives during OSHA inspections and to higher penalties being assessed for OSHA violations. We do not detect a significant effect on the number of OSHA violations or on our noisy measures of occupational injury risk.

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Tables

Table 1: Summary statistics

Variable	Mean	Std. Dev.	Min	Max
Vote share	0.49	0.22	0	1
Eligible voters	105.08	216.32	20	19000
Changes in				
Fatal injury rate	0.12	3.01	-143.9	182.6
Violation index	0.06	0.97	-159.0	42.8
Penalty index	0.12	1.72	-195.1	151.4
Union-rep share	0.05	0.23	-1	1
Pre-election				
Fatal injury rate	0.04	1.42	0	143.9
Violation index	0.00	0.80	0	161.1
Penalty index	0.00	0.99	0	199.5
Union-rep share	0.01	0.10	0	1
Average fatality rate in 2-digit SIC	0.06	0.08	0	0.4

Notes: Pre- and post-election injury rates – including fatality, hospitalized, non-hospitalized and total – were calculated by dividing each count by (100,000 /(2.43*eligible voters*number of years)). For violations and penalties, annual rates were calculated by dividing each count by number of years. Indexes of violation and penalty are predicted value of factor analysis of variables including serious willful and repeated violations. The number of observation of each variable is 93,309.

Table 2: Coefficients on union certified in Seemingly Unrelated Regression at varied bandwidth

Bandwidths: Outcome variables:	(1) 0.1	(2) 0.2	(3) 0.3	(4) 0.4	(5) 0.5
Pre-election fatality rate	0.00240	-0.0237	-0.00724	-0.00361	-0.0147
	(0.0263)	(0.0209)	(0.0183)	(0.0175)	(0.0165)
Pre-election violations index	0.00899	-0.00246	0.00196	0.0116	0.00687
	(0.0151)	(0.00991)	(0.00792)	(0.0106)	(0.00930)
Pre-election penalty index	0.00820	-0.00767	-0.00211	0.00667	-0.00112
	(0.0304)	(0.0172)	(0.0127)	(0.0127)	(0.0115)
Pre-election union-rep share	-0.000565	0.00123	0.00235	0.00189	-0.00170
	(0.00244)	(0.00162)	(0.00132)	(0.00118)	(0.00114)
Total number of votes	11.34*	4.380	-5.634*	-14.77***	-25.14***
	(5.365)	(3.526)	(2.762)	(2.432)	(2.160)
Average fatality rate in 2-digit SIC	0.00111	0.000662	0.000766	0.00194	0.00118
	(0.00194)	(0.00136)	(0.00113)	(0.00102)	(0.000940)
Observations	29,100	55,984	74,923	85,822	93,309
chi^2	5.49	4.02	8.23	43.6	139.99
p-value	0.4831	0.6738	0.2216	0.0000	0.0000

Note: Estimate (SE). ***Significant at the 1% level, **Significant at the 5% level, *Significant at the 10% level. All estimates shows coefficients for the union certified in seemingly unrelated regressions. chi^2 and p-value are from the joint hypothesis test of null discontinuity effects across pre-election variables.

Table 3: Effect of union certification on change in fatality rate at bandwidth 0.2 with varying sets of conditioning variables

VARIABLES	(1) Change in fatality rate	(2) Change in fatality rate	(3) Change in fatality rate
Union certified	0.012	0.008	0.010
	(0.031)	(0.031)	(0.031)
Vote share	-0.168	-0.163	-0.155
	(0.176)	(0.176)	(0.176)
Vote share*certified	0.263	0.279	0.294
	(0.290)	(0.290)	(0.290)
Pre-election violation index		-0.120***	-0.123***
		(0.0133)	(0.0134)
Pre-election penalty index		0.0174***	0.0181***
		(0.00508)	(0.00508)
Pre-election union-representative share		0.216***	0.222***
		(0.0696)	(0.0698)
1-digit SIC code 2 indicator			0.0986
			(0.0651)
1-digit SIC code 3 indicator			0.0806
			(0.0653)
1-digit SIC code 4 indicator			-0.0114
			(0.0634)
1-digit SIC code 5 indicator			0.0211
			(0.0690)
1-digit SIC code 6 indicator			0.00761
			(0.0772)
1-digit SIC code 7 indicator			0.0655
			(0.0731)
1-digit SIC code 8 indicator			0.0257
			(0.0706)
Average fatality rate in 2-digit SIC code			0.555***
			(0.148)
Constant	0.0527***	0.0533***	-0.0372
	(0.0204)	(0.0204)	(0.0696)
Observations	55,984	55,984	55,984
R^2	0.000	0.002	0.003
Adjusted R^2	-3.37e-05	0.00204	0.00250

Notes: Estimate (SE). ***Significant at 1% level, **Significant at 5% level, *Significant at 10% level.

Table 4: Effect of union certification on changes in fatality rate by establishments' occupational risk quartile at bandwidth 0.2

	(1)	(2)	(3)	(4)
Industrial Risk Quartile	Q1	Q2	Q3	Q4
Union certified	-0.0121	-0.000465	0.0509	0.0620
	(0.0191)	(0.0495)	(0.0997)	(0.104)
Observations	17,662	15,568	10,256	12,498
R^2	0.001	0.001	0.004	0.018
Adjusted R^2	0.000544	0.000692	0.00286	0.0170

Notes: Estimate (SE). ***Significant at the 1% level, **Significant at the 5% level, *Significant at the 10% level. All estimates based on specification (3) in Table 3 that also includes piecewise linear function vote share, establishment's pre-election occupational fatality, hospitalization, and other injury records, OSHA enforcement action histories, broad industry (SIC1) indicators, and narrow industry (SIC2) occupational fatality rate. Data is divided into four by average fatality rate by 2-digit SIC code. Q1 includes the establishments with the lowest risk while Q4 with the highest risk.

Table 5: Effect of union certification on occupational fatality rate at varying bandwidths

Bandwidth:	(1) 0.1	(2) 0.2	(3) 0.3	(4) 0.4	(5) 0.5
Union certified	-0.0261	0.00957	0.00481	0.00527	0.0227
	(0.0433)	(0.0312)	(0.0260)	(0.0237)	(0.0223)
Observations	29,100	55,984	74,923	85,822	93,309
R^2	0.009	0.003	0.002	0.004	0.004
Adjusted R^2	0.00823	0.00250	0.00135	0.00373	0.00360

Notes: Estimate (SE). ***Significant at the 1% level, **Significant at the 5% level, *Significant at the 10% level. All estimates based on specification (3) in Table 3 that also includes piecewise linear function vote share, establishment's pre-election occupational fatality, hospitalization, and other injury records, OSHA enforcement action histories, broad industry (SIC1) indicators, and narrow industry (SIC2) occupational fatality rate.

Table 6: Effect of union certification on various changes in occupational injury rates at bandwidth 0.2

Outcome:	(1) Change in Fatality	(2) Change in Hospitalized	(3) Change in Non-hospitalized	(4) Change in Total Injuries
Union certified	0.010	0.118	0.015	0.012
	(0.031)	(0.076)	(0.043)	(0.092)
Observations	55,984	55,984	55,984	55,984
R^2	0.003	0.003	0.001	0.003
Adjusted R^2	0.00250	0.00230	0.000397	0.00227

Notes: Estimate (SE). ***Significant at the 1% level, **Significant at the 5% level, *Significant at the 10% level. All estimates based on specification (3) in Table 3 that also includes piecewise linear function vote share, establishment's pre-election occupational fatality, hospitalization, and other injury records, OSHA enforcement action histories, broad industry (SIC1) indicators, and narrow industry (SIC2) occupational fatality rate.

Table 7: Effect of union certification on change in occupational fatality rate at bandwidth 0.2, varying exclusion of elections with narrow margins of victory (MOV) to account for possible post-election manipulation

Exclude elections that would change outcome if MOV votes switched sides	(1) MOV = 1	(2) MOV = 2	(3) MOV = 4	(4) MOV = 6
Union certified	0.00595	0.0256	0.0212	0.0116
	(0.0335)	(0.0359)	(0.0415)	(0.0458)
Observations	53,280	47,291	34,780	24,552
R^2	0.003	0.003	0.005	0.007
Adjusted R^2	0.00254	0.00309	0.00471	0.00652

Notes: Estimate (SE). ***Significant at the 1% level, **Significant at the 5% level, *Significant at the 10% level. All estimates based on specification (3) in Table 3 that also includes piecewise linear function vote share, establishment's pre-election occupational fatality, hospitalization, and other injury records, OSHA enforcement action histories, broad industry (SIC1) indicators, and narrow industry (SIC2) occupational fatality rate.

Table 8: Effect of union certification by strictness of matching-algorithm

VARIABLES	(1) NLRB id1 Post-election fatality	(2) NLRB id2 Post-election fatality	(3) NLRB id3 Post-election fatality
Union certified	0.0111	0.00957	0.00343
	(0.0313)	(0.0312)	(0.0317)
Observations	56,584	55,984	55,523
R^2	0.002	0.003	0.002
Adjusted R^2	0.00173	0.00250	0.00163

Note: Establishments were matched using three different matching algorithms by strictness. (NLRB id1: strgroup threshold 0.2, NLRB id2: strgroup threshold 0.25, NLRB id3: strgroup threshold 0.3). All estimates based on specification (3) in Table 3.

Table 9: Effect of union certification on OSHA-enforcement outcomes at bandwidth 0.2

Outcome:	(1) Change in violation index	(2) Change in penalty index	(3) Change in union-representative share
Union certified	0.031	0.238***	0.055***
	(0.021)	(0.053)	(0.004)
Observations	55,984	55,984	55,984
R^2	0.000	0.000	0.017
Adjusted R^2	4.75e-06	0.000386	0.0169

Notes: Estimate (SE). ***Significant at the 1% level, **Significant at the 5% level, *Significant at the 10% level. All estimates based on specification (3) in Table 3 that also includes piecewise linear function vote share, establishment's pre-election occupational fatality, hospitalization, and other injury records, OSHA enforcement action histories, broad industry (SIC1) indicators, and narrow industry (SIC2) occupational fatality rate.

Figures

Figure 1: Histogram of vote-shares across NLRB elections with at least 20 votes cast

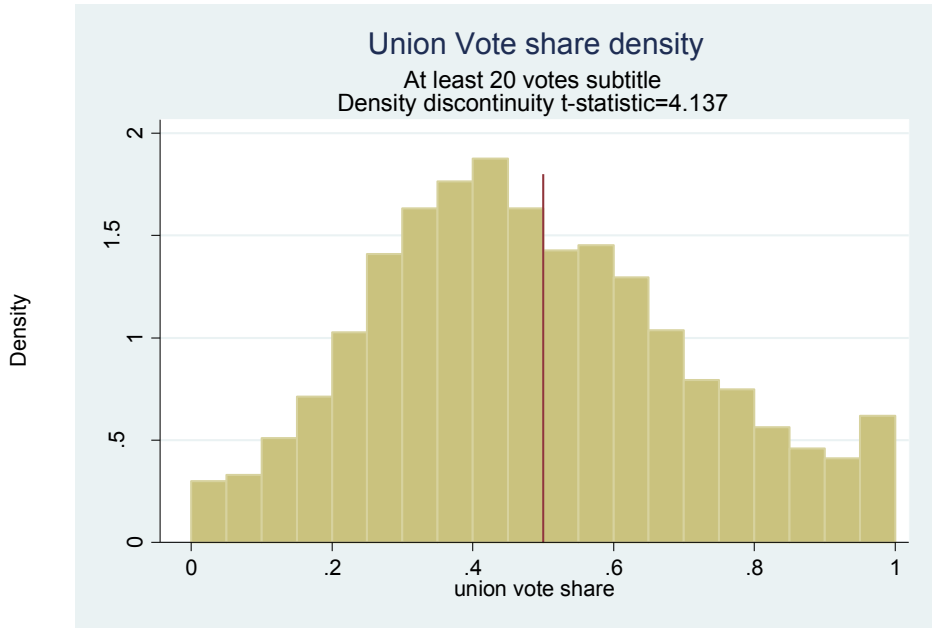
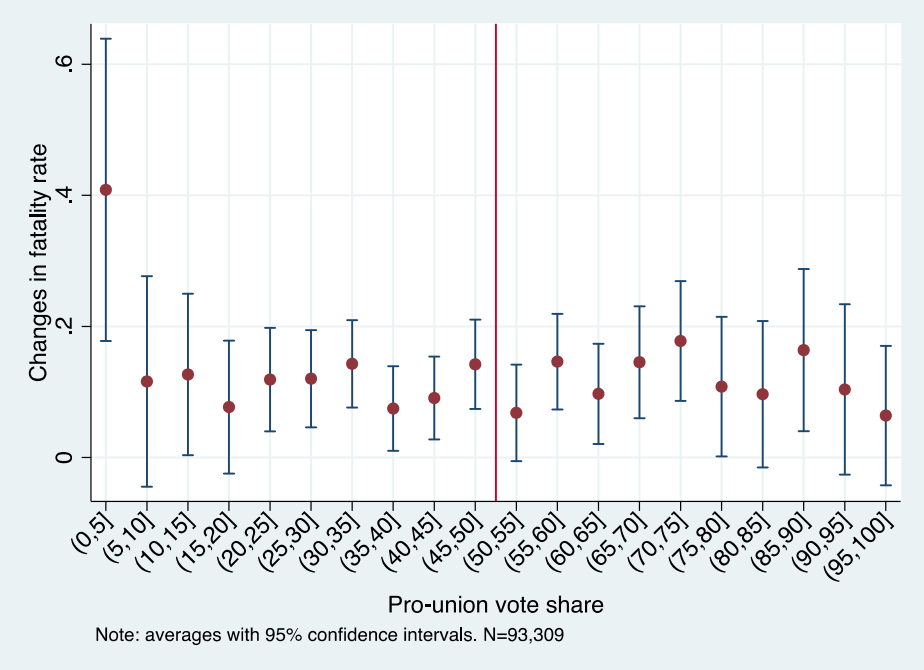
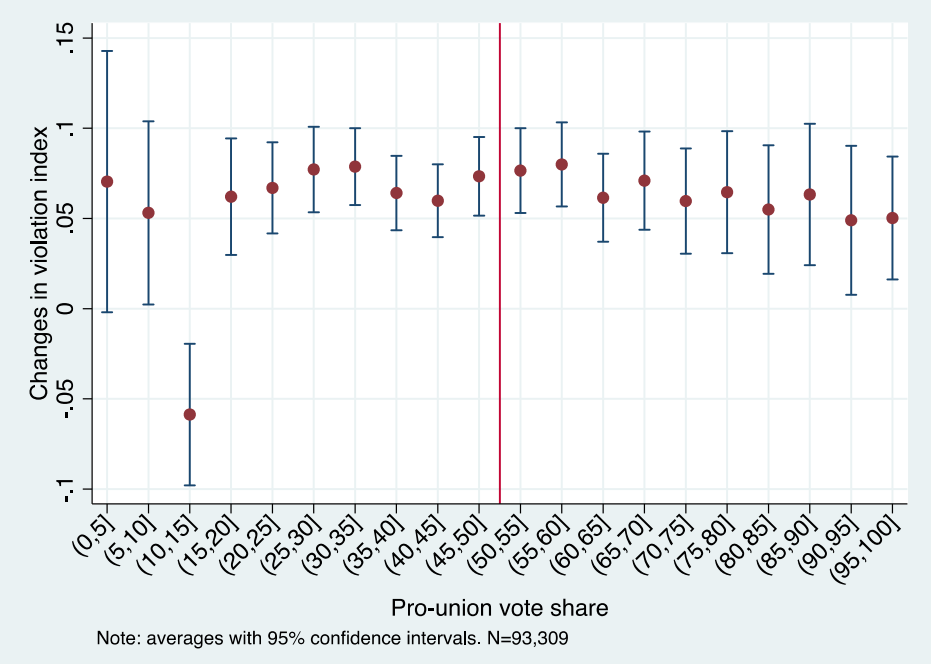


Figure 2: Average establishment (a) change in fatality rate (b) change in violation index, (c) change in penalty index, and (d) change in union-rep share by vote-share bin

(a)



(b)



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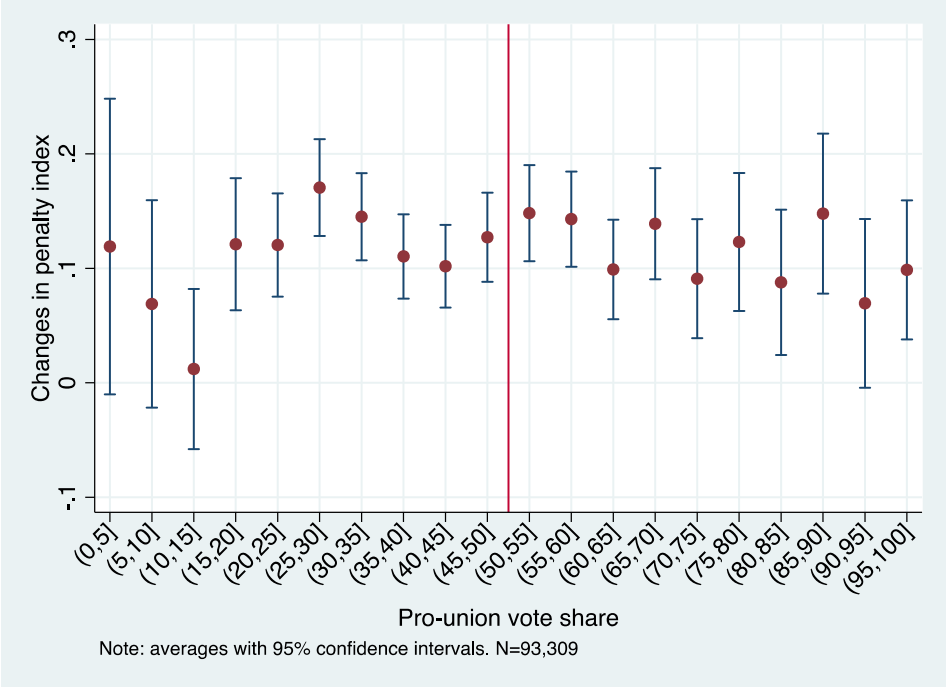
Changes in penalty index
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.1
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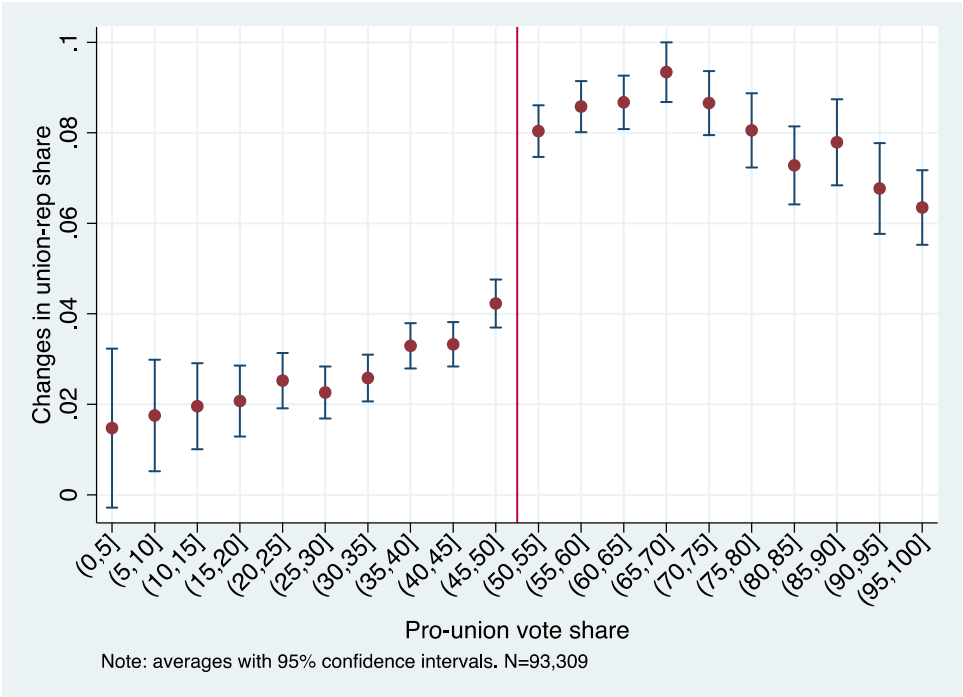
Pro-union vote share
Note: averages with 95% confidence intervals. N=93,309

(d)


Changes in union-rep share
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(75,80]
(80,85]
(85,90]
(90,95]
(95,100]
Pro-union vote share
Note: averages with 95% confidence intervals. N=93,309

Appendix Tables & Figures

Table 10: Distribution of elections across industries

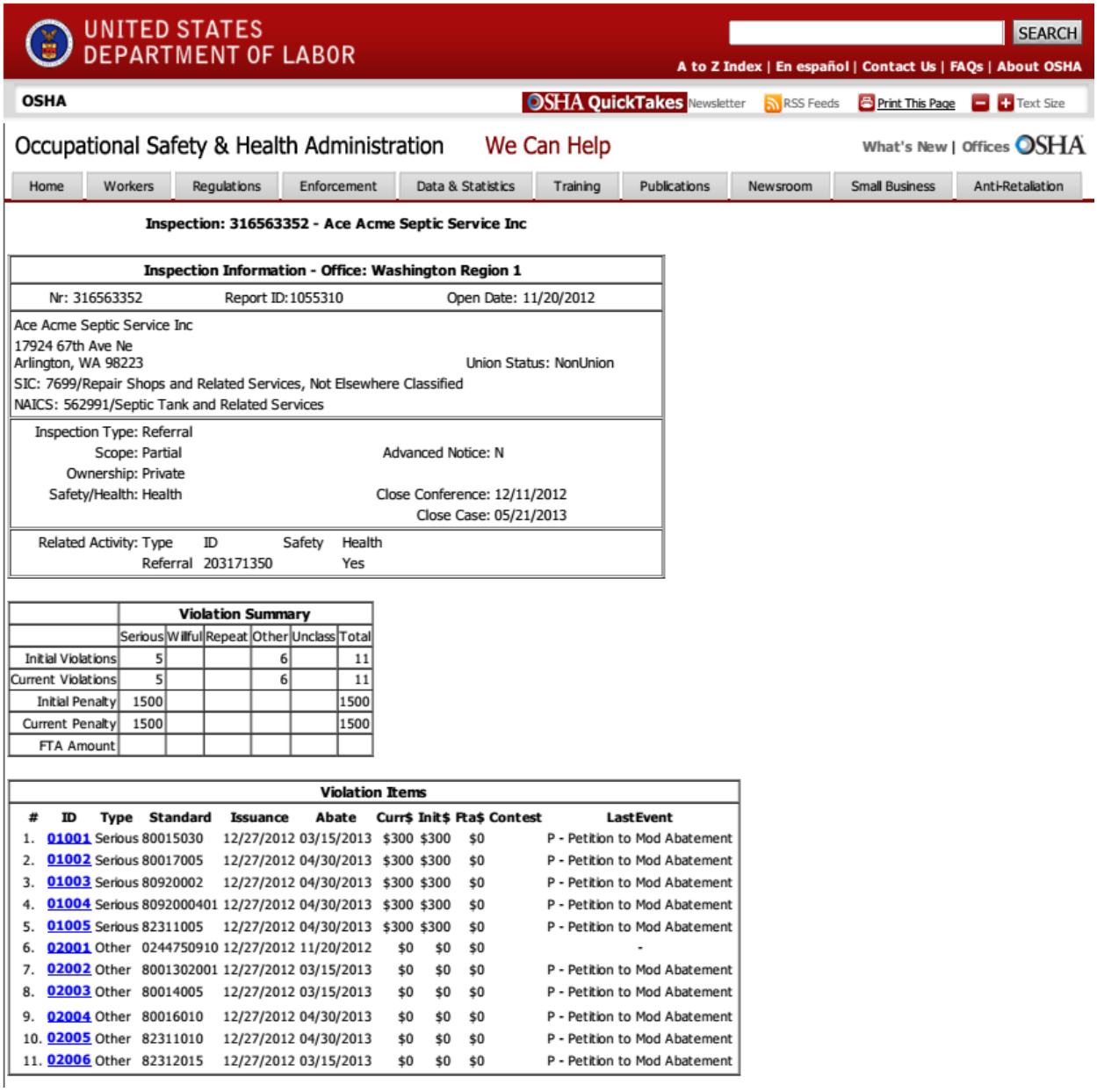
1-digit SIC code	Freq.	Percent
1 (Mineral Industries/Construction Industries)	3,078	3.27
2 (Manufacturing)	18,351	19.50
3 (Manufacturing)	30,677	32.59
4 (Transportation, Communications, and Utilities)	10,309	10.95
5 (Whole sale Trade/Retail Trade)	13,604	14.45
6 (Finance, Insurance, and Real estate)	2,925	3.11
7 (Service Industries)	6,708	7.13
8 (Service Industries)	8,466	9.00
Total	**94,118**	**100**

Table 11: Summary stats and factor construction

Variable	(1) Overall Mean (SD)	(2) Pre-election mean (SD)	(3) Post-election mean (SD)	(4) Penalty index scoring coefficient
Violations; annual number of				
Serious violations	0.03 (0.14)	0.01 (0.14)	0.03 (0.17)	0.521
Willful violations	0.00 (0.02)	0.00 (0.04)	0.00 (0.02)	0.227
Repeated violations	0.00 (0.02)	0.00 (0.04)	0.00 (0.02)	0.258
Penalty measures; annual averages				
Initial Penalties for:				
Serious violations	25.5 (191.4)	5.8 (104.4)	32.8 (277.8)	0.145
Willful violations	5.5 (240.5)	2.1 (246.9)	5.8 (256.9)	0.107
Repeated violations	3.1 (78.2)	1.0 (69.9)	3.8 (102.4)	0.135
Current Penalties for:				
Serious violations	15.6 (113.4)	3.8 (62.5)	19.9 (162.1)	0.237
Willful violations	2.9 (138.3)	1.0 (117.7)	3.1 (143.0)	0.296
Repeated violations	1.8 (42.7)	0.7 (53.6)	2.1 (54.6)	0.283
Failure-to-abate Penalties for:				
Serious violations	0.4 (17.0)	0.2 (10.9)	0.4 (21.0)	0.019
Willful violations	0.1 (16.9)	0.1 (20.6)	0.1 (13.1)	0.004
Repeated violations	0.0 (3.3)	0.0 (2.4)	0.0 (5.1)	0.028

Notes: Annual rates of violations and penalties were calculated by dividing each count by number of years. The number of observation of each variable is 93,309.

Figure 3: Example of OSHA enforcement data for a particular establishment



UNITED STATES DEPARTMENT OF LABOR

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Inspection: 316563352 - Ace Acme Septic Service Inc

Inspection Information - Office: Washington Region 1		
Nr: 316563352	Report ID: 1055310	Open Date: 11/20/2012

Ace Acme Septic Service Inc
17924 67th Ave Ne
Arlington, WA 98223 — Union Status: NonUnion
SIC: 7699/Repair Shops and Related Services, Not Elsewhere Classified
NAICS: 562991/Septic Tank and Related Services

Inspection Type: Referral
Scope: Partial — Advanced Notice: N
Ownership: Private
Safety/Health: Health — Close Conference: 12/11/2012
Close Case: 05/21/2013

Related Activity: Type	ID	Safety	Health
Referral	203171350		Yes

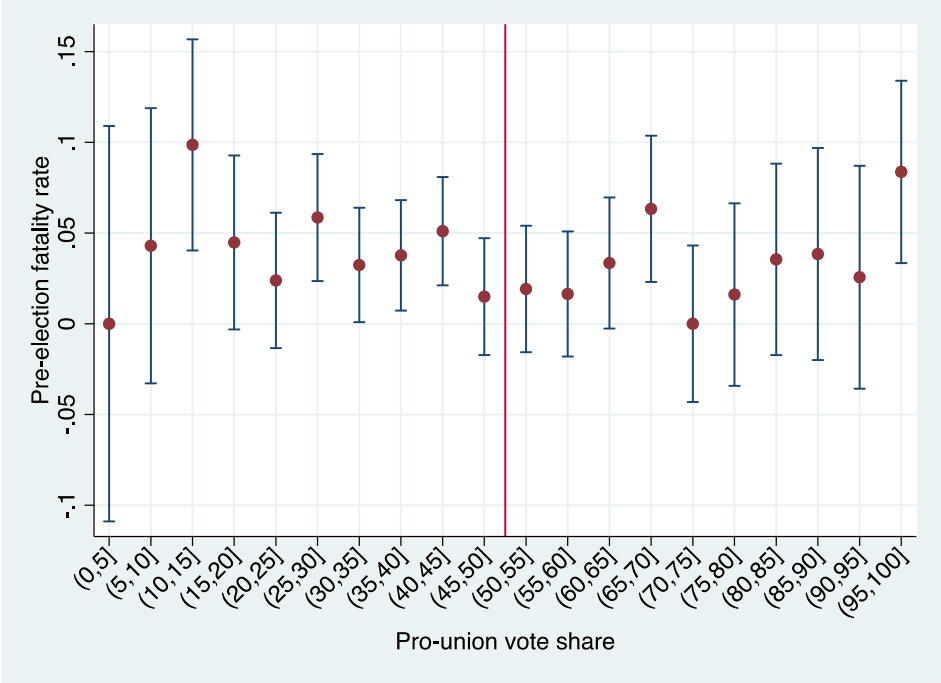
	Violation Summary					
	Serious	Willful	Repeat	Other	Unclass	Total
Initial Violations	5			6		11
Current Violations	5			6		11
Initial Penalty	1500					1500
Current Penalty	1500					1500
FTA Amount						

Violation Items

#	ID	Type	Standard	Issuance	Abate	Curr$	Init$	Fta$	Contest	LastEvent
1.	01001	Serious	80015030	12/27/2012	03/15/2013	$300	$300	$0		P - Petition to Mod Abatement
2.	01002	Serious	80017005	12/27/2012	04/30/2013	$300	$300	$0		P - Petition to Mod Abatement
3.	01003	Serious	80920002	12/27/2012	04/30/2013	$300	$300	$0		P - Petition to Mod Abatement
4.	01004	Serious	8092000401	12/27/2012	04/30/2013	$300	$300	$0		P - Petition to Mod Abatement
5.	01005	Serious	82311005	12/27/2012	04/30/2013	$300	$300	$0		P - Petition to Mod Abatement
6.	02001	Other	0244750910	12/27/2012	11/20/2012	$0	$0	$0		-
7.	02002	Other	8001302001	12/27/2012	03/15/2013	$0	$0	$0		P - Petition to Mod Abatement
8.	02003	Other	80014005	12/27/2012	03/15/2013	$0	$0	$0		P - Petition to Mod Abatement
9.	02004	Other	80016010	12/27/2012	04/30/2013	$0	$0	$0		P - Petition to Mod Abatement
10.	02005	Other	82311010	12/27/2012	04/30/2013	$0	$0	$0		P - Petition to Mod Abatement
11.	02006	Other	82312015	12/27/2012	03/15/2013	$0	$0	$0		P - Petition to Mod Abatement

Figure 4: Average establishment (a) pre-election fatality rate, (b) pre-election violation index, (c) pre-election penalty index, and (d) pre-election union-rep share by vote-share bin

(a)



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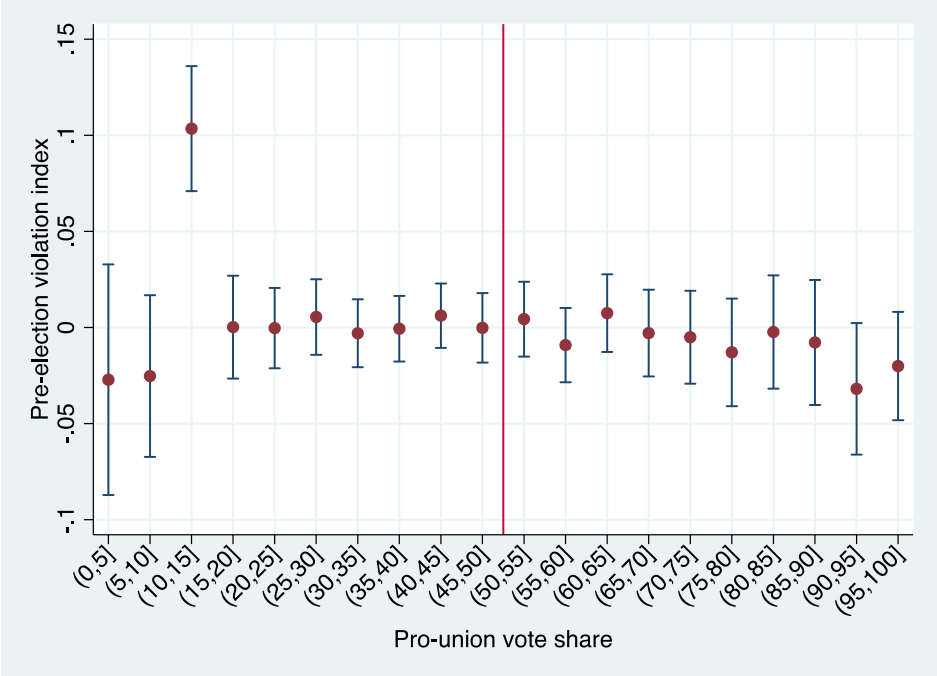

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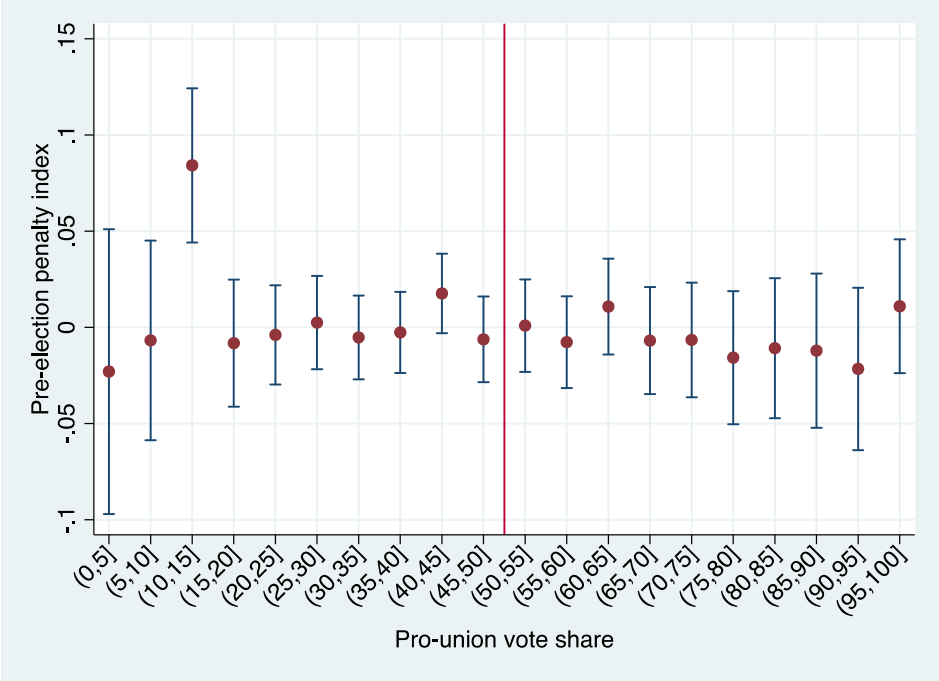
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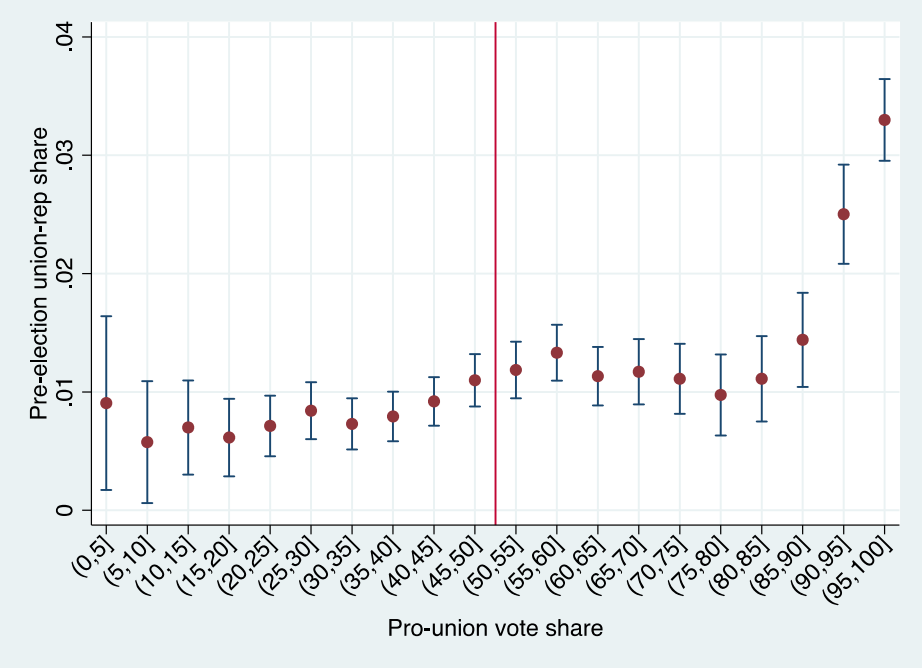
Pro-union vote share

(d)


Pre-election union-rep share
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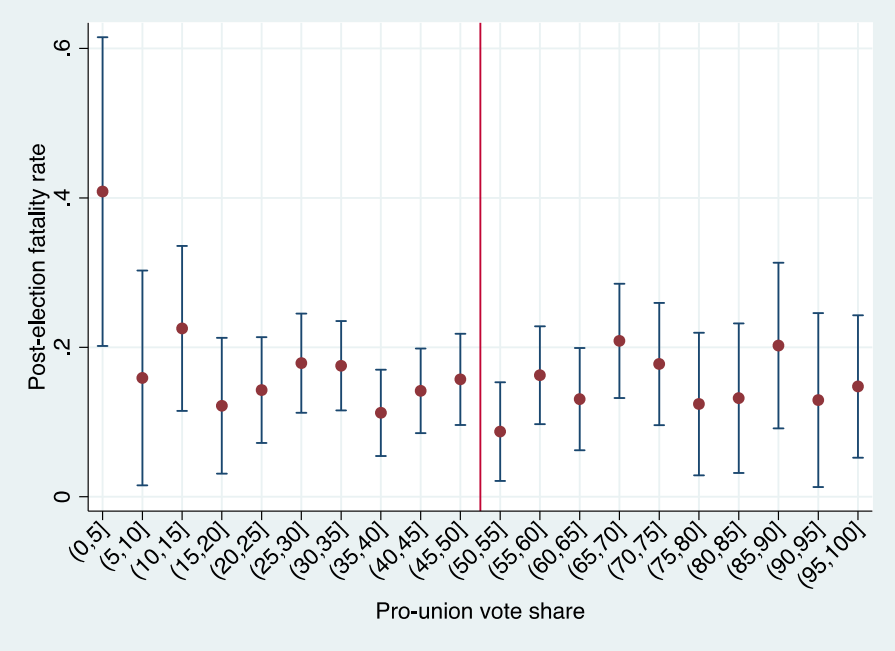
(95,100]
Pro-union vote share

Figure 5: Average establishment (a) post-election fatality rate, (b) post-election violation index, (c) post-election penalty index, and (d) post-election union-rep share by vote-share bin

(a)



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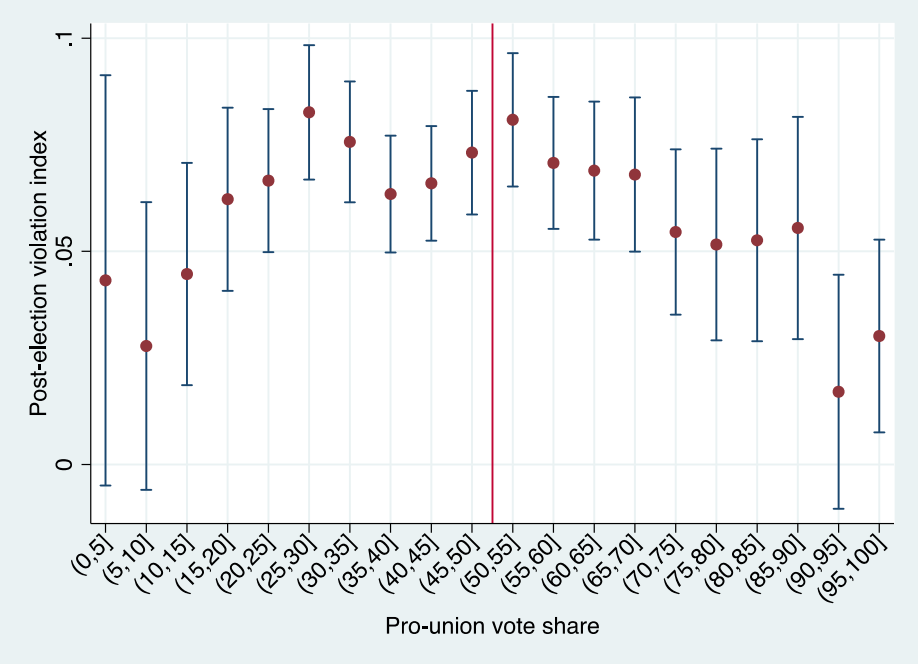
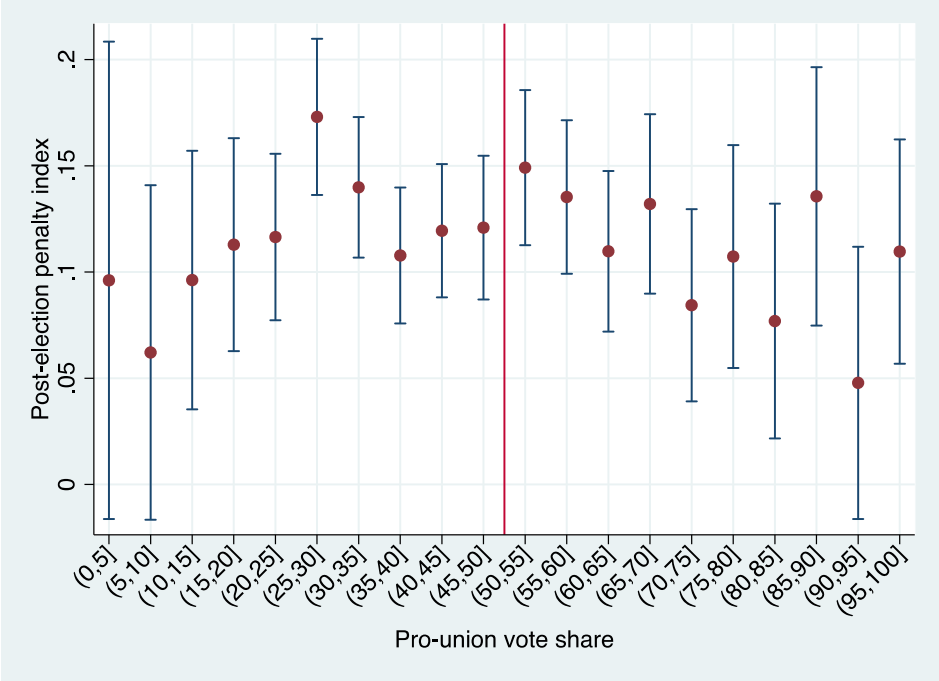


(c)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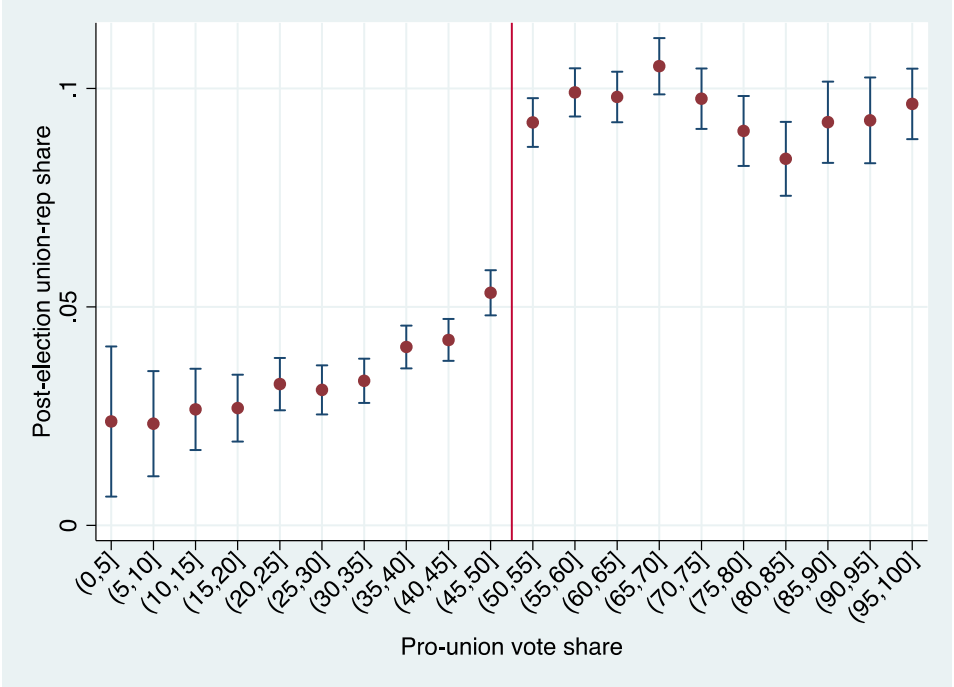
Pro-union vote share

(d)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(95,100]
Pro-union vote share