

Unionization, Employer Opposition, and Establishment Closure^{*}

Sean Wang[†] Samuel Young[†]

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Abstract

We study the effect of private-sector unionization on establishment employment and survival. Specifically, we analyze National Labor Relations Board (NLRB) union elections from 1981 to 2005 using administrative Census data on the universe of establishments in the U.S. Our empirical strategy extends a difference-in-differences design with regression discontinuity extrapolation methods. This strategy allows us to estimate treatment effects that include elections that win by large margins of support. We show that unionization decreases an establishment's employment and likelihood of survival. We hypothesize that two reasons for these effects are firms' ability to avoid working with new unions and managers' opposition to unions. We test this hypothesis for unionization in manufacturing. There, the negative effects are significantly larger for elections at multi-establishment firms. Additionally, after a successful union election at one establishment, employment increases at the firms' other establishments. Both pieces of evidence are consistent with firms avoiding new unions by shifting production from unionized establishments to other establishments. Finally, we find larger declines in employment and survival following elections when managers were more opposed to the union. To support this, we estimate treatment effect heterogeneity based on two proxies for managers' opposition: delays during the election process and the lack of other unionized establishments at the firm. Taken together, our results are consistent with firms' union avoidance tactics contributing to the overall negative effects of unionization.

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[†]Massachusetts Institute of Technology. Emails: swang1@mit.edu and sgyoung@mit.edu.

1 Introduction

Union elections in the U.S. are extremely contentious. Managers frequently threaten to close establishments if they unionize, and survey evidence suggests that some follow through on these threats (Bronfenbrenner, 1996).¹ The conventional economic explanation for establishments closing after unionization is that unions’ demands for wage increases or other workplace changes make the businesses unprofitable. This explanation, however, is not supported by existing research, which finds little evidence of successful union elections leading to higher wages or lower productivity.²

Consider two examples of how employers responded to unions that suggest alternative reasons why unionization may lead to establishment closure. First, during a 2017 campaign to unionize the news website Gothamist, the owner wrote to employees, “as long as it’s my money that’s paying for everything, I intend to be the one making the decisions.” One week after the workers voted to unionize, the owner shut down the business (Wamsley, 2017). This example suggests that some establishment closures could be driven by managers’ unwillingness to operate alongside unions or their general dislike of working with unions. Second, consider Boeing’s production of 787 airplanes. In 2011, Boeing shifted production of some 787s away from a unionized plant in Washington state to a non-union plant in South Carolina. In 2021, it produced all 787s in South Carolina (Cameron, 2020). According to a Boeing executive, the motivation “was not the wages we’re paying today. It was that we cannot afford to have a work stoppage, you know, every three years” (Greenhouse, 2011). This example illustrates how some firms can avoid working with unionized workers without eliminating their production. Additionally, the example suggests that conflictual labor relations between unions and firms may also lead to establishment closures. Both examples illustrate how unionization may lead to establishment closure even without direct wage or productivity effects.

This paper assesses whether these examples generalize by analyzing the effect of unionization on establishment employment and survival. We then examine whether firms’ ability to avoid working with new unions and managers’ general opposition to unions help explain the employment and survival effects. Our setting is around 27,000 U.S. private-sector union certification elections through the NLRB from 1981-2005. We link these elections to administrative Census data on establishment employment and survival from the Longitudinal Business Database (LBD) and total factor productivity from the Annual Survey of Manufactures (ASM). We supplement these data with union contract data from the Federal Mediation and Conciliation Service (FMCS).

We analyze these elections using a novel research design that extends standard difference-in-differences techniques with falsification tests from the regression discontinuity extrapolation literature. This strategy allows us to estimate treatment effects that include elections that win by large margins of support. Using this design, we find that unionization decreases establishments’

¹ Establishments are distinct locations where employees work. Firms are groups of establishments under the same ownership. Union elections are generally at the establishment level. Thus, a firm could have unionized and non-unionized establishments.

² Frandsen (2021); DiNardo and Lee (2004); Freeman and Kleiner (1990b) do not find wage increases following successful union elections. Additionally, Dube et al. (2016); Sojourner et al. (2015) find productivity *increases* following unionization, and the overall unions and productivity literature generally finds zero or *positive* effects (Freeman and Medoff, 1984). As exceptions, Knepper (2020) finds non-wage benefit increases and LaLonde et al. (1996) find productivity declines following unionization.

employment, primarily by lowering their likelihood of survival. We estimate a five-year effect on establishment survival of four percentage points (pct. pts.) relative to a survival rate of 82 % for establishments where the union lost. The overall employment declines are also bigger for larger margin-of-victory elections. Additionally, we document substantial effect heterogeneity across three broad industry groups: manufacturing, services, and other industries.³ In the service sector, where the majority of recent union organizing has occurred, we find much smaller and sometimes insignificant effects of unionization. Alternatively, the overall employment and survival declines are driven by large effects in manufacturing and “other industries.” For example, the ten-year effect on survival for manufacturing elections is eight pct. pts.

Motivated by these overall effects of unionization, we test whether firms’ ability to avoid working with new unions and managers’ opposition to unions help explain the effects. For this analysis, we focus on manufacturing elections because we have better data to test the specific parts of our hypothesis, and it is the largest sector with substantial negative employment effects.⁴

The first part of our hypothesis is that some firms can avoid working with new unions by shifting production from unionized establishments to other establishments.⁵ To test this, we first estimate whether the effects of unionization are larger at establishments part of multi-establishment (or multi-unit, MU) firms than single-establishment (SU) firms. We find significantly larger employment and survival decreases from elections at MU firms. For example, the ten-year effects on survival are twelve pct. pts. versus three pct. pts. at MU and SU firms, respectively. This heterogeneity is consistent with MU firms avoiding working with new unions by shifting production to other establishments. In contrast, SU firms need to either work with the union or shut down entirely.

Next, we more directly test for production shifting after successful elections. Specifically, following successful versus unsuccessful elections at MU firms, we compare the employment growth at the firms’ *other* establishments. Overall, we do not find different post-election employment growth between the firms’ *other* manufacturing establishments. Yet, these establishments may produce different products than the election establishment, making production shifting difficult. To test for production shifting between establishments that produce similar products, we focus on establishments in the same three-digit NAICS industry as the election establishment. With this restriction, we find significantly higher employment growth for the other establishments at firms with successful elections. However, these effects are insignificant five years after the election, at which point the firm may have shifted production to *new* establishments. Both pieces of evidence support firms avoiding unions through production shifting as one explanation for the impact of

³Examples of service-sector elections include hospitals, nursing homes, grocery stores, and janitors. The “other” industry elections include transportation, warehouse, and construction elections.

⁴The manufacturing data are better for two reasons. First, in manufacturing, the union avoidance method we test is shifting employment away from newly unionized establishments, which we can measure with the establishment and firm linkages. Second, we have high-quality establishment-level productivity measures that we use to test effect heterogeneity by baseline productivity.

⁵While employment shifting is the most prominent union avoidance tactic in manufacturing (see, e.g., [Bluestone and Harrison \(1982\)](#); [Verma \(1985\)](#); [Kochan et al. \(1986a\)](#)), other tactics may be more prominent in other industries. For example, [Hatton \(2014\)](#) document replacing unionized workers with independent contracts in several industries, and [Evans and Lewis \(1989\)](#) document construction firms opening separate non-union firms to avoid hiring unionized workers. We hypothesize that all of these tactics may explain the employment effects of unionization, but we only have the data to test for employment shifting.

unionization on employment and survival.

The second part of our hypothesis is that the effects of unionization are greater when management is more opposed to the union. To test this, we estimate treatment effect heterogeneity using two proxies for managers' opposition. First, we estimate effects separately for MU firms with and without any *other* unionized establishments. Survey evidence indicates that less unionized firms would more "vigorously resist dealing with unions," and managers' anti-union philosophies were often a key motivation for this opposition (Freedman, 1979; Foulkes, 1980). Additionally, similar to Selten (1978)'s "chain store paradox," non-unionized firms may aggressively resist the first unionization campaign to deter future attempts, even if not economically profitable when considering the attempt in isolation. Consistent with this evidence, we find significantly larger long-run employment and survival declines from successful elections at non-unionized firms than (partially) unionized firms.

Our second proxy for managers' opposition to the union is delays during the election process. Strategies that delay elections are a key way that managers attempt to influence elections and consequently a proxy for their opposition. For example, in "Confessions of a Union Buster," Levitt and Conrow (1993) write that the National Labor Relations Act "presents endless possibilities for delays, roadblocks, and maneuvers that can undermine a union's efforts" and that delay "steals momentum from a union-organizing drive." We define election delay as the time between the date the union filed for the election and the election date. We estimate separate treatment effects for elections with shorter and longer delays, and find significantly larger employment and survival decreases following longer delay elections. For example, the ten-year survival effect for MU elections in the top tercile of election delay times is 20 pct. pts. versus 7 pct. pts. for the bottom tercile.

Finally, we test for effect heterogeneity between establishments with different baseline productivity, which is implied by the conventional explanations for why unionization leads to establishment closures. Specifically, theoretical and empirical work from other contexts identifies productivity as a key determinant of establishment closure decisions. This suggests that wage increases or productivity declines from unionization should lead to larger survival effects for lower-productivity establishments.⁶ However, we do not find significant differences in the survival effects of unionization between establishments with different baseline total-factor productivity, measured using establishment-level input and output data from the ASM and Census of Manufacturers. Thus, this evidence is more consistent with alternative explanations for why unionization leads to establishment closures (e.g., our union avoidance hypothesis) than the conventional wage and productivity explanations.

There are several potential interpretations of our evidence on managers' opposition to unions and their use of union avoidance tactics. One interpretation is that the opposition and avoidance are motivated by a dislike of working with unions unrelated to their direct costs. This interpretation helps resolve the puzzles discussed earlier. First, existing research has not found that recent union elections have raised wages or negatively affected establishment productivity. Second, it is hard to rationalize unions making demands that lead to establishment closures, as this would directly

⁶See, for example, the theoretical and empirical literature on the reallocation effects of minimum wages (Berger et al., 2021; Dustmann et al., 2020) and the relationship between productivity and establishment exit (Foster et al., 2008).

harm their members (Friedman, 1951). Under the conventional explanations for closures, both points are difficult to reconcile with our large survival estimates. Yet, they are consistent with closures being driven by managers’ idiosyncratic dislike of working with unions.⁷ This interpretation is also consistent with our finding of no treatment effect heterogeneity by baseline establishment productivity. On the other hand, we cannot rule out that our proxies for manager opposition simply measure where unions would have been the costliest. Supporting this interpretation, there is research suggesting direct costs of recent union elections (e.g., Lee and Mas (2012)’s evidence of equity declines following successful elections). Overall, while our results do not measure the direct costs of unionization, they suggest that the employment and survival effects of unionization may be excessive relative to these costs.

We next summarize our econometric methodology and multiple falsification tests of our identifying assumption. We start by implementing a difference-in-differences design that compares outcomes before and after union elections at establishments where the union won versus lost. Our identifying assumption is that outcomes at establishments with different election vote shares but the same baseline characteristics would have followed parallel trends had no election occurred. To support this assumption, we first show that only conditioning on baseline employment and industry yields similar pre-election employment and payroll growth rates between establishments with winning and losing elections. This similarity holds when we add much richer baseline covariates and for up to ten years before the union elections. Additionally, we show that our treatment effects are increasing in the share of workers covered by the union and not driven by firm-level trends, which are consistent with our estimates being driven by unionization.

To further support our design, we assess several additional testable implications of our identifying assumption that are possible since we observe election vote shares. These checks extend tests from the regression discontinuity extrapolation literature to panel-data settings (Angrist and Rokkanen, 2015; Bennett, 2020). First, we show that the similarity in *pre-election* employment growth rates holds across the entire vote-share distribution in our sample. In other words, we test for trends in pre-election growth rates by vote share, which is a stronger test of “pre-trends.” Second, we show that establishments’ *post-election* employment growth and survival were similar between *losing elections* with different vote shares. If our treatment effects were driven by contemporaneous shocks correlated with vote shares, we would also expect different post-election outcomes between losing elections with different vote shares. Overall, these tests show that our identifying assumption holds for several sets of observations where we observe untreated potential outcomes.

Our empirical strategy combines features of regression discontinuity (RD) and panel data methods that have previously been used to analyze union elections.⁸ Although RD methods are appealing due to their internal validity, there are some disadvantages in this setting. First, there is

⁷As further support for this interpretation, survey evidence has not found that the firms most opposed to unions are also where unions are likely the costliest (Freedman, 1979; Bronfenbrenner, 2001).

⁸See DiNardo and Lee (2004); Sojourner et al. (2015); Knepper (2020); Bradley et al. (2017) for RD analyses and Freeman and Kleiner (1990b); LaLonde et al. (1996); Lee and Mas (2012); Dube et al. (2016); Goncalves (2021) for panel data analyses. Frandsen (2021) also combines these methods by implementing a regression discontinuity design on first differenced outcomes.

manipulation around the 50 % threshold that leads to *pre-election* discontinuities in establishment characteristics (Frandsen, 2017). Second, the effects of close union elections may be different than elections with larger margins of support. To address these issues, our paper expands the bandwidth to include all 20-80 % vote-share elections and uses the panel dimension to account for selection into winning versus losing elections. The wider bandwidth also gives us more power to estimate heterogeneous treatment effects. Relative to the other panel-data analyses, we better exploit observing the vote shares to implement tests of our identifying assumption.⁹ These tests could also be implemented in other DiD analyses where the “forcing variable” is observed (e.g., Ganong and Noel (2020); Harju et al. (2021)).

Our overall employment and survival estimates contribute to the literature on the effects of unionization in the U.S. Due to the different empirical strategies, our estimates complement Frandsen (2021)’s RD estimates of short-run decreases in establishment employment for close union elections and his suggestive evidence of negative survival effects.¹⁰ Our estimates also generalize other research finding employment declines following successful union elections in specific sectors (Sojourner et al., 2015; LaLonde et al., 1996).¹¹ As a consequence, our results contrast with the null effects of close union elections in DiNardo and Lee (2004) and other research finding no relationship between unions and business survival (Freeman and Kleiner, 1999). These differences are potentially due to our use of higher-quality establishment survival data. Finally, our evidence of larger employment declines from successful elections with larger vote shares mirrors Lee and Mas (2012)’s finding of larger stock market declines from larger vote-share elections.

Our evidence supporting the manager opposition and union avoidance hypothesis is novel relative to economic research on union elections but supports other research about firms’ responses to unionization. For example, Bronfenbrenner (2000, 2001) report similar results from a survey of union organizers in elections during the 1990s. She finds survival effects of twelve pct. pts. following successful elections. She also finds that establishment closing threats were more common at the types of elections where we find larger survival effects (e.g., in manufacturing and at MU firms). In addition, our evidence that managers more opposed to the union are more likely to shut down establishments after successful elections adds to the literature on anti-union firms’ broader union avoidance tactics (Freeman and Kleiner, 1990a; Kleiner, 2001; Flanagan, 2007). In particular, this result complements Ferguson (2008)’s finding that successful elections with unfair labor practice charges (another proxy for employers’ opposition) are less likely to reach a first contract. Finally, our production shifting evidence is consistent with firms becoming less unionized during this time through investing in and opening non-union establishments (Kochan et al., 1986a; Verma, 1985).

⁹Lee and Mas (2012) and Frandsen (2021) present pre-trends and post-election outcomes *across the vote-share distribution* but do not use these estimates as formal tests of their identifying assumptions.

¹⁰His survival estimates are differences in survival probabilities around the 50 % threshold and he writes that “a causal interpretation of the differences in survival probability should be made with caution” due to manipulation around the threshold.

¹¹LaLonde et al. (1996) analyze the employment and output effects of manufacturing union elections from 1977-1989 using a difference-in-differences design. Yet, their analysis differs from ours on several dimensions. First, they do not analyze the effect on establishment closures, which makes interpreting the results conditional on survival difficult. Second, due to their smaller sample size, their pre-trend estimates are often imprecise, making it difficult to assess the parallel trends assumption.

The rest of the paper is structured as follows. Section 2 describes the institutional details of NLRB union elections. Section 3 describes the election and Census data. Section 4 discusses our empirical strategy and tests of our identifying assumption. Section 5 presents our estimates of the overall effects of unionization on employment and survival. Section 6 provides multiple tests of our manager opposition and union avoidance hypotheses. Finally, Sections 7 and 8 discuss our results.

2 Unionization through NLRB Elections

The National Labor Relations Act (NLRA) guarantees most workers in the U.S. the right to collective bargaining and action. Under the NLRA, when a union represents a group of workers, their employer is required to bargain with the union over the conditions of employment.¹² This bargaining generally occurs at the establishment level (Traxler, 1994). During negotiations, the union may go on strike or the employer may “lockout” workers to pressure the other party. The NLRA also created the National Labor Relations Board (NLRB), a quasi-judicial agency that administers union elections and enforces unfair labor practice violations. Much of the current U.S. policy discussion around organized labor focuses on increasing representation at non-unionized establishments.¹³ Our results speak directly to the potential consequences of these efforts to increase unionization.

The primary way for private-sector workers to gain union representation is through a secret-ballot NLRB election. The organizing drive is initiated by workers at the establishment, either on their own initiative or prompted by outreach from a union. The first step is getting cards indicating union support signed by workers in the proposed “bargaining unit” (i.e., the workers the union would represent). The bargaining unit generally only contains workers at a single establishment. It can range from workers in a single occupation (e.g., delivery truck drivers) to all non-managerial workers. After gathering signatures from at least 30 % of the bargaining unit, the union files an election petition with the NLRB. The NLRB then confirms that the cards show sufficient support for the union, resolves any disagreements over the composition of the bargaining unit, and schedules the election. After the petition is filed, employers frequently attempt to delay the election to reduce union support (e.g., contesting the composition of the bargaining unit) (Levitt and Conrow, 1993).

Before the election, the union and employer often actively campaign for and against union representation. Union organizers and pro-union workers can campaign by (1) speaking with other workers at work or during “house calls,” (2) publicly showing solidarity among union supporters (e.g., rallies or wearing pro-union attire), or (3) enlisting the support of community groups (Bronfenbrenner and Juravich, 1998). Employers also have many campaign tools at their disposal, including one-on-one meetings with supervisors and “captive audience meetings,” where employees are required to attend. Employers also frequently hire “union avoidance” consultants and law firms (Logan, 2002). Finally, although there are legal restrictions on firing pro-union workers and threatening to close

¹²The goal of these negotiations is a contract. Contracts commonly specify wage and non-wage compensation for each job title, grievance procedures for disputes, policies for implementing layoffs, and promotion policies (Slichter et al., 1960).

¹³For example, the currently debated *Protecting the Right to Organize (PRO) Act of 2021* would limit employers’ ability to campaign against union elections and increase penalties for unfair labor practices during elections.

establishments, these tactics still occur (Weiler, 1983; Schmitt and Zipperer, 2009). If a majority of workers vote for the union, the union is certified by the NLRB to represent the bargaining unit. After the union is certified, the employer is required to bargain “in good faith” with the union. But the parties are not required to reach an agreement.¹⁴ If a contract is not reached one year after certification, employees can vote out the union by holding a *decertification* election.

NLRB elections are the primary method for private-sector workers at an establishment to gain union representation. However, there are two reasons why some unionization occurs without an election. First, the NLRA does not cover all workers (General Accounting Office, 2002).¹⁵ Second, workers covered by the NLRA can gain union representation without an election through voluntary “card check” recognition. However, card check is much less common than elections.¹⁶

Selection into Union Elections and the Determinants of Winning Elections

Since our empirical design compares winning and losing elections with similar baseline characteristics, it is helpful to review the literature on selection into holding and winning elections. This literature motivates which baseline characteristics we condition on and our additions tests of whether election vote shares are related to remaining unobservable shocks. For selection *into* elections, Dinlersoz et al. (2017) find that elections are more likely at larger, more productive, and younger establishments. We account for this selection by only comparing establishments that hold elections.

For election outcomes, workers, employers, and other factors could all influence whether the union wins. For our empirical strategy, a concern is that vote shares may be related to future establishment productivity changes. For example, workers who expect their establishment to become more productive and have more rents to share may be more likely to vote for a union. This would generate a positive bias between vote shares and establishment growth. Alternatively, firms that expect to become more productive may campaign harder against unions, leading to a negative bias.

Research on election outcomes finds that these factors all play some role. The most consistent finding is higher union win rates for smaller bargaining units (Heneman and Sandver, 1983; Farber, 2001). Win rates also vary substantially across industries (Bronfenbrenner, 2002). In the 2000s, the win rate in manufacturing was around 40 % versus 60 % for services. These factors motivate our first specification that just conditions on establishments’ baseline industry and employment. In terms of the influence of employer versus union campaigns, Bronfenbrenner (1997) finds that “union tactic variables explain more of the variance in election outcomes than any other group,” including employer tactics or characteristics. Yet, other research finds that the strength of firms’ anti-union campaigns is associated with lower win rates (Freeman and Medoff, 1984). To address the concern that firms’ anti-union campaigns lead to a negative bias between vote shares and establishment

¹⁴In a review, CRS (2013) find that 56-85 % of successful elections result in first contracts during the period we consider.

¹⁵Some workers lack collective bargaining rights (e.g., some small business employees, independent contractors, domestic workers, and “agricultural laborers”). Other workers have collective bargaining rights but are not covered by the NLRA. For example, airline and railroad employees’ collective bargaining rights are covered by the *Railway Labor Act*. Similarly, public-sector workers’ bargaining rights are covered by various federal, state, and local statutes.

¹⁶Schmitt and Zipperer (2009) estimate that from 1998-2003, 60 % of workers were organized through NLRB elections but assume that before then 90 % of organizing occurred through elections.

growth, we implement multiple tests of how vote shares are related to firm productivity shocks. Additionally, past research has found that winning versus losing elections have similar pre-election productivity trends. For example, [Dube et al. \(2016\)](#) find similar productivity pre-trends for nursing home elections, and [Lee and Mas \(2012\)](#) find similar stock-market trends, which is a stronger test since it incorporates expectations of future productivity growth.

Motivation for Estimating the Effects of Larger Margin-of-Support Elections

An advantage of our empirical strategy is that it does not rely on comparing only elections that barely won or lost. One motivation for this is evidence of non-random sorting of elections just around the 50 % threshold (i.e., “vote-share manipulation”). Figure 2 Panel A plots the vote-share distribution for the elections in our sample and shows manipulation around the 50 % threshold previously documented by [Frandsen \(2017\)](#) (e.g., a missing mass of elections that barely win).¹⁷ [Frandsen \(2021\)](#) also documents that this manipulation leads to large differences in observable establishment characteristics across the threshold (e.g., 13-22 % differences in employment).

Another motivation for our empirical strategy is that the treatment effect of unionization may depend on the election vote share. For example, [Lee and Mas \(2012\)](#) find that the negative stock market effects of unionization are larger for higher margin-of-victory elections. One potential reason for this heterogeneity is that close union elections are often followed by lengthy delays before bargaining begins (e.g., debates about challenged votes). Figure 2 Panel B shows this by plotting the average number of days between the election date and the case closing date (e.g., when the union is officially certified). The figure shows a striking increase in this delay time for close elections (e.g., the median (mean) for elections that barely win is around 118 (223) days versus only 11 (57) days for 60 % vote-share elections). Since delays can dampen the unions’ bargaining power, this evidence suggests that the effects of close elections may be different than higher vote-share elections.

Second, for close elections, firms may delay the bargaining process anticipating a future decertification election. Figure 2 Panel C provides evidence that close elections are more likely to be decertified by plotting the probability of each certification election experiencing a *decertification* election in the five years following the original election. It shows that more than twelve percent of very close winning elections experience a future decertification election compared to less than five percent of larger margin-of-victory elections. This suggests that higher margin-of-victory elections may be more likely to reach first contracts, leading to more changes at the establishment. A final reason why the treatment effect of unionization may vary by vote share is that unions that win with more support may be able to more credibly threaten to strike. Figure 2 Panel D supports this by showing that within manufacturing, where strikes were more common, the probability of a post-election works stoppage increases in the election vote share (see Appendix C for details). Overall, these results show that several proxies for the unions’ bargaining power increase in the

¹⁷It is difficult to see manipulation in this figure because of the *discrete* running variable and since our sample includes elections small numbers of votes. Consequently, we plot elections with exactly 50 % of votes separately to make the manipulation easier to see. [Frandsen \(2017\)](#) finds evidence of manipulation using formal tests that accommodate discrete running variables. Additionally, Figure A2 plots vote-share density for elections with more than 50 votes, where it is clearer to see manipulation.

election vote share, suggesting that the effects of unionization may also differ along this margin.

3 Election, Contract, and Establishment Data

For our analysis, we combine union election and contract data with administrative establishment-level data from the U.S. Census Bureau. These data are uniquely suited to study union elections. First, the data contain the universe of *establishments*, the level at which most elections are held. Analysis of more aggregated data would include establishments not directly affected by the elections and attenuate the effects of unionization. Second, the Census constructs high-quality longitudinal establishment linkages that allow us to separate real establishment exit from spurious exit due to administrative reasons or ownership changes (Haltiwanger et al., 2013). These links are important for our analysis because survival is a key outcome of interest. Finally, the rich establishment covariates allow us to compare similar winning and losing elections (e.g., same size, age, and industry).

NLRB Union Election Data We combine data from multiple sources to construct a comprehensive dataset of union elections from 1962 to 2018. Specifically, we use datasets assembled by Henry Farber, J.P. Ferguson, and Thomas Holmes and public data from the NLRB.¹⁸ The data contain election vote counts that we use to define treatment. Additionally, they include employers’ names and addresses that we use to match elections to Census data. Finally, the data include the election petition filing date, the actual election date, and the closing date. We define our treatment time based on the filing date of each election because this is the earliest date we observe for each election. We also use these dates to define the time between filing the election petition and holding the election, a proxy for managements’ opposition to the elections described further in Section 6.

FMCS Contract Notice Data To measure whether an establishment is covered by any collective bargaining agreement, we use contract notice data from the Federal Mediation and Conciliation Service (FMCS) from 1984-2019. We combine data from Thomas Holmes and the FMCS. The data include both notices of *initial contracts* (i.e., first-contract negotiation after an election) and *contract renegotiation or reopening* for existing contracts. These “notices of bargaining” are provided to the FMCS so it can be ready to provide mediation. Although filing is legally incentivized, underreporting is possible. These data also include names and addresses for matching. We use these data to measure whether an election establishment has any other workers covered by a collective bargaining agreement and whether the election establishment’s firm has any other unionized establishments.

Employment, Payroll, and Survival Data from the LBD Our primary source of establishment-level outcomes is the Longitudinal Business Database (LBD). It contains annual employment and payroll for the universe of non-farm, private sector establishments from 1976-2015 (Jarmin and Miranda, 2002). Our employment measure is the total number of employees in March of each year.

¹⁸For duplicates across datasets, we pick one observation for each NLRB case number (see Appendix C for details). Appendix Figure A1 shows that this yields a similar number of cases each year to the number of cases from the NLRB’s annual reports.

The payroll measure is employees’ total “wages, tips, and other compensation” over the entire year. Consequently, we would expect larger effects on “event-time 0” payroll than employment. The data also contain high-quality longitudinal establishment IDs that identify the same establishments over time, even across ownership changes. We use these IDs to define establishment survival. Specifically, we define survival based on the last year the establishment has non-zero employment. Finally, we use the [Fort and Klimek \(2016\)](#) 2012 NAICS codes to classify each establishment into consistent industries across the entire time period.

We address potential biases from how the Census defines employment at establishments part of *multi-establishment (MU) firms* by focusing on longer-run outcomes. In particular, although the LBD is at the establishment level, some of the annual employment and payroll data are received at higher levels of aggregation. These aggregate measures are initially allocated proportionately across establishments based on their previous employment. Consequently, if a unionized establishment at an MU firm shrinks, some of this decrease in employment may be initially allocated to the firm’s other establishments, creating a short-run underestimate of the effect of unionization. To avoid these allocation issues biasing our results, we focus on longer-run outcomes (e.g., five to ten years after the elections) since the Census receives establishment-level employment measures at least every five years (see [Appendix C](#) for details).

Sample Selection and Matching Elections to Census Establishments Before matching the election data to the Census data, we impose sample restrictions to focus on elections likely to shift an establishment’s union status. [Appendix Table A1](#) shows how these restrictions affect the number of elections and eligible voters in our final sample. First, we restrict the sample to elections held between 1981-2005. Since the LBD starts in 1976 and ends in 2015, this gives us a five-year pre-period and ten-year post-period for all elections. Second, we drop non-representation election cases (e.g., decertification elections). Third, we drop contested elections, which are elections with multiple unions on the ballot. These elections often involve incumbent unions (e.g., “union raids”) and consequently may not be associated with changes in union representation ([Sandver and Ready, 1998](#)). Fourth, we drop elections with fewer than six workers in the bargaining unit to ensure that the election could lead to a non-trivial increase in union representation.

After these sample restrictions, we implement a name and address matching procedure to link each election to a unique establishment in the LBD (our strategy is similar to [Kline et al. \(2019\)](#)). We match each election to the universe of LBD establishments by calculating a weighted average of the *Soft TF-IDF* distance between employer names and the geographic distance between geocoded addresses. We match each election to the Census establishment with the highest match score above a minimum threshold. This procedure yields a match for 70 % of elections.¹⁹ We also apply the same procedure for each FMCS contract notice. See [Appendix C](#) for details on our matching algorithm.

We further restrict the election sample based on the requirements of our empirical strategy. For each establishment, we only keep the first election. As discussed in [Section 4](#), this means

¹⁹Although matching introduces measurement error in our binary treatment variable, such measurement error should bias us against finding effects of unionization (see e.g., [Card \(1996\)](#) for measurement error in *individual-level* union status).

that our estimates should be interpreted as the effects of winning the *first* union election at an establishment. Next, we drop elections at establishments less than three years old. Since a key test of the identifying assumption is that the outcomes for winning and losing elections evolved similarly before the election, we do not want to include observations where we cannot evaluate this for at least three time periods. Finally, to keep our sample the same across model specifications, we require that each observation have non-zero payroll and employment one year before the election. These restrictions result in an overall sample of approximately 27,000 elections (see Appendix Table A1).

Finally, for much of our analysis, we restrict the sample to 20-80 % vote-share elections. Appendix Table A1 shows that this decreases our sample to 19,000 elections. The motivation for this restriction is that some of the tests of our identifying assumption discussed in Section 4 fail for the extreme vote-share elections. To assuage concerns that this choice of bandwidth drives our results, we show that our main results are robust to instead including a 30-70 % bandwidth.²⁰

Table 1 presents summary statistics for winning and losing union elections in our sample. The estimates confirm the patterns of selection into winning elections described in Section 2. In particular, we find that winning elections are at establishments that are, on average, smaller, less likely to be part of multi-establishment firms, and more likely to already have another unionized bargaining unit. The differences, however, are less striking for workers' average wages or establishment age.

4 Empirical Strategy and Identifying Assumptions

Our research design combines standard difference-in-differences (DiD) techniques with tests of our main identifying assumption from the regression discontinuity extrapolation literature. Our identifying assumption is a conditional parallel trends assumption between elections with different vote shares. Since we observe vote shares that determine treatment assignment, we can assess several testable implications of this assumption that are not possible in a standard DiD setting.

Potential Outcomes To fix ideas, consider establishments, i , that held an election in one year, E_i (e.g., all elections in 1995). We refer to these elections as *cohort* E_i . Treatment at time t , D_{it} , is defined as both holding an election and the union receiving a vote share, V_i , of more than 50 %²¹

$$D_{it} = \mathbb{1}[V_i > .5 \text{ \& } t \geq E_i]. \quad (1)$$

An establishment's non-unionized potential outcome is Y_{it}^0 . Its unionized potential outcome is $Y_{it}^E(V)$ which depends on its cohort E and election vote share V . This allows for dynamic treatment effects and heterogeneous treatment effects by vote share, respectively. We assume *no anticipation* before

²⁰Specifically, our vote-share heterogeneity estimates in Figure 6 show that the overall estimates are not driven by the 20-30 or 70-80 % elections. Additionally, Tables A4, A5, and A6 present the heterogeneity estimates with a 30-70 % bandwidth and show that the results are qualitatively the same although sometimes less precise than with the wider bandwidth.

²¹This definition assumes that treatment is absorbing (i.e., $D_{it} = 1 \Rightarrow D_{it'} = 1 \forall t' > t$). This assumption ignores that workers may lose union representation through a *decertification* election. Additionally, after losing an election, unions may hold another election. Since we only include the first election at each establishment, we interpret treatment as the dynamic effects of winning a *first union election* which does not correspond one-to-one with union representation or having a contract.

the year of the election (i.e., $Y_{it}^E(V) = Y_{it}^0$ for all $t < E_i$). Observed outcomes are thus²²

$$Y_{it} = Y_{it}^0 + D_{it} \left(Y_{it}^{E_i}(V_i) - Y_{it}^0 \right). \quad (2)$$

Our estimand of interest is the treatment effect n years after a successful election with vote share V

$$\delta_n(V) = E \left[Y_{it}^{E_i}(V_i) - Y_{it}^0 \middle| V_i = V \ \& \ t - E_i = n \right]. \quad (3)$$

DiD Specifications For a single cohort, we can estimate the following specification

$$Y_{it} = \gamma_i + \alpha_t + \sum_n \delta_n \cdot \mathbb{1}[t - E_i = n] \times \mathbb{1}[V_i > .5] + X_i' \beta_n + \varepsilon_{it} \quad (4)$$

where γ_i are establishment fixed effects (FEs) and α_t are year FEs.²³ The coefficients of interest, δ_n , capture the average, dynamic treatment effects of a successful union election. X_i are baseline, one year before the election, establishment characteristics whose coefficients vary with event time n (i.e., flexible trends by baseline establishment size).

Identifying Assumption Our identifying assumption conditional parallel trends *by vote share*. Specifically, we assume that outcomes at establishments with different election vote shares but the same baseline characteristics would have followed parallel trends had no election occurred

$$E \left[Y_{it}^0 - Y_{it-1}^0 \middle| X_i, V_i \right] = E \left[Y_{it}^0 - Y_{it-1}^0 \middle| X_i \right]. \quad (5)$$

There are several things to note about this assumption. First, it does not restrict selection *into* union elections (e.g., organizers targeting productive establishments) or selection *on gains* based on the effects of unionization (e.g., workers only voting for effective unions). Second, the assumption is stronger than the standard DiD assumption because it requires parallel trends by vote share instead of only, on average, between the treated and control observations. Yet, this stronger assumption yields a richer set of testable implications discussed next. Third, the assumption imposes a functional form restriction on potential outcomes (Kahn-Lang and Lang, 2020; Roth and Sant’Anna, 2021), which we discuss for each specific outcome later. Finally, as discussed in Section 2, vote shares are influenced by workers, employers, and other factors that could lead to violations of this assumption. This possibility motivates our conditioning on particular baseline X_i s and assessing multiple testable implications of this assumption to provide reassurance that such selection is not biasing our results.

Our empirical strategy also addresses the concern that vote-share manipulation around the 50 % threshold could violate assumption 5 because elections just around the threshold are only a

²²Here, we assume that losing elections have no causal effect. This assumption is stronger than what we make in our empirical approach since we cannot disentangle the effect of losing an election from the selection into holding an election. We make this assumption for simplicity, but we could also index losing election potential outcomes by cohort to relax the assumption.

²³We exclude establishment FEs for outcomes that are identical for all establishments in the baseline year, $t - E_i = -1$ (e.g., establishment survival and DHS growth rates). We include them for log outcomes. See the outcome discussion for details.

small share of our overall sample. For example, our vote-share heterogeneity estimates show that excluding elections right around the 50 % threshold would not qualitatively change our results.

Testable Implications of the Identifying Assumption Our identifying assumption yields several testable implications. The intuition for these tests is that we observe Y_{it}^0 for many observations and can test whether equation 5 holds for different subsets of these observations.

The first testable implication is that, if equation 5 holds, there should be conditional parallel trends in **pre-election** outcomes across **all vote shares**

$$E[Y_{it} - Y_{it-1}|X_i, V_i] = E[Y_{it} - Y_{it-1}|X_i] \text{ for all } t < E_i. \quad (6)$$

This test nests the standard DiD pre-trends test between all winning versus losing elections. Moreover, we can test for similar pre-trends between finer vote-share groups. For example, we can estimate whether establishments where the union won by different margins of victory grew at different rates before the election by comparing pre-trend estimates for 50-60 % versus 60-70 % elections. This test mirrors the tests proposed by Angrist and Rokkanen (2015) and Bennett (2020) for regression discontinuity identification away from the threshold. They argue that conditional mean independence of potential outcomes and the running variable for a given bandwidth around the RD threshold is strong support for being able to estimate treatment effects within that bandwidth. One reason that we only include 20-80 % vote-share elections in our preferred specification is that for some outcomes, we find violations of equation 6 for extreme parts of the vote-share distribution.

The second testable implication is that there should be conditional parallel trends in **post-election outcomes** between **losing elections** with different vote shares

$$E[Y_{it} - Y_{it-1}|X_i, V_i] = E[Y_{it} - Y_{it-1}|X_i] \text{ for all } t \geq E_i \text{ \& } V_i \leq .5. \quad (7)$$

To implement this test, we can estimate whether post-election outcomes are different between losing elections with different vote shares (e.g., compare conditional post-election survival rates for 30-40 % versus 40-50 % elections). This test gives us one way to address the concern that election vote shares are correlated with future productivity shocks. If this were the case, we would also expect these shocks to cause differences between the outcomes at losing elections with different vote shares.²⁴

Figure 1 illustrates our identifying assumption and these testable implications. It plots average outcomes two years before the election, $Y_{i,-2}$ and $Y_{i,-1}$, and one year afterward, $Y_{i,1}$, by vote share. Testing parallel *pre-trends* by vote share corresponds to comparing the distance between $Y_{i,-2}$ and $Y_{i,-1}$. Likewise, testing parallel *post-trends for losing elections* corresponds to comparing the distance between $Y_{i,-1}$ and $Y_{i,1}$ for losing elections.

²⁴This test also allows us to evaluate one version of the “union threat” hypothesis. In particular, it allows us to test whether losing a union election by a small margin of victory affects an establishment differently than losing by a larger margin. This test, however, would not capture across-the-board union threat effects that don’t vary by vote shares.

Estimating Effects for Multiple Cohorts Our sample includes all election cohorts from 1981-2005. To estimate the effect across all cohorts, we pool these elections and estimate

$$Y_{it} = \gamma_i + \alpha_{t,E_i} + \sum_n \delta_n \cdot \mathbb{1}[t - E_i = n] \times \mathbb{1}[V_i > .5] + X_i' \beta_{n,E_i} + \varepsilon_{it}. \quad (8)$$

This specification is the same as the single-cohort specification in equation 4, except that the year FEs and baseline controls can now vary by cohort (i.e., α_{t,E_i} and β_{n,E_i} have E_i subscripts). The motivation for this flexibility is that with cohort-specific controls, our estimates are the same as estimating δ_n cohort-by-cohort except we use regression weights to aggregate the estimates. Consequently, there are two differences between our setting and the standard “staggered adoption” DiD setting. First, we avoid the potential negative weight issues that arise from heterogeneous, cohort-specific treatment effects (Sun and Abraham, 2020; Goodman-Bacon, 2021; de Chaisemartin and D’Haultfœuille, 2020).²⁵ Second, we only need to assume that our identifying assumption in equation 5 holds *within each cohort*.²⁶ Both differences are because our estimates come from comparing winning and losing elections *within the same cohort* rather than across cohorts which might lead to negative weights or alternative parallel trend assumptions. Finally, we cluster standard errors at the *firm* level.²⁷

Establishment-Level Controls To account for observable determinants of election outcomes, we control for progressively richer establishment-level characteristics. All controls are from one year before the election and interacted with event time. The event-time interaction allows for flexible pre- and post-election trends by baseline characteristics (e.g., differential employment growth rates for large versus small establishments). Our first *industry and employment controls* specification includes baseline employment and three-digit NAICS industry-by-year controls.²⁸ The motivation for starting with these covariates is that they are among the strongest predictors of union election victory (see Section 2), and they are key determinants of establishment growth and survival dynamics (Dunne et al., 1989; Haltiwanger et al., 2013). Next, we add other characteristics in the LBD (baseline payroll, establishment age, and single/multi-establishment status) and an indicator for whether we observe a previous FMCS contract at the establishment (i.e., another bargaining unit already unionized at the establishment).²⁹ We refer to this specification as the *pooled controls* specification.

²⁵We test for negative weights on each cohort treatment effect using Sun and Abraham (2020)’s `eventstudyweights` package.

²⁶With multiple cohorts, our identifying assumption is $E[Y_{it}^0 - Y_{it-1}^0 | X_i, E_i, V_i] = E[Y_{it}^0 - Y_{it-1}^0 | X_i, E_i]$. Thus, we do not require that selection into elections in the 1980s is the same as selection into elections in the 2000s.

²⁷This accounts for serially correlated establishment-level outcomes across time and across elections at different *establishments* within the same *firm*. Our regression weighting to aggregate the δ_n estimates easily accommodates this level of clustering.

²⁸Our baseline specification interacts industry by year and event time because some of our outcomes are cumulative measures (e.g., the DHS growth rates and survival). Thus, just industry-by-year FEs would capture industry growth rates over different time horizons. For all continuous variables, we flexibly parameterize their functional form with *decile* fixed effects.

²⁹The motivation for including the previous contract control is that union elections are more successful when other workers at the same establishment are already unionized (Bronfenbrenner, 2002). The selection into such elections may also differ from the selection into elections for an establishment’s first bargaining unit. When we pool all industries together, we interact controls in this specification with three coarse industry groups (e.g., manufacturing, services, and “other”). This keeps them at the same level of granularity for our overall and manufacturing estimates.

Finally, we interact all controls from the previous specification with cohort (i.e., year of election). This is our preferred *flexible controls* specification. The cohort interactions result in the within-cohort identification assumption discussed previously. We show, however, that our main results are robust to pooling controls across cohorts or only including the employment and industry controls.

Establishment-Level Outcomes The first outcome we consider is the [Davis, Haltwanger and Schuh \(1996\)](#) (DHS) symmetric growth rate for employment and payroll

$$G_{it} = 2 \times \frac{Y_{i,t} - Y_{i,t=E_i-1}}{Y_{i,t} + Y_{i,t=E_i-1}}. \quad (9)$$

This growth rate is a second-order approximation of the log difference from time t to one year before the union election, $E_i - 1$. Yet, it accommodates establishment exit as G_{it} equals -2 for establishments that do not exist (i.e., have zero employment).³⁰ Consequently, a -0.2 value of G_{it} could represent either an approximately 20 % decline in intensive margin employment with no survival effects or a 10 percentage point decrease in the likelihood of survival. Since the growth rate accommodates exit, we can simultaneously evaluate pre-trends and interpret treatment effects, even if unionization affects establishment survival which could lead to a selected group of survivors. For this reason, the DHS growth rate is commonly used to analyze firm growth dynamics.³¹

To estimate the effect of unionization on extensive margin employment growth, we include establishment survival as an outcome (an indicator for whether the establishment exists at time t). We can compare the survival effect to the DHS growth rate effect to answer how much of the DHS growth rate effect is *mechanically* due to exit (e.g., $G_{it} = -0.2$ could be completely explained by a 10 pct. pt. decrease in survival). However, the residual, the part of G_{it} unexplained by exit, could be either intensive-margin employment changes or selective exit based on employment growth rates.

Finally, we define the outcome as log employment or log payroll. A challenge with interpreting the effects on log outcomes is that treatment effects on establishment survival can bias comparisons of potentially selected *survivors*. The pre-trends for these log outcomes, however, are a useful complement to the DHS growth rate pre-trends.³² For interpreting the treatment effects on log outcomes, we provide two ways of partially alleviating the selective survival concern. First, all specifications with log outcomes include establishment FEs that account for *level* differences between the surviving and exiting establishments.³³ Second, for some results, the timing of the log outcome versus survival effects suggests intensive margin effects (e.g., large effects on log outcomes before any substantial survival effects). Yet, we still recommend interpreting the treatment effects for log outcomes with caution since we cannot completely eliminate potential bias from selective survival.

³⁰Conventionally, the growth rate is defined annually (e.g., from $t - 1$ to t) but we define it over longer time-horizons to measure cumulative changes. Additionally, since our sample restrictions impose non-zero employment at $t = E_i - 1$, G_{it} is never equal to 2 which it usually equals for entrants. Establishments that do not exist at time t before the election have $G_{it} = -2$.

³¹See [Haltiwanger et al. \(2013\)](#); [Chodorow-Reich \(2014\)](#) for general use and [Arnold \(2019\)](#); [Davis et al. \(2014\)](#) for DiD contexts.

³²The DHS pre-trends combine intensive and extensive margin employment changes. However, in specifications where we control for baseline establishment age, the DHS pre-trends will closely approximate pre-trends for log outcomes.

³³For DHS growth rates and survival, we do not include establishment FEs. For DHS growth rates we capture the time-invariant component by differencing relative to $t = E_i - 1$. For survival, it is unclear what time-invariant characteristic FEs would capture.

For our outcomes, we make related parallel trends functional form assumptions. For log outcomes, we assume that log employment and payroll would have (conditionally) evolved in parallel, which we view as a reasonable restriction in this setting.³⁴ Additionally, we can test whether the restriction holds in the pre-period (i.e., equation 6). For establishment survival, we assume that the survival probabilities between elections with different vote shares would have (conditionally) been equal had no election occurred at the establishments. We cannot test whether this assumption holds in the pre-period since all establishments exist at event-time zero. However, we can test whether this functional form assumption holds between the losing elections with different vote shares (i.e., equation 7). For the DHS growth rate, the outcome is approximately a linear combination of log employment changes and survival probabilities, so the two functional form assumptions we have already made imply parallel trends in the DHS growth rate.³⁵

5 Empirical Results: Overall Employment and Survival Effects

In this section, we estimate the effects of successful union elections on establishment employment and survival. We first analyze the differences in employment growth rates between establishments with winning and losing elections. Next, we implement several tests of our parallel trends identifying assumption described in Section 4. Finally, since we later focus on manufacturing, we present our estimates and falsification checks separately for all industries and just for elections in manufacturing.

Overall Employment and Survival Estimates

We start by estimating establishment employment growth for successful versus unsuccessful elections. Figure 3 plots the δ_n coefficients from estimating the “pooled cohort” specification in equation 8 for elections with 20-80 % vote shares. Panel A. plots the estimates for DHS employment growth relative to one year before the election. Panel B. includes log employment as the outcome. Both panels include estimates with no controls (i.e., only year by cohort FEs), the industry and employment controls, and the flexible control specification described in Section 4.

The estimates in Panels A. and B. show that establishments with successful elections had similar conditional pre-election growth rates to establishments with unsuccessful elections but experienced large relative employment decreases following the election. The first, “no control” estimates, however, show that, without any controls, establishments where the union won had relatively slower pre-election employment growth rates than establishments where the union lost.³⁶ However, the next “industry + emp ctrls.” estimates show that just conditioning on baseline

³⁴For example, consider two firms with the same Cobb Douglas production function parameters but different baseline TFP and/or input and output prices. In response to the same demand shock (e.g., the same proportional change in the price of output), their log payroll and log employment would both evolve in parallel while their levels would diverge.

³⁵Specifically, we assume $E[\Delta \ln Y_{it}^0 | X_i, V_i] = E[\Delta \ln Y_{it}^0 | X_i]$ and $E[\mathbb{1}[Y_{i,t}^0 = 0] | X_i, V_i] = E[\mathbb{1}[Y_{i,t}^0 = 0] | X_i]$ which imply $E[G_{it}^0 | X_i, V_i] \approx E[G_{it}^0 | X_i]$.

³⁶Without any controls, the DHS growth rates and log employment pre-trends measure somewhat different growth rates. The DHS employment growth rates combine intensive and extensive margin changes, while log employment only captures intensive margin changes. The measures are approximately the same in the control specifications that include establishment age.

employment and industry yields similar pre-election growth rates for DHS and log employment. As discussed in Section 4, we start with these controls because they are prominent predictors of election outcomes and establishment employment growth. Starting one year after the election, this specification also shows decreased employment for establishments with successful union elections. The effects stabilize approximately three years after the election. Finally, the results from the third “flexible control” specification show that our pre- and post-election employment growth estimates are very similar adding the richer and more flexible establishment-level controls. In the next section, we show that our other “vote-share heterogeneity” tests of our identification assumption also yield similar estimates with just the industry and employment versus flexible control specifications.

To help interpret the magnitude and timing of the employment effects, Panel C. in Figure 3 additionally plots payroll and establishment survival estimates. Specifically, it includes estimates of DHS employment and payroll growth and establishment survival with the flexible control specification. We find that establishment payroll initially declines faster than employment. This difference could be due to either compositional shifts to low-wage workers or differences in the timing of the payroll versus employment measures described in Section 3. Five years after a successful union election, the cumulative DHS employment and payroll growth rates are -0.13 and -0.14 lower, respectively, than establishments with unsuccessful elections (consistent with a 14 % decrease in payroll or a seven pct. pt. decrease in survival likelihood). Appendix Figure A3 presents estimates from the same specification for *log* employment and payroll. These estimates allow us to reject five-year, pre-election growth rate differences of more than 3.5 % for employment and 1.8 % for payroll. Unlike the DHS measures, we find larger five-year log payroll than employment declines. Although this evidence would be consistent with long-run compositional changes, we recommend interpreting it cautiously given potential biases from selective exit.

The survival estimates in Panel C. of Figure 3 indicate that most of the decrease in DHS employment and payroll growth rates is from a lower likelihood of establishment survival. To decompose what share of the DHS effects is from survival effects, we plot the survival estimates on a separate y-axis scaled to be one-half the DHS growth rate axis. Comparing the exit and DHS coefficients illustrates how much of the DHS effect can be mechanically explained by the survival effect (see Section 4). The estimates show that five years after an election, establishments with successful elections are four pct. pts. less likely to survive, and this effect increases slightly to five pct. pts. after ten years. Consequently, about two-thirds of the -0.13 five-year DHS employment growth rate estimate is mechanically due to decreased establishment survival. Finally, the relatively slower timing of the survival versus employment effects is consistent with an increased legal risk of immediately closing an establishment following an election. For example, [Munger et al. \(1988\)](#) describe how a short time between an election and establishment closure could be used as evidence that the closure is an unfair labor practice due to its “intent to chill unionism” across an entire firm.

Given our later focus on manufacturing, Figure 4 presents the same estimates including only manufacturing elections. For these elections, we find similar pre-election employment growth rates (i.e., a lack of pre-trends) even without baseline industry and employment controls. For example,

Panel B. shows that, without any controls, we can rule out five-year employment growth rate differences of more than five percent. One explanation for the lack of detectable pre-trends without controls is that by only comparing elections in manufacturing, we may account for sector and employment differences that the controls capture when we include all industries. Additionally, for manufacturing elections, the magnitude of the treatment effects is larger than the effects for all industries (e.g., the five-year DHS employment estimates are -0.17 versus -0.13, and the five-year survival effects are -0.05 versus -0.04, respectively). We show later that this difference is because the effects of unionization in the service sector are much smaller.

Vote-Share Heterogeneity Tests of Identifying Assumption

We next provide further evidence that our results are driven by unionization by assessing several testable implications of our identifying assumption. Additionally, we estimate treatment effect heterogeneity by the unions' margin of support. To implement these tests, we first present visual evidence of how treatment effects and pre-election trends vary across the vote-share distribution and then implement parametric tests of linear trends in establishment outcomes by election vote shares.

Nonparametric Vote-Share Heterogeneity To estimate pre-trends and treatment effects for different parts of the vote-share distribution, we estimate the following modified version of our main DiD specification

$$Y_{it} = \alpha_{t,E_i} + \sum_g \sum_n \delta_{g,n} \cdot \mathbb{1}[t - E_i = n] \times \mathbb{1}[V_i \in \mathcal{V}^g] + X_i' \beta_{n,E_i} + \varepsilon_{it} \quad (10)$$

where \mathcal{V}^g are exhaustive subsets of the vote-share distribution.³⁷ We partition the vote-share distribution into eight groups (0 – 20 %, 20 – 30 %, 30 – 40 %, 40 – 50 %, 50 – 60 %, 60 – 70 %, 70 – 80 %, and 80 – 100 %). We omit the 20 – 30 % group, so the estimates for each group are relative to outcomes for 20 – 30 % vote-share elections. This specification allows us to assess the two testable implications of our identifying assumption described in Section 4. First, we test whether establishments' *pre-election* outcomes are similar across the vote-share distribution by comparing $\delta_{g,n}$ estimates for $n < 0$ (i.e., testing equation 6). Second, we test whether post-election outcomes differ between losing elections with different vote shares by comparing $\delta_{g,n}$ estimates for $n > 0$ and $V_i \leq .5$ (i.e., testing equation 7). For presenting these estimates, we start with manufacturing where the results closely support our identifying assumption, making the setup easier to explain. We then turn to estimates for all industries where there is evidence of rejections of these tests for some outcomes. We find, however, that the violations are driven by elections that lost by exactly 50 %, where we would expect such differences.

Figure 5 presents results from estimating equation 10 for all manufacturing elections. The estimates include our *flexible controls* specification (see the following parametric vote-share heterogeneity

³⁷We omit the establishment FEs here because we only estimate this specification for DHS growth rates and establishment survival where we never include establishment FEs.

analysis for robustness to alternative controls). Panel A. includes pre-period and treatment-effect estimates for each vote-share group with DHS employment growth as the outcome. First, the five-, three-, and two-year pre-trend estimates are similar across almost the entire vote-share distribution relative to 20-30 % elections (the one exception is 0-20 % elections which we exclude from our main analysis). These results support our identifying assumption by showing that the similarity between pre-election employment growth rates holds between much finer vote-share groups. Second, the figure shows that none of the five- and ten-year treatment effect estimates for losing elections are significantly different than the estimates for 20-30 % elections. These results provide reassurance against the concern that our main estimates are driven by future productivity shocks correlated with vote shares. In that case, we would also expect these shocks to cause different outcomes for losing elections with different vote shares. Finally, the five- and ten-year treatment effect estimates for winning elections increase in the union vote share but are not statistically different (e.g., -0.18 versus -0.28 ten-year estimates for 50-60 % and 70-80 % elections, respectively).

Figure 5 Panel B. plots the same estimates with establishment survival as the outcome variable. Although we cannot test for pre-trends in establishment exit rates, we can test our parallel trends assumption for establishment survival by estimating whether losing elections' with different vote shares had different post-election survival rates. Reassuringly, the survival rates for all losing election vote-share groups are not statistically different than the survival rate for 20-30 % elections. For winning elections, however, the figure shows that the long-run effects on survival increase in the union vote share, although the differences are not statistically different across groups.

Figure 6 presents the vote-share heterogeneity estimates for elections in all industries. Panel A. shows that for our main sample of 20-80 % vote-share elections, we find very similar pre-election employment growth rates for elections with different vote shares. For 0-20 and 80-100 % vote-share elections, however, we find evidence of different pre- and post-election growth rates, which is one motivation for excluding these elections from our main analysis. For post-election outcomes, we find similar DHS employment growth rates between 20-30 % and 30-40 % elections but find somewhat slower employment growth for 40-50 % vote-share elections. The ten-year estimate for 40-50 % elections is also significantly different from zero at the 10 % level. However, these negative estimates are driven by elections where the union received exactly 50 % of votes, and there are multiple reasons to expect differences between 50 % vote-share elections and elections where the union lost by slightly larger margins.³⁸ To see this, when we estimate the 40-50 % effects excluding elections with a 50 % vote share, the five-year estimate is -0.015 (SE 0.025) and the ten-year estimate is -0.032 (SE 0.028). Both estimates are much smaller than the treatment-effect estimates for the neighboring group of

³⁸There are multiple potential reasons for outcome differences at establishments with 50 % vote-share elections. First, due to the discreteness of total votes, elections with exactly 50 % vote shares have a small number of total votes cast (see the "integer problem" in DiNardo and Lee (2004)). For example, based on the NLRB data, the median (mean) number of voters in 50 % vote-share elections is 12 (22) compared to 50 (96) voters in elections with vote shares in the [45, 50) range. Although our employment controls capture corresponding establishment size differences, they do not capture potential differences in the bargaining unit size to employment shares. Second, the manipulation around the 50 % threshold is largely due to challenges to single votes which disproportionately affects elections with 50 % vote shares Frandsen (2017). Finally, experiencing an election where the union loses by just one vote may have a different effect on employers than losing by larger margins of support.

winning elections with 50-60 % of votes (-0.11 and -0.16 at the two time horizons). Furthermore, Panel B. of Figure 6 shows that there is no evidence of differential survival rates between 20 – 30, 30 – 40, and 40 – 50 % losing elections.

Parametric Vote-Share Heterogeneity To complement the previous *nonparametric* analysis, we estimate a series of *parametric* vote-share heterogeneity tests. Specifically, we test for linear trends in pre- or post-election outcomes by vote share. There are two motivations for this extension. First, these tests may have more power. Second, they provide a parsimonious way to assess robustness to different sets of controls. Specifically, we show that our estimates from these tests are qualitatively the same with just the *employment and industry controls* and the *flexible control* specification.

We first test for a linear trend in pre-election employment growth rates across the vote-share distribution. Second, we test for linear trends in post-election outcomes separately for winning and losing elections. To implement these tests, we estimate a modified version of the specification in equation 8. Specifically, instead of only interacting event-time with the winning indicator (e.g., $\mathbb{1}[V_i > .5]$), we include the following interactions with event-time³⁹

$$\underbrace{\mathbb{1}[t - E_i = n]}_{\text{Event Time}} \times \begin{cases} \rho \cdot V_i & \text{if } n < 0 \\ \eta \cdot \mathbb{1}[V_i > .5] + \theta \cdot V_i + \tau \cdot [V_i - .5]^+ & \text{if } n \geq 0 \end{cases} \quad (11)$$

For the pre-trend, vote-share heterogeneity test (i.e., $n < 0$), we do not include an interaction with treatment, so pre-period “treatment effects” would be captured in the ρ estimates. For the post-election outcome tests, we include an interaction with treatment so η estimates the treatment effect for close winning elections (e.g., a linear RD estimate). Consequently, the θ and τ coefficients estimate slope differences that do not include the change in outcomes right around the 50 % threshold.

Table 2 includes estimates of pre-election growth rate trends by vote share, ρ , for one to five years before the election. The estimates are for the main 20-80 % vote-share sample and are presented separately for all industry and manufacturing elections. We present estimates with the *employment and industry* and *flexible control* specification. Across all estimates, we never find significant pre-election growth rate trends.⁴⁰ These estimates complement the nonparametric evidence in Figures 5 and 6 by showing that the lack of pre-trends across the vote-share distribution holds formally testing for linear trends and only including more limited controls.

³⁹ $[V_i - .5]^+$ is equal to $[V_i - .5] \times \mathbb{1}[V_i > .5]$. Since we only estimate this specification for elections with 20-80 % vote shares, we actually shift the vote-share variables to all start at zero (e.g., subtracting 0.2 from the V_i variables and 0.3 from the winning vote-share variable). This ensures that the vote-share coefficients only capture slope and not level differences.

⁴⁰To assess the magnitude of the estimates, the largest positive point estimate is 0.05. A reasonable benchmark is what the estimates imply for the differences between 20-30 and 70 -80 % elections presented in Figure 6. Since the midpoints between those bins are 0.5 apart, the 0.05 coefficient implies a small difference in pre-election employment growth rates of around 2.5 % between 20-30 and 70 -80 % elections. For all industries, the confidence intervals also allow us to reject large trends in pre-election employment growth rates (e.g., with our flexible controls we can rule out five-year growth rate differences between the previous groups of more than around four percent). For manufacturing, however, the 95 % confidence intervals on some of the estimates would include relatively large pre-election growth rate differences.

Table 3 presents the estimates testing for post-election outcome vote-share trends. We present estimates of separate slopes for losing elections (i.e., θ) and winning elections (i.e., $\theta + \tau$). This table includes our preferred *flexible control* specification but Appendix Table A2 shows qualitatively similar results only including the *employment and industry* controls. Motivated by the potential issues with 50 % vote-share elections (see footnote 38), we also present the estimates with and without excluding 50 % elections from the estimates.

The results for all industries in Table 3 Panel A. indicate significant negative trends in DHS employment growth rates by vote share for both losing and winning elections. However, mirroring the nonparametric analysis, when we exclude the 50 % elections, we do not detect significant trends for losing elections. However, we find significant negative trends by vote share for winning elections, consistent with increasing treatment effects for larger margin-of-support elections. For example, we estimate a vote-share trend of -0.066 (SE 0.122) for losing elections and -0.389 (SE 0.149) for winning elections for five-year DHS employment growth rates. For establishment survival, we never find significant trends for winning or losing elections. For some specifications, the losing election trends are actually positive, further supporting our overall survival estimates not being driven by negative productivity shocks correlated with election vote shares.

The manufacturing estimates in Table 3 Panel B. are similar to the estimates for all industries pooled together. Without excluding the 50 % elections, we find negative although insignificant DHS employment trends for losing elections. However, dropping the 50 % elections results in smaller trends for losing elections and large although insignificant vote-share heterogeneity estimates for winning elections (e.g., five-year DHS trend estimates of -0.072 (SE 0.199) for losing elections and -0.406 (SE 0.299) for winning elections). The manufacturing survival estimates are also never significant for winning or losing elections and, at times, positive for losing elections.

Overall, these estimates in Table 3 also confirm the nonparametric post-election estimates in Figures 5 and 6. First, they show that, excluding the 50 % elections, the lack of a trend in post-election DHS employment growth by vote share holds testing for linear trends and only including the employment and industry controls. For establishment survival, we also cannot detect trends with and without excluding the 50 % vote-share elections. We note, however, that the 95 % confidence intervals for some of these estimates include relatively large post-election growth rate differences for losing elections. Additionally, the estimates for winning elections provide a formal test of treatment effect heterogeneity by vote share. Specifically, for the overall DHS employment growth rate estimates, we find significant vote-share heterogeneity.⁴¹ For our establishment survival effects, however, we do not find significant evidence of vote-share heterogeneity.

Employment and Survival Effect Robustness We next present two additional checks that further validate our overall estimates of the negative impacts of unionization on establishment survival and employment. First, we assess whether our estimated effects increase in the size of the bargaining unit (Lee and Mas (2012) conduct a similar test). The motivation is that the relative

⁴¹For manufacturing, the estimates are only significant at the 10 % level although, Appendix Table A2 presents more significant manufacturing vote-share heterogeneity estimates only including the industry and employment controls.

share of unionized workers should mediate many direct effects of unionization. However, potential violations of our identifying assumption may not be mediated by the share of unionized workers (e.g., workers voting based on their expectations of future company performance or managers’ competence). Appendix Table A3 presents the coefficient estimates from interacting the three-, five-, and ten-year treatment indicators with the share of each establishment’s total employment included in the bargaining unit (see Appendix C for details). It shows that the three and five-year treatment effects are significantly increasing in the bargaining unit share for both outcomes. These estimates confirm that the effect seems mediated by the share of workers gaining union representation. The interactions, however, are no longer significantly different than zero at the ten-year horizon. One explanation for the lack of persistence is that the relative size of the bargaining unit versus establishment employment could change substantially over time.

Second, Appendix Figure A4 plots DHS employment growth rate estimates with *ten-year* pre- and post-periods.⁴² First, it shows no evidence of large pre-trends in employment growth rates up to ten years before elections in manufacturing or for all industries pooled together. Although we find significant pre-period estimates six, seven, and eight years before the election in all industries, the estimates are economically small (e.g., approximately 1.7 to 2.0 percent differences). Moreover, the ten-year pre-period estimate is insignificant, and its confidence interval allows us to rule out employment growth differences of more than approximately 3.2 percent. Second, the figure shows that the post-election effects are relatively stable starting three years after the election. For manufacturing, however, there is a slight increase in the effect from years five to ten.

Industry-Specific Employment and Survival Estimates

We next separately estimate the effects for different industries and show that the overall effects are driven by non-service-sector elections. There are multiple reasons to expect heterogeneity across industries. First, the quality of labor relations may differ across sectors (e.g., the higher post-election strike propensity for manufacturing elections in Figure 2 suggests more adversarial relations). Second, firms in different industries may differ in how easily they can “avoid unionization.” For example, mobile, multi-establishment manufacturing firms may avoid working with new unions by shifting production to other establishments. However, this tactic may be difficult in non-tradable industries (e.g., hospitals) or tradable industries with ties to their local area (e.g., hotels).

To estimate this heterogeneity, we classify our elections into three broad industry groups: manufacturing, services, and a residual “other” group.⁴³ Weighted by the number of eligible voters,

⁴²Note, since our data start in 1976 and our elections start in 1981, the -10 to -6 estimates are from an unbalanced panel (e.g., the -6 point estimate is based on a different number of election years than the -5 estimate). This is one motivation for why we focus on the -5- to 10-year estimates with the balanced panel for the main analysis.

⁴³These classifications are based on the Fort and Klimek (2016) 2012 NAICS sectors of the Census establishment we match to each election. We define manufacturing as NAICS sectors 31-33 and services as NAICS 51-81 and NAICS 44-45. Our classification of services differs slightly from other measures. For example, compared to the sampling frame for the Census’s Service Annual Survey, we include retail trade in the services group and exclude utilities and transportation and warehousing. Bronfenbrenner (2002) also excludes utilities and transportation and warehousing from service-sector unions. The motivation for these changes is that we want to capture a notion of “service-sector unionization.” Retail workers (e.g., grocery store workers) are commonly referred to as part of service-sector unionization. As evidence of this, the “OUR Walmart” campaign was frequently described as

70 % of our service-sector elections are for healthcare (e.g., hospitals and nursing homes), security, restaurants, grocery stores, universities, and print media establishments. The other category includes agriculture, construction, mining, transportation and warehousing, utilities, and wholesale trade.

To estimate the industry-specific heterogeneity, we use the following specification for a categorical heterogeneity variable H_i (e.g., the three industry groups)⁴⁴

$$Y_{it} = \alpha_{t,E_i} + \sum_h \sum_n \delta_{h,n} \cdot \mathbb{1}[t - E_i = n] \times \mathbb{1}[V_i > .5] \times \mathbb{1}[H_i = h] + X_i' \beta_{n,E_i} + \varepsilon_{it}. \quad (12)$$

The $\delta_{h,n}$ coefficients now estimate the dynamic effects of successful union elections for elections with $H_i = h$. We also estimate all subsequent heterogeneity in Section 6 using equation 12.

Table 4 presents the DHS employment growth and survival effects estimated separately for each industry group. First, there is limited evidence of employment growth pre-trends for any of the groups. The only marginally significant pre-period estimate is for the service sector, where we find the smallest main effects. Second, the overall employment and survival decreases are driven by similarly sized effects for elections in manufacturing and the other sector. For elections in the service sector, the effects of unionization are substantially smaller. For example, the five-year DHS employment growth estimates for manufacturing and services are -0.174 (SE 0.029) and -0.057 (SE 0.024), respectively. Moreover, the ten-year survival estimate for the service sector is not significantly different than zero, and the confidence interval allows us to reject effects of more than four pct. pts.

Appendix Table A4 shows that the smaller effects of unionization in the service sector are robust to alternative sets of controls and sample selection criteria. Specifically, it presents the point estimate and standard error of the difference between the manufacturing and service-sector coefficients over each time horizon. The effects in manufacturing remain significantly larger when we (1) pool controls across cohorts, (2) restrict the sample to 30-70 % vote-share elections, and (3) restrict the sample to elections where the size of the bargaining unit was at least 25 % of total establishment employment. The results with the last restriction show that the smaller effects of unionization in the service sector are not because service-sector elections are more likely to only include a relatively small share of the establishments' overall employment.

6 Testing for Manager Opposition and Union Avoidance

After documenting the large overall impacts of successful union elections on establishment employment and survival, we test whether some of this effect is due to managers' dislike of working with unions or firms' ability to easily avoid working with new unions. For this analysis, a sector-specific analysis is the most appropriate because the tactics that employers can use to avoid unions may differ across

attempting to unionize service workers (Brown, 2011). Alternatively, most elections in utilities involved electrical workers (more similar to crafts unions in the building trades), and many elections in transportation and warehousing involved drivers.

⁴⁴This specification has two advantages relative to restricting the sample for each value of H_i . First, we can pool the control coefficients across heterogeneity groups and use all the data to estimate their coefficients. For all heterogeneity estimates, we also add the specific heterogeneity group as an additional control in X_i so that we account for any differential trends by the specific heterogeneity groups. Second, it allows us to easily conduct Wald tests of equality across the different heterogeneity groups.

sectors. In manufacturing, a common union avoidance tactic for multi-establishment firms during this time was shifting production away from unionized establishments to non-unionized establishments (Bluestone and Harrison, 1982; Verma, 1985; Kochan et al., 1986a). However, in construction, one of the industries in the “other” industry group where we also find negative effects, most firms are single-establishment firms, so they cannot shift production across establishments (Butani et al., 2005).⁴⁵ So, the same test might not capture union avoidance across sectors. Consequently, we focus on elections in manufacturing for three reasons. First, manufacturing is the largest sector where we find negative effects.⁴⁶ Second, as discussed above, we can use our data’s high-quality establishment and firm linkages to test for union avoidance via production shifting. Finally, we have detailed measures of establishment-level productivity in manufacturing that we use for this analysis. In this section, we refer to manufacturing “establishments” and “plants” interchangeably.

Multi- versus Single-Establishment Manufacturing Firms

The first part of our hypothesis is that firms avoid working with unions by shifting production away from newly unionized plants to other plants. Since this shifting is only possible for firms with multiple plants, we start by estimating whether the effects of union elections are larger at establishments part of multi-establishment (or multi-unit, MU) firms versus single-establishment (SU) firms. Specifically, we define “an election at an MU firm” based on whether the establishment’s firm had at least one other establishment under its control one year before the election.

Figure 7 plots the estimates for the DHS employment growth and survival effects for elections at SU versus MU firms. The left panel plots the cumulative DHS employment growth rates for five years before and three, five, and ten years after the election. Below each x-axis label, we include the p-value of the difference between the SU and MU estimates. Reassuringly, there is no evidence of differential pre-election employment growth rates for either group. After the election, we find significantly larger employment declines for elections at MUs at the three- and ten-year horizon. The estimates for SUs are, however, still negative and significant. For the establishment survival estimates in the right panel, the differences are even more striking. For all post-election time horizons, the effects are significantly larger for MUs, and none of the estimates for SUs are significantly different than zero. For example, the ten-year survival estimates are -0.122 (SE 0.021) versus -0.029 (SE 0.029) for MUs and SUs, respectively.

Appendix Table A5 shows the robustness of these estimates to (1) including controls pooled across cohorts and (2) only using 30-70 % vote-share elections to estimate effects. It presents the difference and standard errors between the SU and MU estimates. The estimates are very similar

⁴⁵Alternatively, in construction, there is evidence of firms avoiding unions by going “dual shop” and opening a new non-unionized shop that does previous work of the unionized shop (Evans and Lewis, 1989). Although we may see these new establishment openings in our data by linking establishments across Census `firmids`, owners may try to disguise the common ownership of these establishments to avoid potential labor-law issues with going “dual shop” (Milne, 1985). In other sectors, there is also evidence of employers using temporary workers to replace newly unionized workers (Hatton, 2014).

⁴⁶For all sectors in our manufacturing and other industry groups where we find large negative effects, manufacturing makes up 54 % of elections compared to 18 % for transportation and warehousing (the next largest sector). Weighting by the number of eligible voters, manufacturing comprises 68.8 % of voters due to its relatively large bargaining units.

with only pooled controls. For the 30-70 % bandwidth, we still estimate substantially larger survival effects for MU firms (e.g., six pct. pts. at the ten-year horizon), but the larger standard errors only lead to a significant difference for survival at the five-year horizon.

We interpret these results as showing that the effects of unionization on establishment survival in manufacturing are driven by plant closings at MU firms. For the overall employment declines, the effects are also significantly larger at the multi-establishment firms but still significant for SUs. This evidence is consistent with MU firms responding to unionization by shifting production across plants which we investigate more directly next. As an alternative explanation, MU firms may have a greater incentive to react strongly to unionization due to concerns about unionization spreading to their other establishments. We also investigate this later by focusing on entirely non-unionized MUs where this incentive may be even sharper.

Employment Shifting after Successful Elections

Next, we directly test the hypothesis that manufacturing firms avoid working with new unions by shifting production to other plants. Specifically, we analyze whether a successful election at one of a firm’s plants increases the employment and survival of the firm’s other plants. While the production-shifting hypothesis predicts positive effects on other plants, other mechanisms like input-output linkages or firms’ financial constraints predict negative spillovers (Boehm et al., 2019; Giroud and Mueller, 2017). However, one prediction of the production-shifting hypothesis is that the positive effects should be the largest at plants where it is easiest to produce the same products as the election plant. Consequently, we start by only considering the effects on other manufacturing plants and then restrict to plants in the same three-digit NAICS industry as the election plant.⁴⁷

To construct the sample for this analysis, we start with all manufacturing elections in a specific year at MU firms. Next, we take all of the firms’ *other* manufacturing plants that existed during the election year and never experienced their own union election.⁴⁸ We then calculate these plants’ DHS employment growth rates before and after the election relative to one year before the election. Finally, we stack these observations from all cohorts together and estimate a modified version of our main DiD specification 8.⁴⁹ The two differences from our main specification are that (1) relative time and vote-share variables are defined from the election at the firms’ *other* plant, and (2) we

⁴⁷LaLonde et al. (1996) similarly analyze within-firm employment spillovers of successful union elections. They do not find any evidence of spillovers but only consider the effects on all other manufacturing plants, where we also do not find only spillovers. We only find evidence of spillovers when we focus on other plants within the same three-digit NAICS industry. Bradley et al. (2017) similarly find that firms shift RD activity away from newly unionized establishments.

⁴⁸We exclude plants that ever experienced an election so our “spillover estimates” are not contaminated by direct effects. Yet, this conditioning could selectively bias our sample. The most plausible mechanism, however, biases us *against* finding positive spillovers. Specifically, assume that successful elections lead to *more* future elections at a firm. Since elections occur at relatively fast-growing plants and the plant needs to survive to hold a future election, we would drop faster-growing plants at firms with successful elections. This would downward bias our overall spillover estimates.

⁴⁹This construction results in some establishments being in the data set multiple times if their firms experience multiple union elections. For our baseline analysis, we avoided this problem by taking the first election at each establishment. For this analysis, similar conditioning is more difficult because the Census **firm** IDs change over time, even for firms that stay in business, and establishments can switch to different **firm** IDs. This also motivates our two-way clustering by firm and establishment.

weight the regression by each plant’s share of its firms’ total employment.⁵⁰ The reason for the weighting is that the sample could include multiple plants matched to each election, and we want to weight each election equally (i.e., not give the most weight to elections at firms with the most other plants). For this specification, we two-way cluster the standard errors by firm and establishment.⁵¹

Figure 8 Panel A. plots the dynamic employment effects of successful elections on the firms’ non-election plants. It presents estimates that include all manufacturing plants and that only include plants in the same three-digit NAICS industry as the election plant. For all manufacturing plants, there is no evidence of relatively higher employment growth at the other plants following successful elections. There are two things to note about this result. First, even if firms shifted employment away from newly unionized plants, it is not surprising that we do not find spillovers when we include *all* other plants. Specifically, many of these plants may have produced different products than the election plant, making production shifting more costly. Second, it is reassuring that we do not estimate *lower* post-election employment at the *other* plants of firms with successful elections. If our plant-level productivity shocks bias our estimates of the direct effects of unionization, we might expect some of these shocks to be firm-wide. Yet, the estimates in Figure 8 allow us to rule out differences in five-year DHS employment growth rates of more than -0.04 which is much smaller than our overall estimate for elections at MUs of -0.21.

We find significant employment growth effects when we restrict the sample to other plants that produced similar products to the election plants. The solid estimates in Panel A. present the coefficients estimated from just the firm’s other plants in the same three-digit NAICS industry as the election plant. Two years after the election, we estimate growth rate differences of 0.043 (SE = .019) for plants at firms with successful versus unsuccessful elections. These effects persist three and four years after the election. However, the effect becomes insignificant five years after the election and remains insignificant ten years following the election. Additionally, Table 5 presents both DHS growth rate and survival estimates and indicates that some of the overall increase in employment growth is due to an increased likelihood of plant survival.

Figure 8 Panel B. further splits up the same-industry elections based on whether or not the *election plant* made up a large share of the firm’s total employment. The motivation is that we would not expect to have enough power to detect spillovers when the election plant was only a small share of the firm’s overall employment. We specifically split up elections based on whether the election plant was more than 10 % of the firm’s employment in the same three-digit NAICS industry during the election year.⁵² The estimates in Panel B. show that the overall increase in other plants’ employment growth is driven by relatively large elections. It is reassuring for two reasons that the effects are driven by same-industry plants and by relatively large elections. First, these are the types of plants where we would expect to detect the most production shifting. Second,

⁵⁰For the denominator, we only include employment at plants in the sample so the employment weights sum to one.

⁵¹We use each establishments’ firmid during the election year (e.g., the clustering variable is fixed over time). Yet, since an establishment can appear multiple times in the sample, establishment clustering is not nested by firmid clustering.

⁵²This heterogeneity specification is estimated the same as other heterogeneity specifications (e.g., estimated jointly with pooled controls and controlling for the heterogeneity group by event time).

it is not clear why we would also expect potential threats to our parallel trend assumption to be more pronounced for these specific groups.⁵³

These other plant employment growth estimates are both economically and statistically significant. For the same-industry plant estimates, the increase in DHS employment growth rate of around 0.04 is consistent with a two percentage point increase in survival probabilities. When we focus on the same-industry plants at high-employment share elections, the spillover effects are even larger between 0.07-0.09. As a benchmark, the direct three-year DHS employment growth rate effect of unionization for elections at MU manufacturing plants is -0.23. While our spillover estimates suggest that a sizeable share of the overall negative effects of unionization may be offset by employment shifting, there are several reasons that we cannot use these estimates to calculate this share. First, our spillover estimates are average establishment-level employment changes, while we would need firm-level estimates to calculate the total share offset by reallocation.⁵⁴ Second, we focus on a specific subset of plants where we are most likely to detect spillovers. However, calculating the total share offset by reallocation requires the total firm-level employment changes (e.g., the estimates for all manufacturing plants where we do not find significant spillovers).

Overall, this evidence of successful union elections leading to faster employment growth at the firm's other plants is consistent with firms shifting production away from newly unionized plants. Furthermore, the higher survival probabilities for firms' other plants suggest that some of this production shifting occurs via decisions over which plants to close. Although we do not find significant long-run employment spillover estimates, this does not necessarily indicate a lack of long-run production shifting. First, given the increased variance of long-run employment growth rates, we may not have enough power to detect effects. Second, we may not be capturing all margins of production shifting that could occur over longer time horizons. For example, our analysis does not include shifting production by opening *new* plants or shifting production to plants in other countries (see e.g., [Bluestone and Harrison \(1982\)](#) and [Bronfenbrenner \(2000\)](#) for evidence of shifting production internationally following successful union elections).

Firms' Unionization Status

The second part of our hypothesis is that the effects of unionization are greater when management is more opposed to the union. To test this, we estimate treatment effect heterogeneity based on two proxies for managers' opposition. First, we estimate effects separately for elections at MU firms with and without other unionized establishments. The motivation for this analysis is evidence that, during this time period, non-unionized firms (e.g., firms without any unionized establishments) were more opposed to unions than (partially) unionized firms. For example, [Freedman \(1979\)](#) and [Kochan et al. \(1986b\)](#) show that less unionized firms were more committed to remaining non-union and

⁵³For this analysis, the concern that would violate the parallel trends assumption is that the other plants at winning election firms are growing faster than other plants at losing election firms.

⁵⁴We conduct an establishment-level analysis for two reasons. First, the longitudinal establishment linkages are higher quality than firm-level linkages ([Haltiwanger et al., 2013](#)). Second, we may have more power at the establishment level because we can include age, baseline employment, and time-varying industry controls that explain some of the employment growth variation.

they provide accounts of managers at non-union firms “vigorously resist[ing] dealing with unions.”⁵⁵ To test for heterogeneity by firms’ unionization status, we split up our elections at MUs based on whether we observe an FMCS contract at any of the firm’s establishments in the five years before the election.⁵⁶ Since the contract data start in 1984, we classify MUs as unionized versus non-unionized for elections starting in 1985 and show robustness to instead starting in 1990.

Figure 9 presents the DHS employment growth and survival estimates for elections at unionized versus non-unionized firms. The estimates are presented the same as the previous heterogeneity results (e.g., Figure 7). For overall employment growth rates, elections at non-unionized firms lead to larger employment decreases than elections at unionized firms. These differences are significant at the five- and ten-year horizons. For establishment survival, the differences are rather small and insignificant at the three- and five-year horizon. However, at the ten-year horizon, the negative survival effect is substantially larger for elections at non-unionized firms (e.g., -0.20 (SE 0.040) versus -0.09 (SE 0.027) for elections at non-unionized versus unionized firms, respectively).

Appendix Table A6 shows that the larger effects at non-unionized firms are robust to alternative sets of controls and sample selection criteria. Specifically, it presents estimates of the difference between effects at unionized versus non-unionized firms when (1) pooling controls across cohorts, (2) only classify firms into unionized versus non-unionized firms starting in 1990, and (3) only using 30-70 % vote-share elections to estimate the effects. The differences between estimates for non-unionized versus unionized firms are larger than our baseline specification when we define firms’ unionization status starting in the 1990s. For the other two specifications, the estimates are qualitatively the same as our baseline estimates.

These estimates show that the long-run negative effects of unionization are substantially larger at firms without any previously unionized establishments. This evidence is consistent with these firms being more opposed to and more rigorously resisting unionization. As one explanation for this opposition, Foulkes (1980) documents that some non-unionized firms were motivated by a philosophical opposition to unions even if they did not have previous bad experiences with unions. Alternatively, similar to Selten (1978)’s “chain store paradox”, non-unionized firms may have a strong incentive to aggressively deter the first unionization attempt to prevent unionization from spreading across the firm (even if not economically profitable when considering each establishment in isolation).⁵⁷ Both cases suggest that the larger effects at non-unionized firms may be quite excessive relative to the direct costs of unions at these firms.

⁵⁵One reason unionized firms would respond less aggressively to new unionization attempts is that their other unionized workers could apply pressure on the entire firm to discourage aggressive responses. An anecdotal example is the failure of GM’s “southern strategy” of opening non-unionized plants in the South due to pressure from the UAW (Nelson, 1996).

⁵⁶We include previous contracts at both the election establishment (i.e., a separate bargaining unit already unionized) and at all of the firm’s other establishments. See Appendix C for details.

⁵⁷As an extreme example, consider Walmart’s switch to pre-packaged meat across *all* stores days after ten Walmart meat cutters at one Texas store voted to unionize in 2000 (Zimmerman, 2000).

Election Delay Time

Our second proxy for managers’ opposition to the union is delay during the election process. The motivation is that managers frequently use tactics that delay the election date to try to win the election. First, delay itself can reduce support for the union. In “Confessions of a Union Buster,” [Levitt and Conrow \(1993\)](#) write that the NLRA “presents endless possibilities for delays, roadblocks, and maneuvers that can undermine a union’s efforts and frustrate would-be members” and that this delay “steals momentum from a union-organizing drive, which is greatly dependent on [...] the sense of urgency among workers.” Additionally, other tactics managers employ to influence elections also delay the election (e.g., challenging the composition of the bargaining unit or filing unfair labor practice charges). Furthermore, research has found that delay is associated with lower election win rates which supports delay time being a proxy for the intensity of managers’ anti-union campaigns ([Roomkin and Block, 1981](#); [Ferguson, 2008](#)).

We start by defining election delay time and verifying that it is related to election outcomes in our sample. We define delay time as the number of days between the date the election petition was filed to the NLRB and the date the election was held (see [Appendix C](#) for details). The average election delay in our sample is 62 days, and the 10th and 90th percentiles are 31 and 80 days. [Appendix Figure A5](#) shows that our measure of delay time is negatively associated with union election success rates and positively associated with the probability of any challenged votes in the election (another proxy for managers’ anti-union campaign intensity). These relationships also hold conditioning on other election characteristics that may be correlated with delay time.

To analyze whether the negative effects of unionization differ by election delay, we start by estimating treatment effect heterogeneity separately by terciles of the within-year delay time distribution. [Figure 10](#) plots the estimated effects for the first and third terciles for DHS employment growth (left panels) and establishment survival (right panels). Panel A. includes results for all elections and Panel B. just includes elections at MU firms. The p-values below the labels are from testing whether the effects for the first and third terciles are equal. Across both figures, the effects of unionization on establishment employment and survival are larger for elections in the top tercile of the delay time distribution. Focusing on elections at MUs, the first versus third tercile estimates are significantly different for both outcomes at the three- and ten-year horizon. For example, the estimated ten-year effect on survival for the top tercile is -0.20 (SE 0.037) versus -0.071 (SE 0.036) for the bottom tercile.

We next assess the robustness of these results to instead using a continuous measure of delay time. While the within-year terciles are appealing because they only rely on within-year variation in delay time and allow for a flexible, functional form, we might have more power using the entire distribution of delay times. To implement this, we add an interaction between the event-time treatment indicators times the *log* election delay time to the specification in [equation 8](#).⁵⁸ [Table 6](#) presents the coefficient estimates on the log election delay interaction for three, five, and ten years post-election. The first two columns show that the negative effects of unionization are significantly

⁵⁸We also control for log delay time interacted with event-time directly in the specification.

larger for elections with longer delays across all time horizons. For the ten-year survival effect, an approximately 10 % increase in election delay is associated with a .7 pct. pt. increase in the probability of a plant closing.⁵⁹ Columns (3) and (4) show that the effects are robust to including the controls pooled across cohorts. Columns (5) and (6) address the concern that our election delay time measure is just capturing larger bargaining units. Specifically, these columns show that our estimates are qualitatively the same when we first residualize the log delay time on bargaining unit size deciles, although the ten-year estimates are only significant at the ten-percent level.

Our primary interpretation of these results is that the negative effects of unionization are largest at establishments where the employer initially campaigned harder against the union. This is supported by anecdotal accounts linking election delays to the intensity of firms’ anti-union campaigns (Levitt and Conrow, 1993). Another interpretation is that delay may be a proxy for hostile labor relations conditions. For example, more adversarial unions and management might have more disagreement before the election that could delay the process. Overall, this heterogeneity adds to our results showing that managers’ opposition to unions plays a role in the overall negative effects of successful union elections.

Unionization and Productivity Reallocation

Finally, we examine how the negative effects of unionization vary by establishment productivity. The motivation for this analysis is that theoretical and empirical research in other contexts predicts that wage increases or productivity declines should have larger impacts at lower productivity establishments.⁶⁰ Since these channels are two leading “economic” reasons why unionization might cause decreased employment or exit, this research suggests that unionization may also have a larger impact on lower productivity plants. Consequently, substantial heterogeneity by establishment productivity may be more consistent with the survival effects being driven by direct effects on wages or productivity than our union avoidance hypothesis.

To measure establishment-specific TFP for our manufacturing elections, we use cost-share-based productivity measures from the Annual Survey of Manufacturers (ASM) and Census of Manufacturers (CM) calculated by Foster et al. (2016). We use within-industry TFP comparisons to address potential measurement or productivity differences across industries. Specifically, we classify each establishment into three productivity terciles based on their pre-election, within year and

⁵⁹Reassuringly, the magnitudes of the continuous-specification estimates are similar to the tercile specification estimates. The implied survival difference from the continuous specification between the 10th and 90th percentiles of the delay time difference is $[\ln(80) - \ln(30)] \times -0.07 = -0.066$. The ten-year survival difference between the first and third terciles is -0.089 .

⁶⁰For the effect of wage increases, Berger et al. (2021) show that minimum wage increases can cause relatively larger employment declines at less productive firms in oligopsonistic labor markets with firm heterogeneity. Luca and Luca (2019); Dustmann et al. (2020) provide empirical evidence of minimum wages increasing the exit rates of low-productivity and smaller firms. We note, however, that the settings are different as minimum wages are market-wide wage increases while unionization is an establishment-specific wage increase. We are not, however, aware of any models of unionization with firm heterogeneity and imperfectly competitive labor markets. For the effect of productivity declines, see (Foster et al., 2008) for evidence that plants’ level of revenue-based productivity is a key determinant of exit. One additional caveat to extending both predictions to the effects of unionization is that unions could base their bargaining demands on establishment productivity (e.g., try to extract more from higher productivity establishments).

six-digit NAICS industry TFP ranking (see Appendix C for details). Figure 11 plots the estimated effects for the first and third terciles of the baseline TFP distribution. We find evidence that the three- and five-year employment and survival effects are larger for lower-productivity establishments. But these differences are never significant and, at the five-year and ten-year horizon, are not economically very large (e.g., -0.066 (SE 0.023) versus -0.041 (SE 0.022) at the five-year horizon for the first and third terciles, respectively). Appendix Figure A6 shows that these patterns hold when we separately estimate heterogeneity by baseline TFP only for MU firms. Overall, we do not interpret this evidence as supporting economically larger survival effects for less productive establishments. Thus, the evidence is more consistent with alternative explanations for why unionization leads to plant closures (e.g., our union avoidance hypothesis) than conventional explanations.

7 Relation to Literature and Implications

In this section, we relate our estimates to the literature on unionization’s employment and survival effects. We also discuss the implications of our union avoidance results for interpreting these effects.

Unionization, Employment, and Survival Literature Our estimates of overall employment and survival decreases following successful union elections add to other recent research finding similar effects following elections. The most comparable results to ours are Frandsen (2021)’s regression discontinuity estimates also using the LBD. We qualitatively match his short-run employment and long-run survival declines but find somewhat smaller effects (e.g., five-year survival effects of 4 pct. pts. versus 8-10 pct. pts.). Some explanations for this are different samples or different empirical strategies and identifying assumptions.⁶¹ Additionally, our smaller employment effects and insignificant survival effects for service-sector elections match Sojourner et al. (2015)’s estimates of negative employment but no survival effects of nursing home elections. However, even for relatively close elections, our estimates are inconsistent with the null effects that DiNardo and Lee (2004) find for establishment survival and employment. One potential explanation is that the LBD longitudinal linkages we use to define survival are higher quality than linkages in the telephone-book-based InfoUSA or the LRD data used in DiNardo and Lee (2004).⁶²

Implications of Firm Union Avoidance and Opposition We also present evidence consistent with firms’ union avoidance tactics and their opposition to unions playing a role in these overall negative effects of unionization in manufacturing. First, we find evidence of firms avoiding new unions by shifting production away from newly unionized plants. Second, we find larger effects of successful union elections at firms more opposed to unions (based on whether the firm has other unionized establishments and on measures of delay during the election process).

⁶¹For example, Frandsen (2021) requires at least 20 votes cast to implement the regression discontinuity analysis while we only require more than five eligible voters. This could explain the differences in the magnitudes of our estimates since the average election in our sample may have a lower bargaining unit to overall establishment employment share. Furthermore, we find larger treatment effects for elections where the bargaining unit is a larger share of overall employment.

⁶²See, Jarmin and Miranda (2002) and Crane and Decker (2019) for comparisons of the linkages across these datasets.

One interpretation of these results is that firms’ opposition to unions and attempts to avoid unions are driven by managers’ dislike of working with unions and unrelated to the direct costs of unions. This interpretation is consistent with accounts suggesting that our measures of manager opposition were not based on expectations of the costs of unions. For example, [Freedman \(1979\)](#) finds that non-unionized firms placed the most weight on resisting new unions but that these firms were also where unions were least able to attain higher wages. For example, the threat of strikes was more limited at less unionized firms because these firms could still produce at non-union plants during a strike. Similarly, [Foulkes \(1980\)](#) documents that some non-unionized firms were motivated by a philosophical opposition to unions even if they did not have previous bad experiences with unions. Finally, [Bronfenbrenner \(2001\)](#) finds that the intensity of firms’ anti-union campaigns was “unrelated to the financial condition of the employer, but rather were a function of the extreme atmosphere of anti-union animus.” This interpretation of our results also helps resolve the puzzle that it has been difficult to find evidence of unionization raising wages or negatively affecting productivity (see the citations in footnote 2), but we find large effects on establishment survival. Additionally, it is puzzling that unions would make demands that push firms out of business, directly harming their workers ([Friedman, 1951](#)). The interpretation of our results that managers’ idiosyncratic opposition to unions drives the survival effects of unionization provides a resolution to these puzzles.

An intermediate interpretation of our results is that while there may be some direct costs unionization, the survival and employment effects we estimate are excessive relative to these costs. For example, if firms can easily shift production away from unionized establishments, even small wage or productivity effects could lead to large survival effects. Furthermore, the larger effects we find at non-unionized firms could be due to efforts to prevent unionization from spreading to other establishments by establishing a reputation for vigorously resisting unions. One channel that could magnify such effects is evidence that successful unionization negatively affects managers’ careers. For example, [Clark \(1980\)](#) finds increased manager turnover after successful elections and [Dunlop \(1994\)](#) documents some managers’ expectations that unionization would hurt their career prospects.

On the other hand, we cannot rule out that our proxies for manager opposition simply reflect rational expectations of where unions would have been the costliest. This interpretation is supported by evidence suggesting direct costs of unions (e.g., stock price declines following successful union elections and a large body of literature on unions reducing firm profits ([Freeman and Medoff, 1984](#); [Lee and Mas, 2012](#))). However, even parts of [Lee and Mas \(2012\)](#)’s stock price results are difficult to reconcile with the interpretation that our results are driven by direct costs of unions. In particular, as noted in [Frandsen \(2021\)](#), [Lee and Mas \(2012\)](#) only find equity value declines away from the 50 % threshold while we and [Frandsen \(2021\)](#) also find survival and employment declines for close elections. A way to reconcile these results is that the survival effects for these very close elections may not be driven by direct costs of unions that would also cause stock-market declines.

Overall, since we do not provide estimates of the magnitude of the direct costs of unions, we cannot rule out any of these interpretations. Yet, our evidence that the largest negative effects of unionization are where firms are the most opposed to unions and can avoid working with new unions

suggests that the overall negative effects may not necessarily imply large direct costs.

8 Conclusion

This paper revisits the effects of successful NLRB union elections on establishments’ employment and survival. We first show that winning union recognition decreases establishments’ employment and long-run survival. While one interpretation of these results is that unions must have large direct costs on businesses, we raise alternative explanations for these effects and explore whether our results for manufacturing elections are consistent with these explanations. First, we hypothesize that the effects of unionization could be magnified by firms’ ability to avoid working with the union. We support this hypothesis by showing that the largest effects are at multi-establishment firms and by providing direct evidence of increased employment at firms’ *other* establishments following successful elections. Both results are consistent with firms shifting production away from newly unionized establishments, one version of avoiding working with unions. Second, the overall negative effects may be partially driven by managers’ dislike of working with unions even if unions do not have large direct costs. Supporting this, we find the largest effects at non-unionized firms and at elections with the longest delay during the election process, both proxies for a firms’ opposition to unions. Overall, these results are consistent with firms’ union avoidance tactics and managers’ opposition to unions playing a role in explaining the overall negative effects of unionization.

Finally, our results raise many questions about the impact of unionization that suggest opportunities for further research. First, our results highlight how firms that strongly oppose unions go to great lengths to avoid working with unions. Another strategy they may pursue is raising workers’ wages to discourage unionization. These “union threat effects” may be one of the main channels through which unions have affected the U.S. wage structure (Taschereau-Dumouchel, 2020). Further research quantifying this channel would be helpful to understand unions’ total effect on wages (Neumark and Wachter, 1995; Farber, 2005). Second, we present evidence of large differences in how firms in different sectors respond to unionization. These findings mirror research that finds small employment effects of minimum wages in the service sector but larger negative effects in manufacturing (Cengiz et al., 2019; Harasztosi and Lindner, 2018). Together, these results suggest that understanding how tradable industries respond to wage increases would be useful for forecasting the effects of mandating higher wages in such industries. Specifically, the production shifting channel we explore in this paper may also explain why other labor market policies have larger effects in tradable industries. Third, our results raise the question of whether the managers’ opposition to unions is driven by the incentives created under the U.S.’s collective bargaining institutions. In particular, since unionization occurs at the establishment level, a single establishment may be the only one in its labor market or in its firm that needs to deal with a union. This setup could exacerbate managers’ incentives to resist unions (Estlund, 1993). Consequently, further research into whether the adverse consequences of unionization in the U.S. reflect the adversarial setup of its institutions would help inform future policies to foster more collaborative labor relations.

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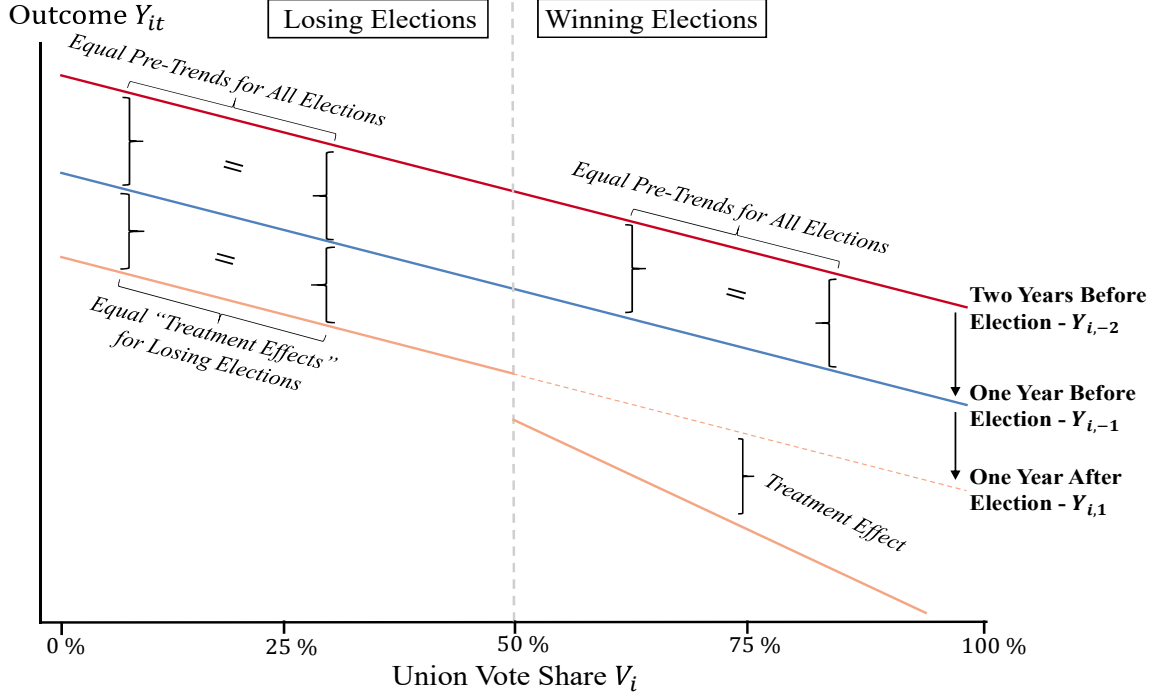
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9 Figures

Figure 1: Testable Implications of Parallel Trends Identifying Assumption

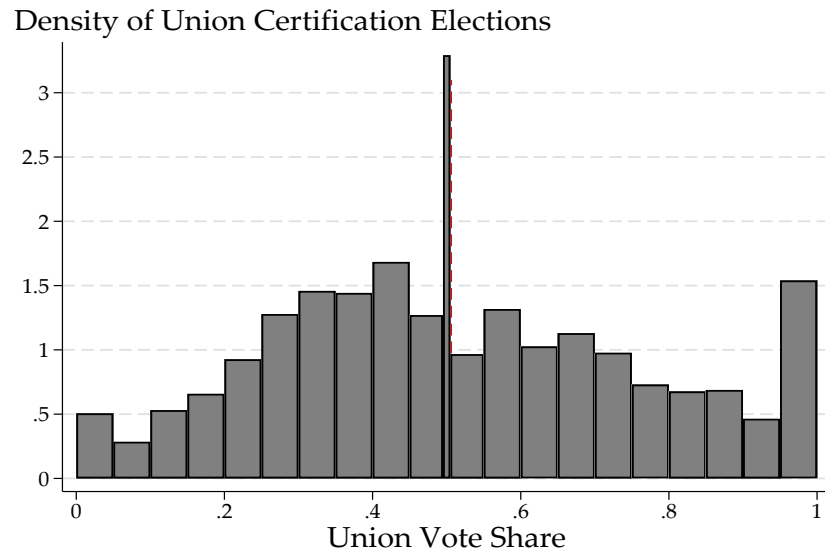


Note: This figure illustrates our empirical strategy’s identifying assumption and its testable implications discussed in section 4. It plots hypothetical average establishment-level outcomes before and after union elections with different vote shares. $Y_{i,-2}$ and $Y_{i,-1}$ correspond to outcomes one and two years before the union election. $Y_{i,1}$ corresponds to outcomes one year after the election. Testing parallel pre-trends by vote share corresponds to comparing the distance between $Y_{i,-2}$ and $Y_{i,-1}$. Testing parallel post-trends for losing elections corresponds to comparing the distance between $Y_{i,-2}$ and $Y_{i,1}$ for losing elections.

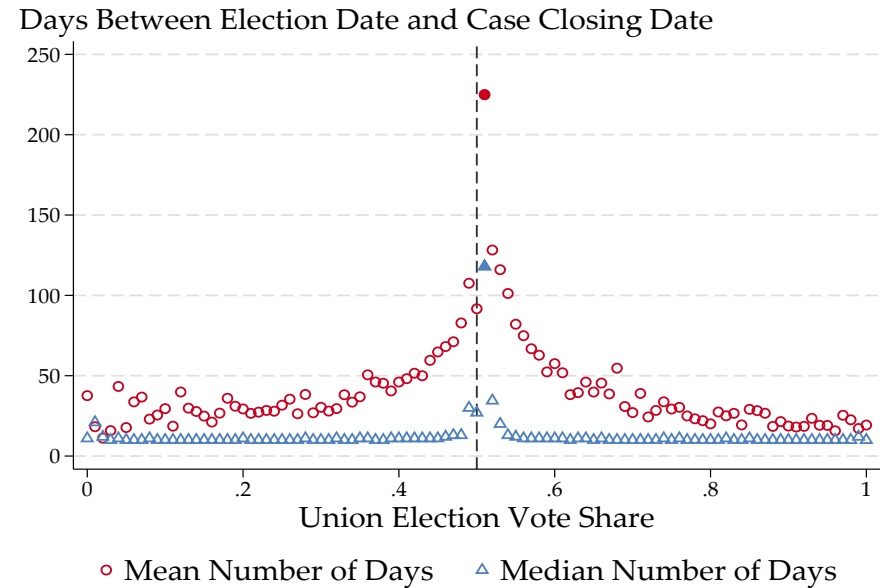
Figure 2 Note: Figure 2 presents four panels illustrating characteristics of close union elections. All panels are constructed using external union election data (e.g., not our final sample matched to the Census) but the sample was constructed to mirror the overall sample construction (see Appendix C for details). Panel A. plots the vote-share histogram of elections included in our sample. Given the discreteness of the running variable and the fact that our sample includes elections with a small number of votes, it is difficult to detect manipulation from the vote-share density figure. Consequently, we plot elections with exactly 50 % of votes separately to make the manipulation easier to see. See Frandsen (2017) for evidence of manipulation using formal tests that accommodate discrete running variables. Panel B. plots the average and median number of days between the union election date and the date that the case closed. Panel C. plots the probability of each union election experiences a decertification election in the five years following the case closing. The decertification elections are also from our combined NRLB datasets but excluded from our main analysis. Panel D. plots the probability of each union election experiencing a works stoppage in the five years following the case closing. The works stoppage data is from the FMCS and covers works stoppages from 1984-2019. Consequently, we only plot follow-up works stoppages for elections from 1984-2005. For the decertification and works stoppage figures, we match based on exact company names and cities rather than the SoftTFIDF algorithm we use for the main analysis. The “conditional regression coefficients” are the coefficients from regressing the stoppage indicators on the vote share for winning elections including controls for deciles of the number of workers in the bargaining unit, the four-digit NAICS industry, and election state.

Figure 2: Characteristics of Close Elections that Motivate Including Larger Margin-of-Support Elections

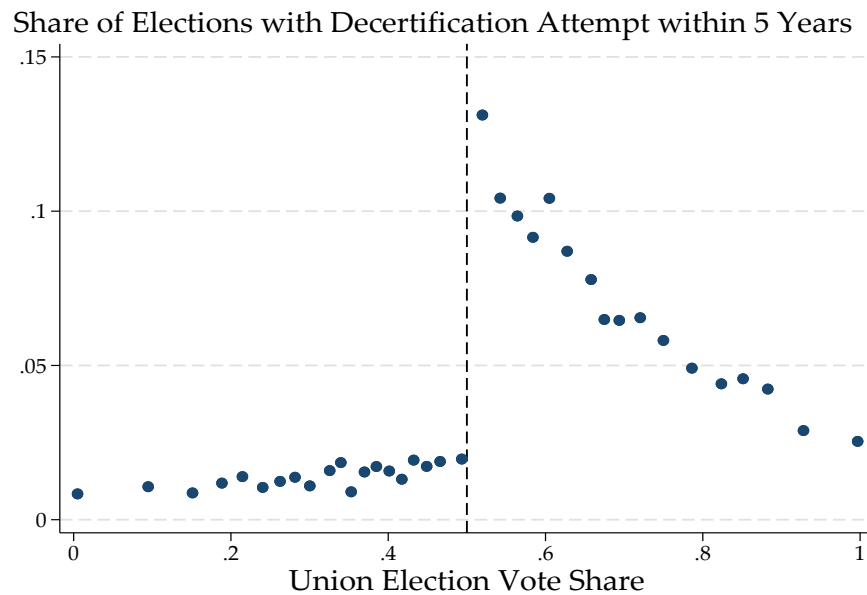
Panel A. Election Vote-Share Histogram



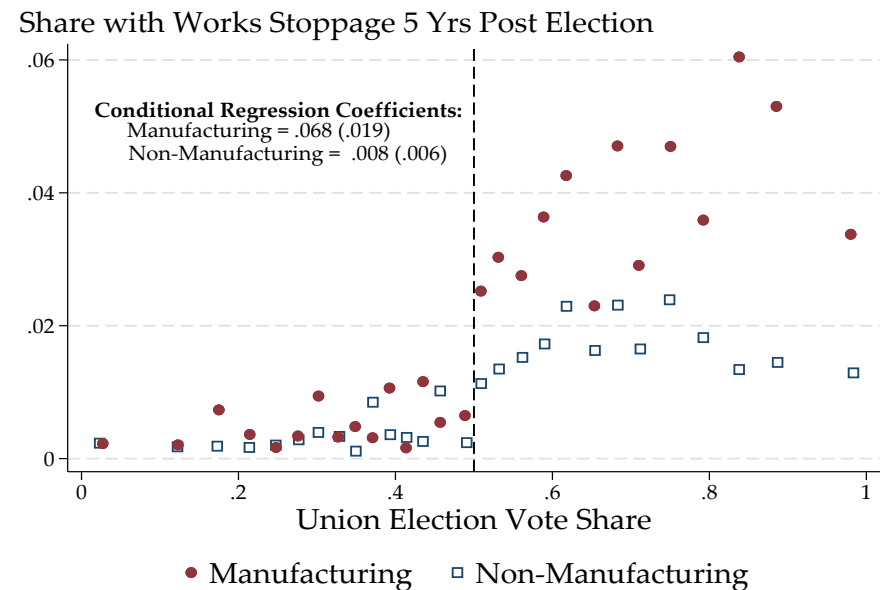
Panel B. Number of Days Between Election and Case Closing Dates



Panel C. Probability *Decertification Election* Five Years Following Election

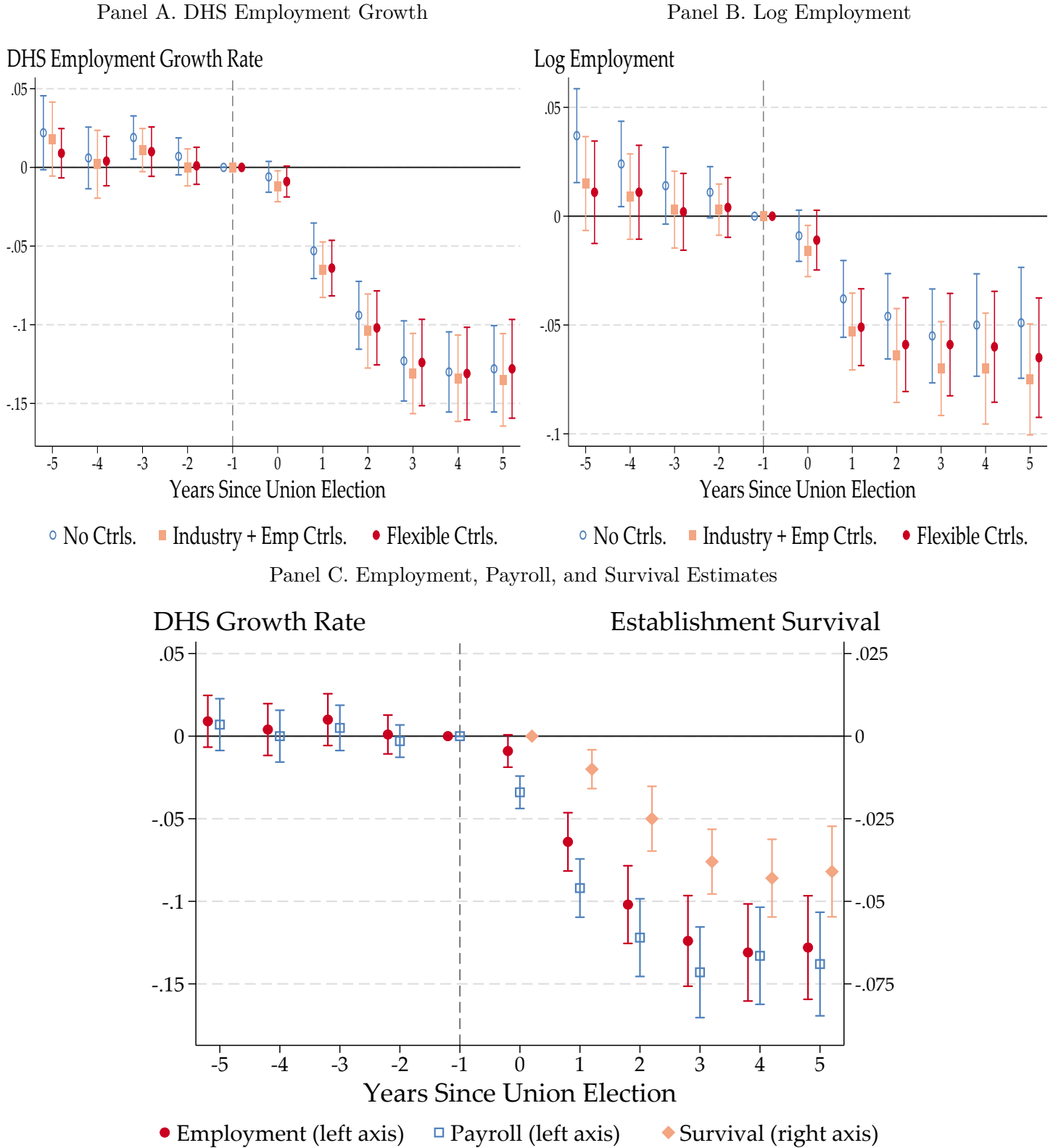


Panel D. Probability of *Works Stoppage* Five Years Following Election



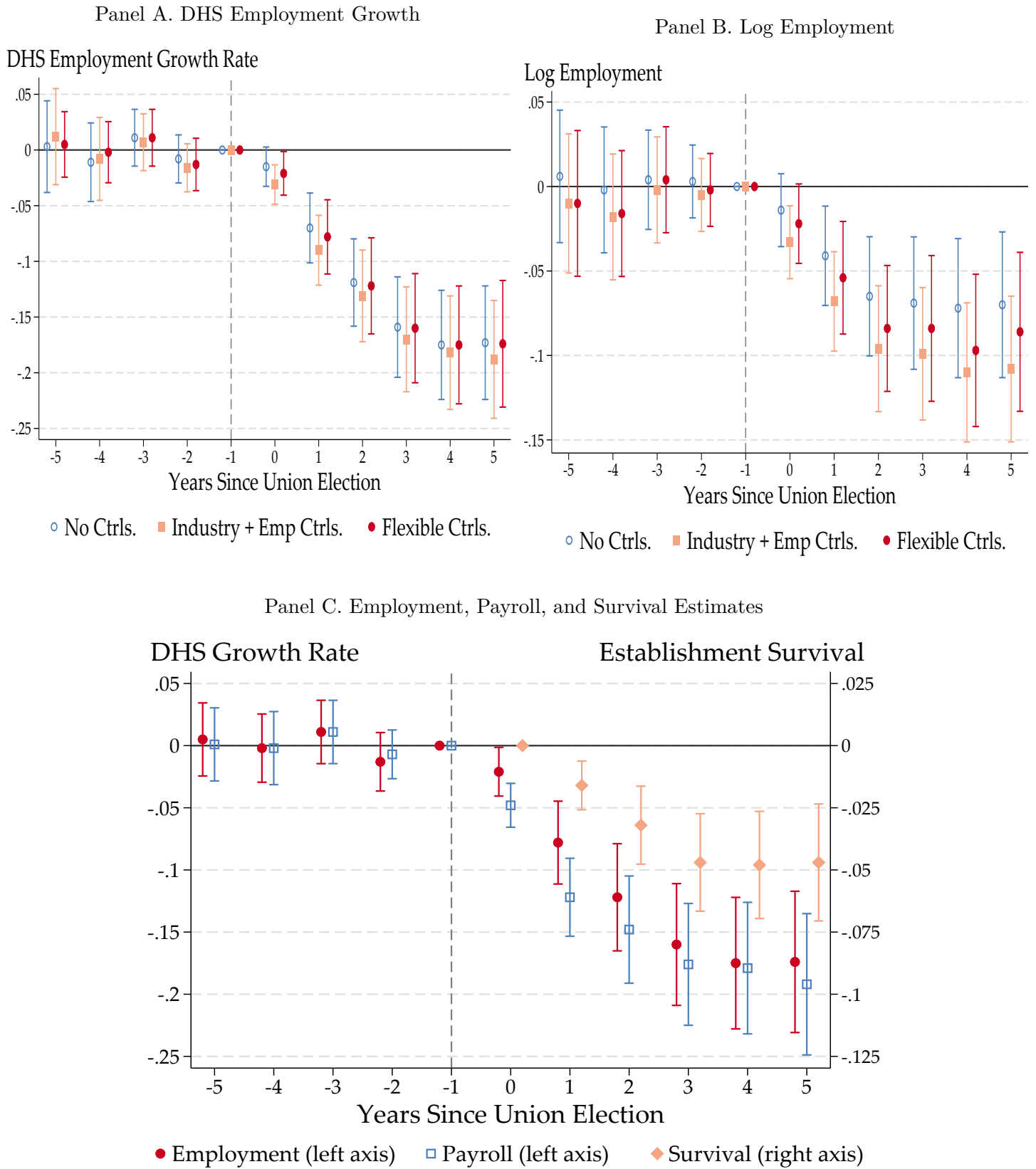
Note: See the previous page.

Figure 3: Employment and Survival Estimates, 20-80 % Vote-Share Elections, All Industries



Note: This figure plots the δ_n coefficients (i.e., the interaction between winning a union election and being n years from the election) from estimating specification 8 for all union elections with 20-80 % vote shares inclusive. The sample includes observations -10 to 10 years before and after each union election but we only plot the -5 to 5 coefficients. The outcome variable for Panel A. is establishment-level DHS employment growth relative to time -1. The outcome variable for Panel B. is establishment-level log employment. The outcome variables for Panel C. are DHS employment and payroll growth rates and an indicator for whether the establishment exists at time t . For Panel C., the survival y-axis is scaled to be one-half the DHS growth rate axis. Consequently, comparing the exit and DHS coefficients illustrates how much of the effect on the DHS growth rate can be mechanically explained by the exit effect. Panels A. and B. include estimates with no controls, just industry and employment controls, and the flexible control specification (see Section 4 for details). Panel C. includes estimates from the flexible control specification. The log outcome estimates in Panel B. include establishment fixed effects but these are not included in Panel A. or Panel C. Standard errors are clustered by establishments' firmid during the year of the election (e.g., the clustering variable is fixed over time for each establishment).

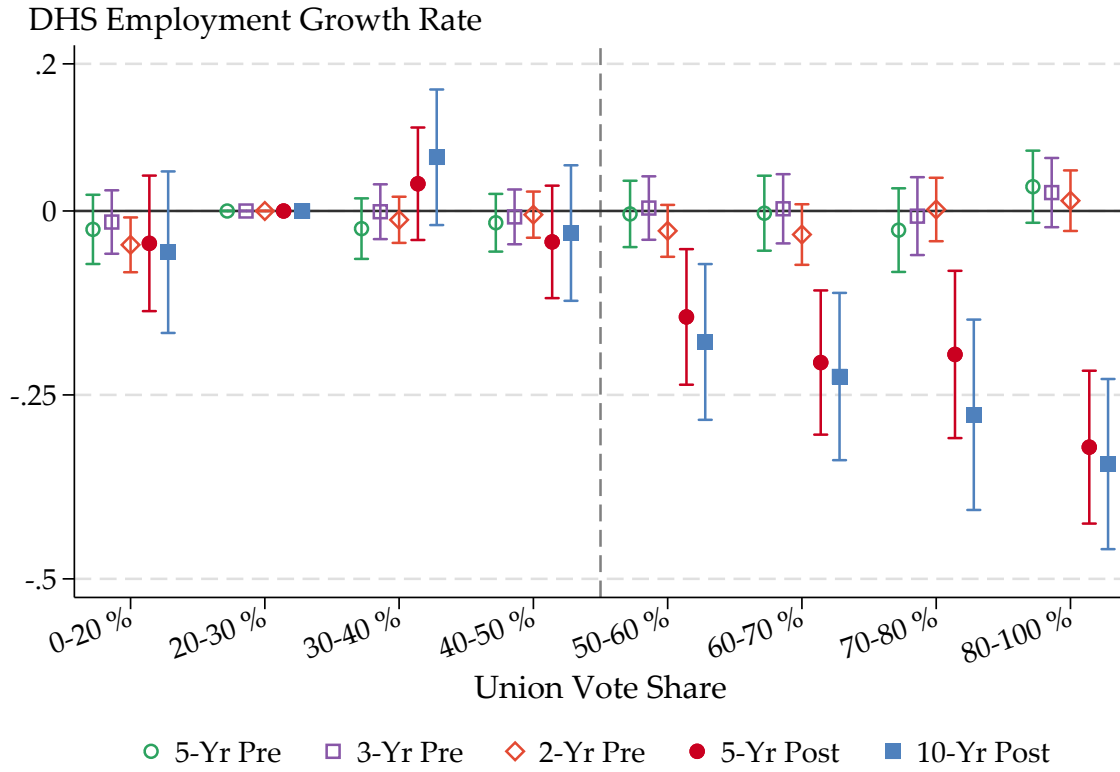
Figure 4: Employment and Survival Estimates, 20-80 % Vote-Share Elections, Manufacturing



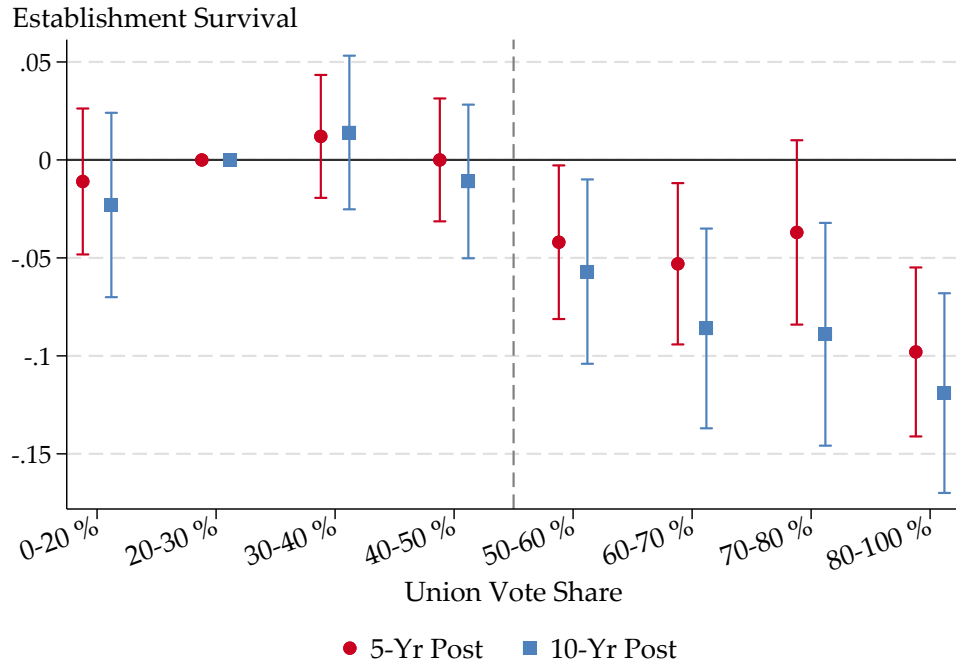
Note: These estimates are identical to Figure 3 except that they are only estimated for manufacturing elections.

Figure 5: Nonparametric Vote-Share Heterogeneity Estimates, Manufacturing

Panel B. DHS Employment Growth Rate



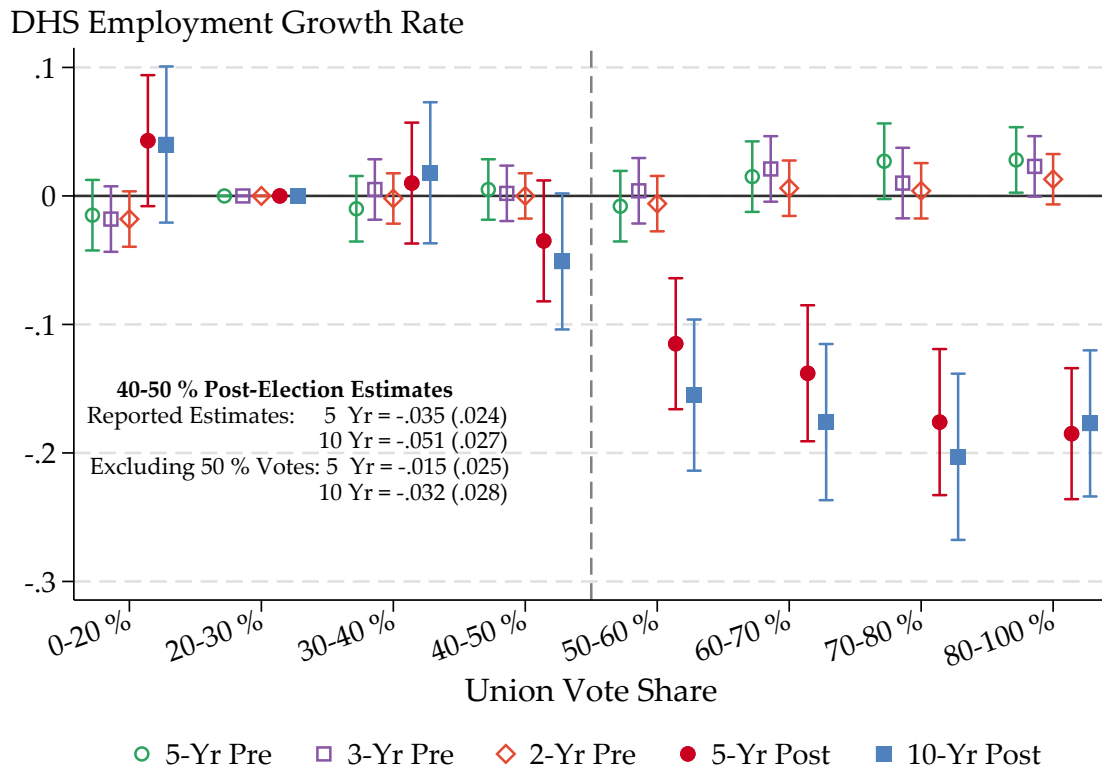
Panel B. Establishment Survival



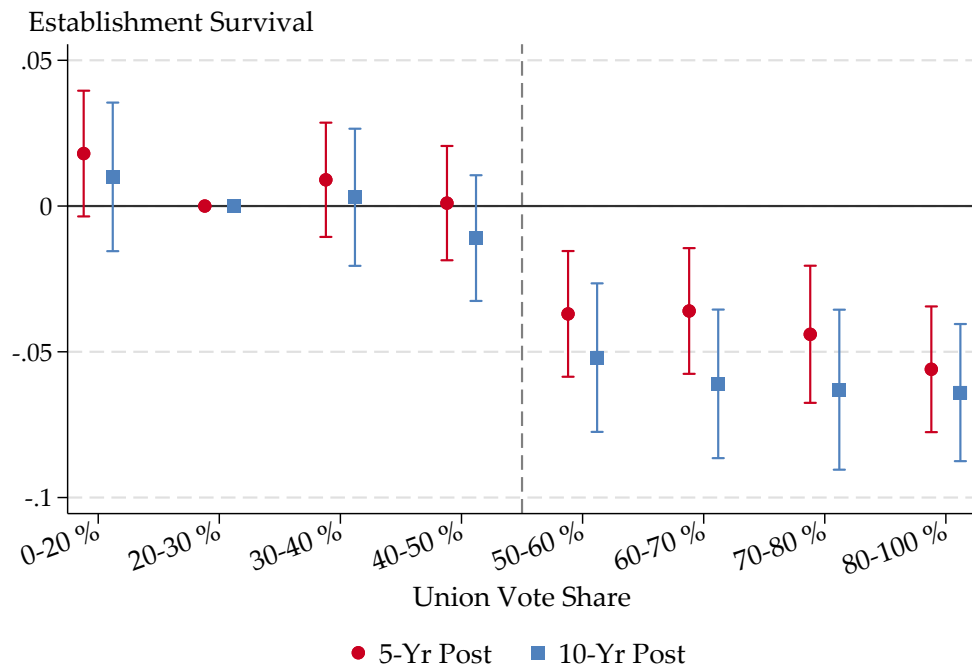
Note: This figure plots the $\delta_{g,n}$ coefficients from estimating the vote-share heterogeneity specification 10 with the vote-share distribution partitioned into eight groups indicated on the x-axis. We omit the 20-30 % election group so the other estimates are relative to that group. The sample includes all manufacturing elections. We include observations -10 to 10 years before and after each union election but we only plot a subset of coefficients. The outcome variable for Panel A. is establishment-level DHS employment growth relative to event time -1. The outcome variable for Panel B. is an indicator for establishment survival. The estimates include the flexible control specification (see Section 4 for details). Standard errors are clustered by establishments' firmid during the year of the election (e.g., the clustering variable is fixed over time for each establishment).

Figure 6: Nonparametric Vote-Share Heterogeneity Estimates, All Industries

Panel A. DHS Employment Growth Rate

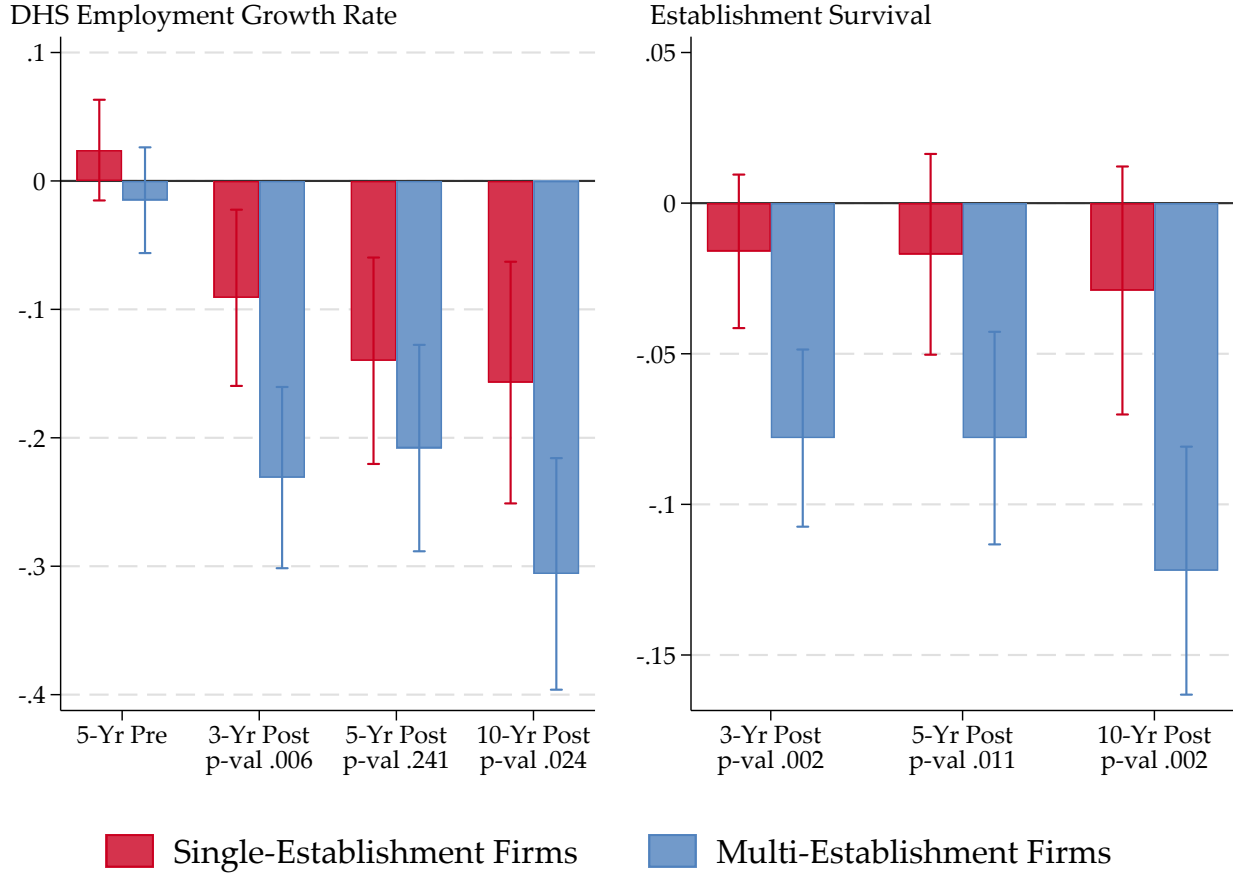


Panel A. Establishment Survival



Note: This figure is identical to Figure 5 except it includes elections across all industries. The alternative estimates listed in the text box in Panel A. are the 40-50 % estimates excluding elections with exactly 50 % of votes (rather than restrict the sample, we include a separate category for 50 % vote elections).

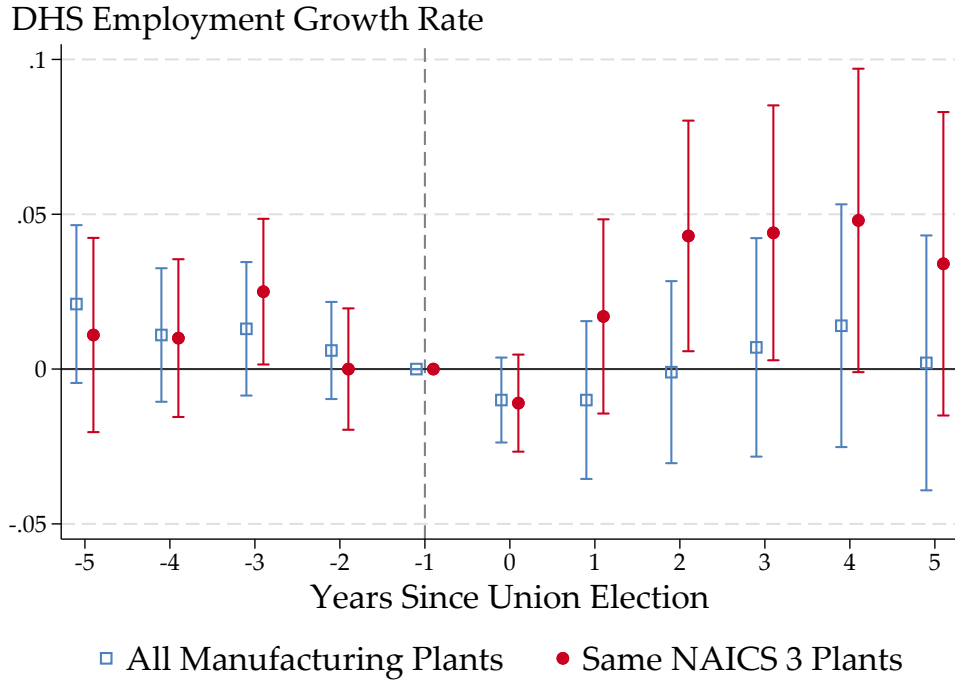
Figure 7: Single- Versus Multi-Establishment Firm Heterogeneity



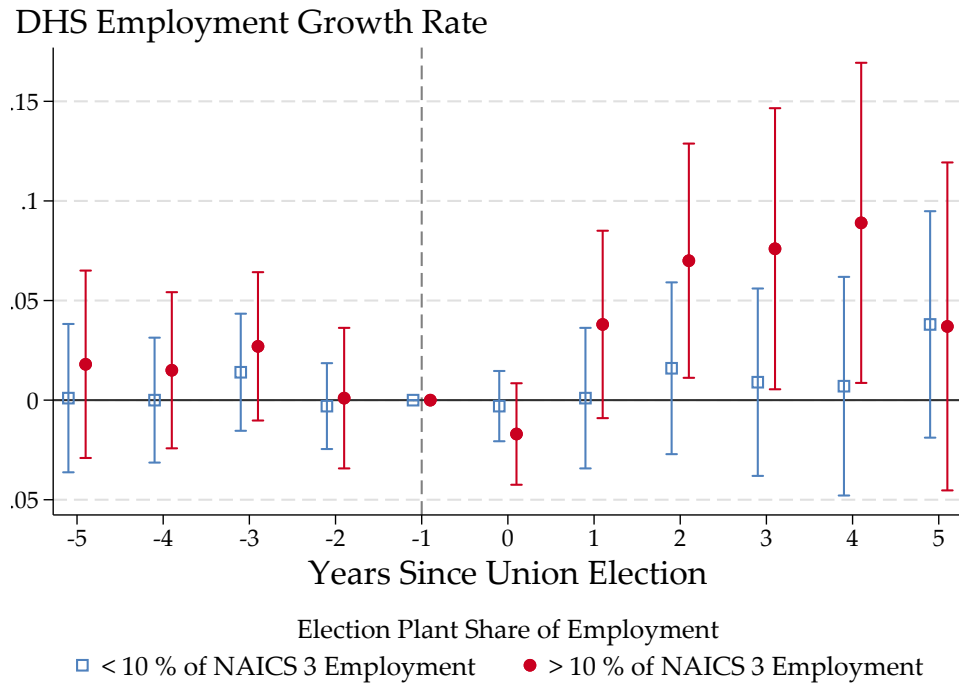
Note: This figure plots the $\delta_{h,n}$ coefficients from estimating our heterogeneity specification in equation 12 for elections at single- versus multi-establishment firms. An election at a multi-establishment firm is defined based on whether the establishment's firm has any other establishments one year before the election. The sample includes all manufacturing union elections with 20-80 % vote shares inclusive. It includes observations -10 to 10 years before and after each union election but we only plot a subset of these coefficients. The outcome variable for the left panel is DHS employment growth rates relative to time -1 (see Section 4 for their definition). The outcome variable for the right panel is an indicator for establishment survival. The estimates include the flexible control specification (see Section 4 for details). The control coefficients are pooled across the heterogeneity groups. See Appendix Table A5 for robustness to alternative controls specifications. Standard errors are clustered by establishments' firmid during the year of the election (e.g., the clustering variable is fixed over time for each establishment).

Figure 8: Employment Effects of Successful Elections on Firms' Other Establishments

Panel A. All Plants and Three-Digit NAICS Plants

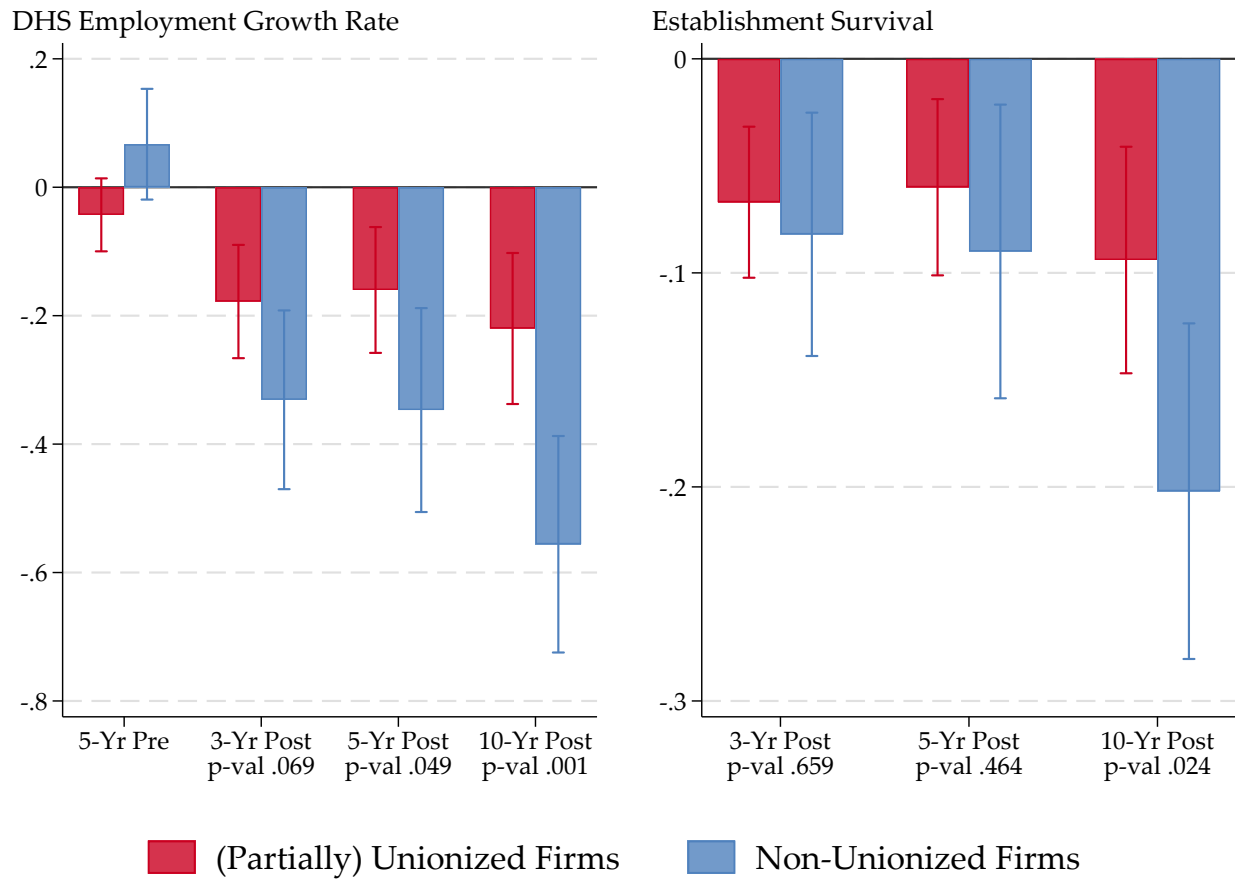


Panel B. Estimates by Election's Employment Share



Note: This figure plots the δ_n coefficients from estimating specification 8. The sample is manufacturing plants at multi-establishment firms where *another* plant experienced a union election. See Section 6 for details about the sample construction. The relative time and vote-share variables are defined from the election at the firm's *other* establishment. We weight the regression by the observation's share of total firm-level employment across all plants included in the sample the year of the election. The outcomes in both panels are establishment-level DHS employment growth rates relative to one year before the union election. The estimates include the flexible control specification (see Section 4 for details) except we do not include a control for establishments SU/MU status (all plants are part of MUs) or for establishments' previous contract status. Since we match plants based on the election year, the industry is also from the year of election. The "All Manufacturing Estabs" estimates in the left panel include all manufacturing establishments with at least two employees during the year of the election. The "Within-NAICS 3 Estabs" estimates restrict the sample to plants that are in the same 3-digit NAICS industry as the election plant. The right panel includes 3-digit NAICS industry matches but separately estimates the effects by whether or not the election establishment comprised more than 10 % of the firm's employment in the same three-digit NAICS industry during the year of election. The estimates in Panel B. are from the same specification with the controls pooled across both groups and the treatment indicators interacted with the two employment share groups. In this panel, we also directly control for the effect of the two employment share groups interacted with event time.

Figure 9: Unionized versus Non-Unionized Firm Heterogeneity



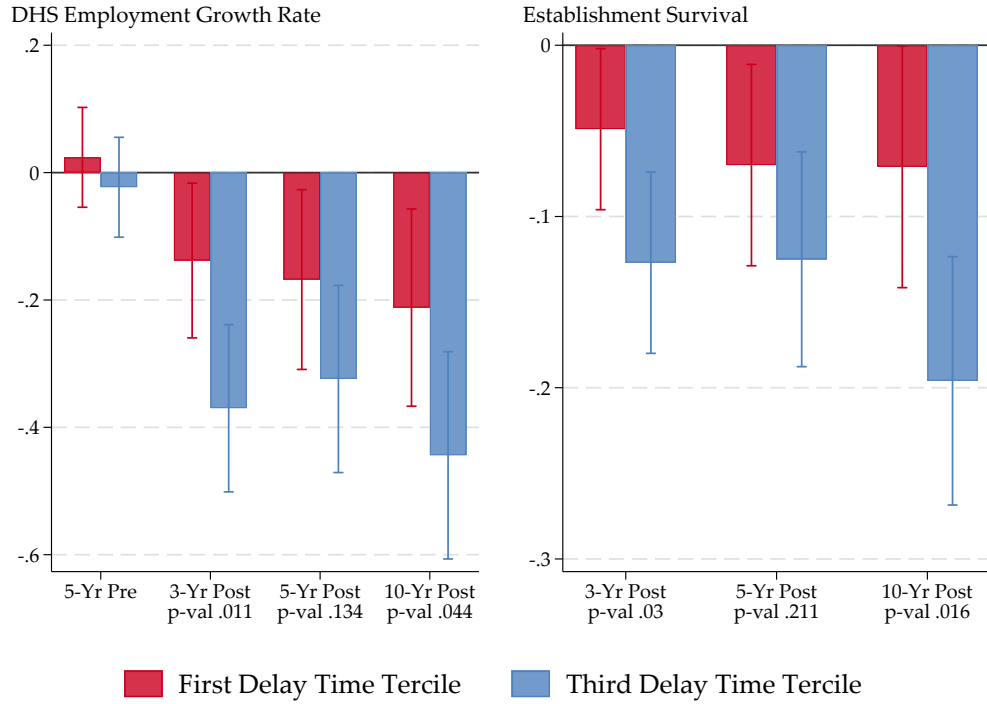
Note: This figure plots similar heterogeneity results as Figure 7 except that the heterogeneity is for elections at multi-establishment firms with at least one unionized establishment versus firms without any unionized establishments. See Appendix C for how we define firms' unionization status. The controls additionally directly include these heterogeneity groups interacted with cohort and event time.

Figure 10: Election Delay Heterogeneity

Panel A. All Elections

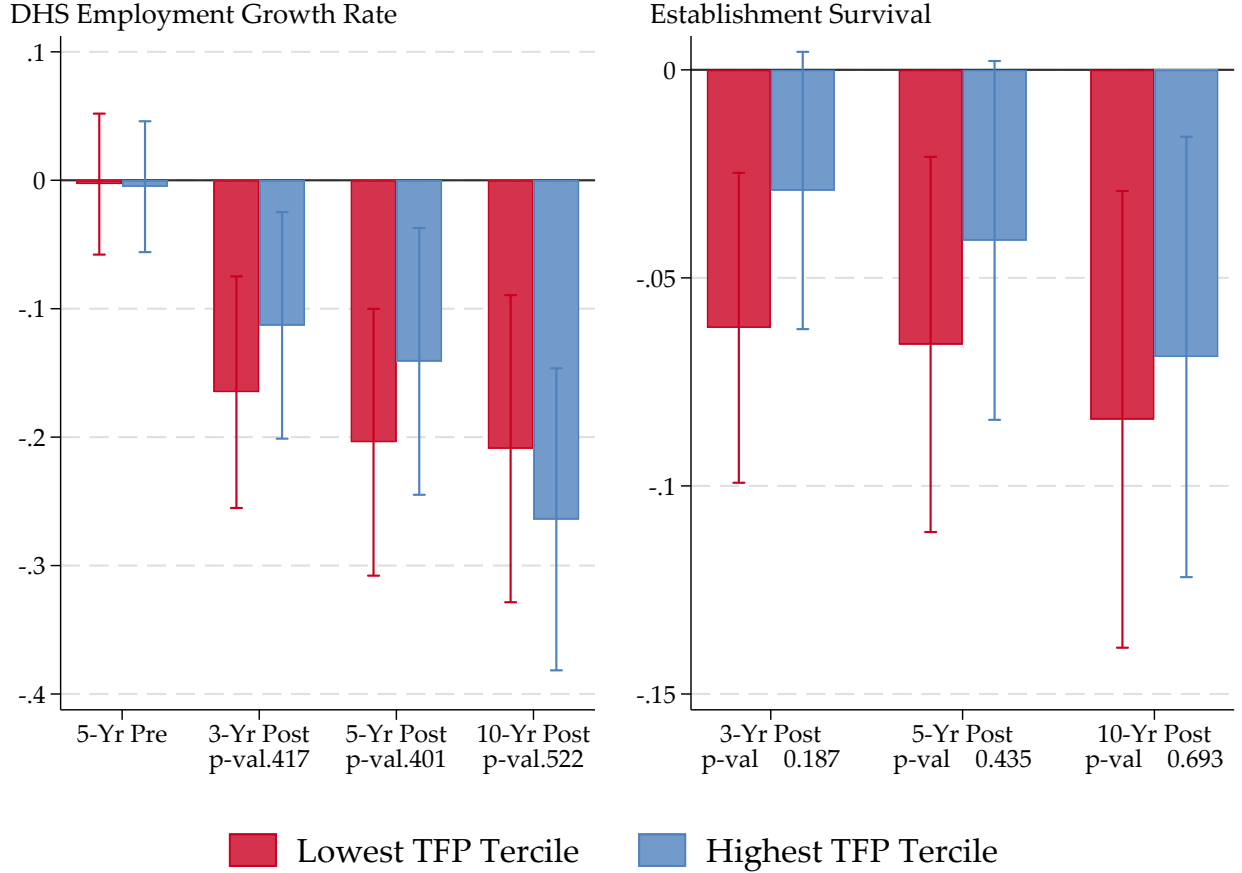


Panel B. Elections at Multi-Establishment Firms



Note: These figures plots the $\delta_{h,n}$ coefficients from estimating our heterogeneity specification in equation 12 for elections in different terciles of the *election delay* distribution. These terciles are defined within each year based on the number of days between the election petition filing date and the election date (see Section C for details). We plot the coefficients for the first and third terciles but estimate the effects for all three. The sample includes all manufacturing union elections with 20-80 % vote shares inclusive. It includes observations -10 to 10 years before and after each union election but we only plot a subset of these coefficients. The outcome variable for the left panel is DHS employment growth rates relative to time -1 (see Section 4 for their definition). The outcome variable for the right panel is an indicator for establishment survival. The estimates include the flexible control specification (see Section 4 for details). Standard errors are clustered by establishments' firmid during the year of the election (e.g., the clustering variable is fixed over time for each establishment). Panel A. defines the election delay terciles across all elections. For Panel B. the election delay terciles are only defined for elections at multi-establishment manufacturing firms. Consequently, we estimate but do not report separate coefficients for elections at single-establishment firms.

Figure 11: Establishment-Level Total Factor Productivity Heterogeneity



Note: This figure plots the $\delta_{h,n}$ coefficients from estimating our heterogeneity specification in equation 12 for elections in different terciles of baseline TFP distribution. These terciles are defined based on plants' pre-election cost-share-based productivity measures from the Annual Survey of Manufacturers (ASM) calculated by Foster et al. (2016). The TFP terciles are defined based on within-year and within six-digit NAICS productivity rankings. See Appendix C for details. We plot the coefficients for the first and third terciles but estimate effects for all three terciles and a fourth group of plants without TFP defined. The sample includes all manufacturing union elections with 20-80 % vote shares inclusive. It includes observations -10 to 10 years before and after each union election but we only plot a subset of these coefficients. The outcome variable for the left panel is DHS employment growth rates relative to time -1 (see Section 4 for their definition). The outcome variable for the right panel is an indicator for establishment survival. The estimates include the flexible control specification (see Section 4 for details). The controls additionally include these heterogeneity groups interacted with cohort and event time. Standard errors are clustered by establishments' firmid during the year of the election (e.g., the clustering variable is fixed over time for each establishment).

10 Tables

Table 1: Winning versus Losing Election Establishment Summary Statistics

| | All Industries | | Manufacturing | |
|--|----------------|------------|---------------|------------|
| | Union Loses | Union Wins | Union Loses | Union Wins |
| Establishment Characteristics | | | | |
| Employees | 154 | 137 | 167 | 148 |
| Payroll/Worker (\$ 2019) | 49,400 | 49,700 | 49,900 | 48,700 |
| Establishment Age | 9.65 | 10.0 | 9.82 | 9.58 |
| Multi-Establishment Firm | 0.512 | 0.476 | 0.525 | 0.464 |
| Previous Contract at Establishment | 0.090 | 0.147 | 0.088 | 0.152 |
| Survival Base Rates | | | | |
| 5-Year Survival | 0.818 | 0.765 | 0.847 | 0.779 |
| 10-Year Survival | 0.667 | 0.610 | 0.702 | 0.608 |
| Approximate Number of Elections | 27,000 | | 7,000 | |

Note: This table presents summary statistics for all union elections included in our analysis sample with vote shares between 0-100 %. All establishment characteristics are measured one year before the union election. Since the FMCS contract data are only available starting in 1984, we only calculate the share of establishments with a previous contract using elections from 1985 onward. The five- and ten-year survival rates are the probability of surviving five and ten years after the union election, respectively. To satisfy the Census' disclosure requirements, all estimates are rounded to only include three significant digits, and sample sizes are round to the nearest 1,000.

Table 2: Pre-Election Employment Growth Trends by Vote Share, 20-80 % Elections

| Outcome: | DHS Employment Growth Rate | | | |
|---|----------------------------|------------------|-------------------|-------------------|
| Industry Group: | All Industries | | Manufacturing | |
| 5-Year Pre Election \times Vote Share | 0.050 (0.037) | 0.033 (0.025) | 0.029 (0.069) | -0.018 (0.047) |
| 4-Year Pre Election \times Vote Share | 0.018 (0.032) | 0.019 (0.024) | 0.026 (0.059) | 0.008 (0.045) |
| 3-Year Pre Election \times Vote Share | 0.028 (0.023) | 0.029 (0.023) | 0.022 (0.041) | 0.018 (0.042) |
| 2-Year Pre Election \times Vote Share | 0.006 (0.018) | 0.012 (0.019) | -0.026 (0.035) | -0.018 (0.037) |
| Industry + Employment Ctrl. | X | X | X | X |
| Flexible Ctrl. | | X | | X |
| Number of Elections | 19,000 | 19,000 | 6,000 | 6,000 |

Note: This table presents estimates testing for linear trends by vote share in pre-election employment growth rates. Significant estimates would violate a testable implication of our parallel trends by vote share assumption (see equation 5). Specifically, the table reports the estimated coefficients on interactions between event-time indicators and the continuous election vote-share (i.e., the ρ coefficients from equation 11). A five-year coefficient of 0.03 implies that elections with 75 % of votes grew approximately 1.5 percent slower during the five years before the election than an election with 25 % of votes. The outcome for all specifications is establishment-level DHS employment growth relative to time -1 . The sample includes 20-80 % vote-share elections. The first two columns include elections in all industries and the last two columns include just manufacturing elections. The odd columns include only industry and employment controls and the even columns include our flexible control specification (see Section 4 for details). Standard errors are clustered by establishments' firmid during the year of the election (e.g., the clustering variable is fixed over time for each establishment). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Post-Election Outcome Trends by Vote Share, 20-80 % Vote-Share Elections

| Outcome: | DHS Emp Growth Rate | | Establishment Survival | |
|---|----------------------|----------------------|------------------------|-------------------|
| Panel A: All Industries | | | | |
| <i>Event-Time \times 0-50 % Vote Share</i> | | | | |
| 3-Year Post Election | -0.216** (0.095) | -0.085 (0.103) | 0.013 (0.036) | 0.036 (0.040) |
| 5-Year Post Election | -0.220** (0.110) | -0.066 (0.122) | -0.031 (0.045) | 0.004 (0.049) |
| 10-Year Post Election | -0.332*** (0.125) | -0.193 (0.140) | -0.080 (0.054) | -0.038 (0.060) |
| <i>Event-Time \times 50-100 % Vote Share</i> | | | | |
| 3-Year Post Election | -0.280** (0.131) | -0.286** (0.131) | -0.028 (0.052) | -0.029 (0.052) |
| 5-Year Post Election | -0.381** (0.149) | -0.389*** (0.149) | -0.052 (0.063) | -0.053 (0.063) |
| 10-Year Post Election | -0.271* (0.164) | -0.278* (0.164) | -0.071 (0.073) | -0.073 (0.073) |
| Panel B: Manufacturing | | | | |
| <i>Event-Time \times 0-50 % Vote Share</i> | | | | |
| 3-Year Post Election | -0.236 (0.159) | -0.145 (0.170) | 0.017 (0.061) | 0.035 (0.065) |
| 5-Year Post Election | -0.216 (0.187) | -0.072 (0.199) | -0.023 (0.076) | 0.025 (0.081) |
| 10-Year Post Election | -0.425* (0.226) | -0.210 (0.241) | -0.151 (0.097) | -0.049 (0.104) |
| <i>Event-Time \times 50-100 % Vote Share</i> | | | | |
| 3-Year Post Election | -0.462* (0.266) | -0.470* (0.266) | -0.009 (0.104) | -0.010 (0.104) |
| 5-Year Post Election | -0.394 (0.299) | -0.406 (0.299) | 0.008 (0.126) | 0.004 (0.126) |
| 10-Year Post Election | -0.559* (0.336) | -0.578* (0.336) | -0.162 (0.150) | -0.171 (0.150) |
| Exclude 50 % Elections | | X | | X |
| Industry + Employment Ctrls. | X | X | X | X |
| Flexible Ctrls. | X | X | X | X |

Note: This table presents estimates testing for linear trends by vote share in post-election outcomes. We test for trends separately across winning versus losing elections. The *Event-Time \times 0-50* rows present estimates of the θ coefficients from equation 11 and capture linear trends in post-election outcomes for losing elections. The *Event-Time \times 50-100* rows present estimates of $\theta + \tau$ and capture linear trends in post-election outcomes for winning elections. Since the specification separately includes an interaction with a winning election indicator, these slope estimates are in excess of any treatment effect right around the 50 % threshold. The outcome for the first two columns is establishment-level DHS employment growth relative to time -1 . The outcome for the last two columns is an indicator of whether the establishment exists at time t . All specifications include our flexible control specification (see Section 4 for details). See Appendix Table A2 for the same results with alternative included controls. The columns that “Exclude 50 % Elections” include an interaction between having a vote share of exactly 50 % and event time. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Employment and Survival Estimates by Industry, 20-80 % Vote-Share Elections

| Industry Group: | Manufacturing | | Services | | Other | |
|------------------------------------|----------------------|----------------------|---------------------|----------------------|----------------------|----------------------|
| Outcome: | DHS Emp | Survival | DHS Emp | Survival | DHS Emp | Survival |
| 5-Year Pre Election | 0.005 (0.015) | | 0.010 (0.012) | | 0.011 (0.016) | |
| 2-Year Pre Election | -0.013 (0.012) | | 0.017* (0.009) | | -0.009 (0.012) | |
| 5-Year Post Election | -0.174*** (0.029) | -0.047*** (0.012) | -0.057** (0.024) | -0.026*** (0.010) | -0.192*** (0.030) | -0.058*** (0.013) |
| 10-Year Post Election | -0.231*** (0.033) | -0.075*** (0.015) | -0.059** (0.027) | -0.017 (0.012) | -0.229*** (0.033) | -0.083*** (0.015) |
| Industry + Employment Ctrls. | X | X | X | X | X | X |
| Flexible Ctrls. | X | X | X | X | X | X |
| Industry Group Number of Elections | 6,000 | 6,000 | 8,000 | 8,000 | 5,000 | 5,000 |
| Industry Group Share of Elections | 0.302 | 0.302 | 0.414 | 0.414 | 0.284 | 0.284 |

Note: This figure plots the $\delta_{h,n}$ coefficients from estimating our heterogeneity specification in equation 12 for elections in three different broad industry groups. Manufacturing is defined as NAICS sectors 31-33, services are defined as NAICS 51-81 and retail trade (NAICS 44-45), and other is the remaining industries. Elections are classified into industries based on their Fort and Klimek (2016) NAICS 2012 codes. Otherwise, the sample, controls, and standard errors are the same as in Figure 3. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effects of Successful Elections on Firms' Other Establishments

| Outcome: | DHS Employment | Survival |
|------------------------------|--------------------|--------------------|
| 1-Year Post Election | 0.017 (0.016) | 0.006 (0.006) |
| 2-Year Post Election | 0.043** (0.019) | 0.012 (0.008) |
| 3-Year Post Election | 0.044** (0.021) | 0.022** (0.009) |
| 4-Year Post Election | 0.048* (0.025) | 0.015 (0.010) |
| 5-Year Post Election | 0.034 (0.025) | 0.023** (0.011) |
| Industry + Employment Ctrls. | X | X |
| Flexible Ctrls. | X | X |

Note: This table presents the DHS employment growth rate and survival estimates estimated as described for Figure 8. The DHS employment growth rate estimates exactly match the DHS employment estimates presented in that table.

Table 6: Election Delay Heterogeneity, Continuous Delay Time Specification

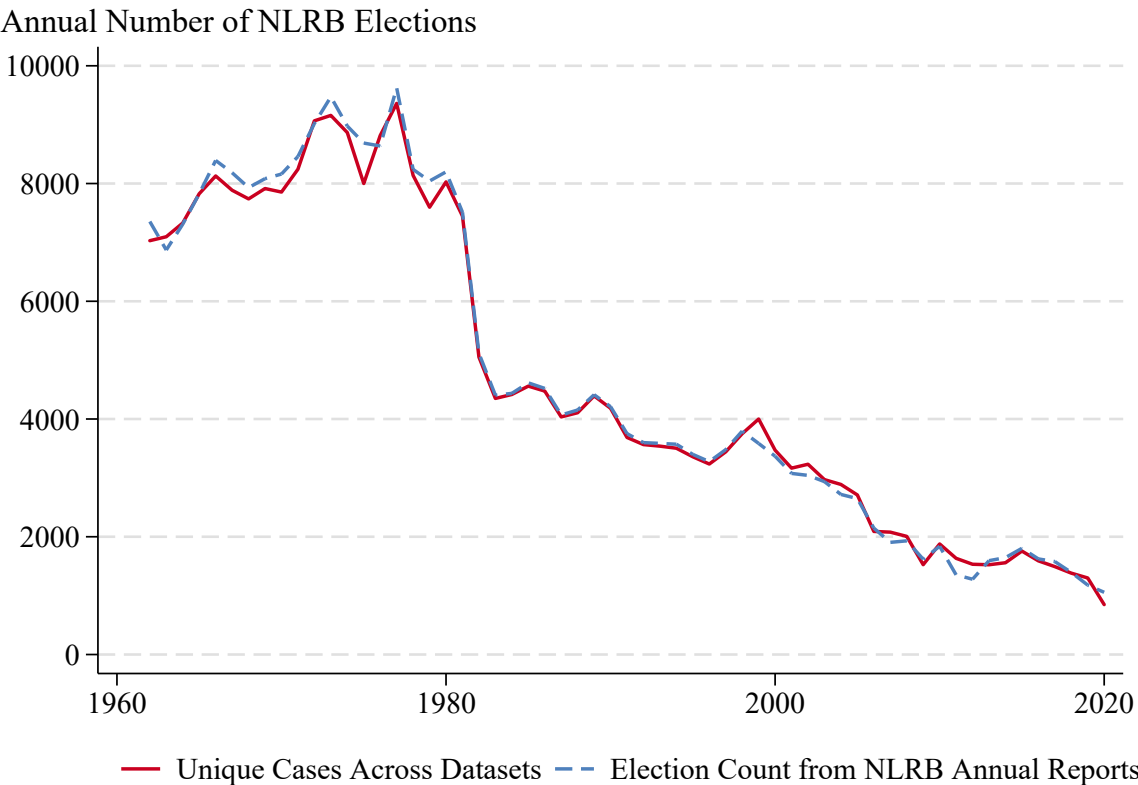
| Treatment: | Log Delay Time | | | | Residualized Log Delay | |
|------------------------------|---------------------|---------------------|---------------------|---------------------|------------------------|---------------------|
| Outcome: | DHS Emp | Survival | DHS Emp | Survival | DHS Emp | Survival |
| 3-Year Post Election | -0.124** (0.058) | -0.057** (0.023) | -0.117** (0.055) | -0.045** (0.022) | -0.120** (0.059) | -0.055** (0.023) |
| 5-Year Post Election | -0.121* (0.063) | -0.064** (0.026) | -0.104* (0.060) | -0.051** (0.025) | -0.113* (0.065) | -0.060** (0.026) |
| 10-Year Post Election | -0.147** (0.073) | -0.071** (0.033) | -0.152** (0.073) | -0.074** (0.032) | -0.132* (0.074) | -0.061* (0.033) |
| Industry + Employment Ctrls. | X | X | X | X | X | X |
| Pooled Ctrls. | | | X | X | | |
| Flexible Ctrls. | X | X | | | X | X |
| Observations | 6,000 | 6,000 | 6,000 | 6,000 | 6,000 | 6,000 |

Note: This table presents coefficient estimates from a modified version of specification 8. Specifically, we interact the treatment by event time indicators with the continuous *log delay time*. See Appendix C for details on how we calculate the delay time. The table reports the coefficients on these interactions at various time horizons. Thus, a survival coefficient of -0.05 means that the effect of successful unionization on survival is 0.5 pct. pts. higher for elections with a 10 % longer delay time. The first four columns use the raw number of days between petition filing and election dates to define the log delay time. For the last two columns, we first regress log delay time on within-year deciles of the election bargaining unit size and use the residuals from this regression as the interaction. The sample includes all elections at manufacturing establishments -10 to 10 years before and after each union election but we only include a subset of these coefficients. The even columns include the DHS employment growth rate relative to time -1 as the outcome variable (see Section 4 for their definition). The odd columns include an indicator for whether the establishment exists at time t as the outcome. Standard errors are clustered by establishments' firmid during the year of the election (e.g., the clustering variable is fixed over time for each establishment). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix for
Unionization, Employer Opposition, and Establishment Closure

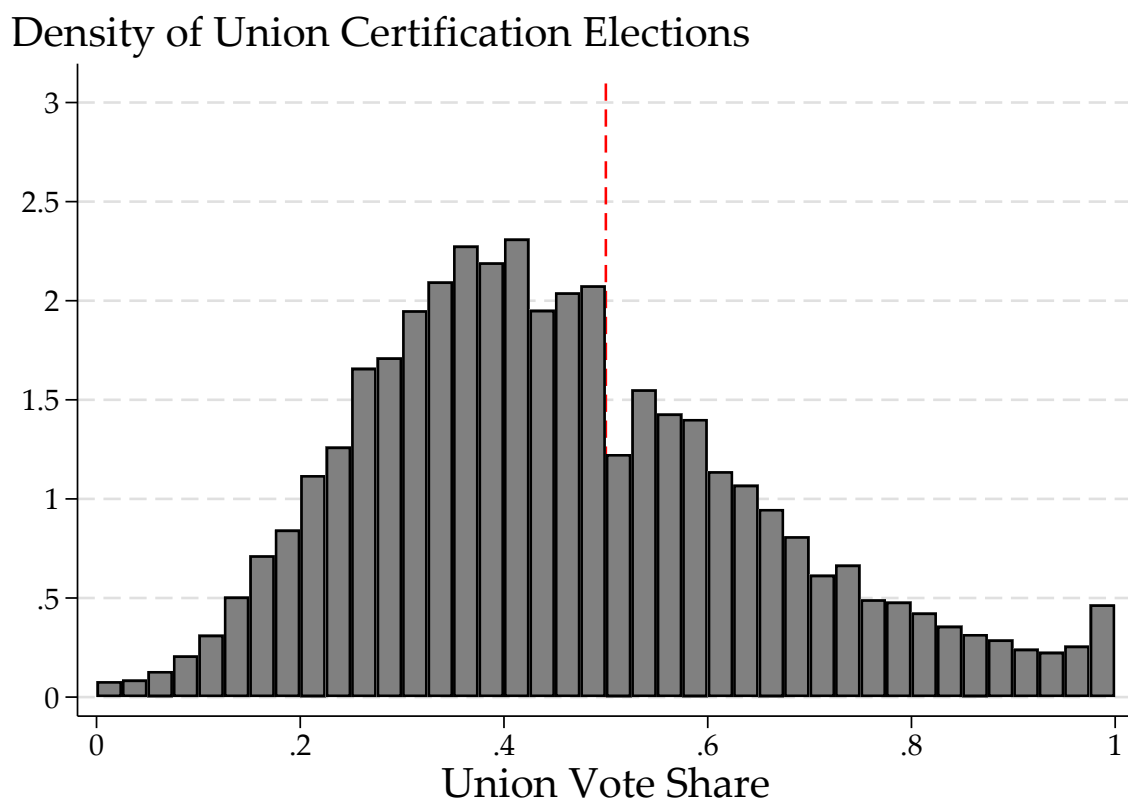
A Appendix Figures

Figure A1: Number of Unique Case Numbers Across Datasets versus NLRB Annual Reports



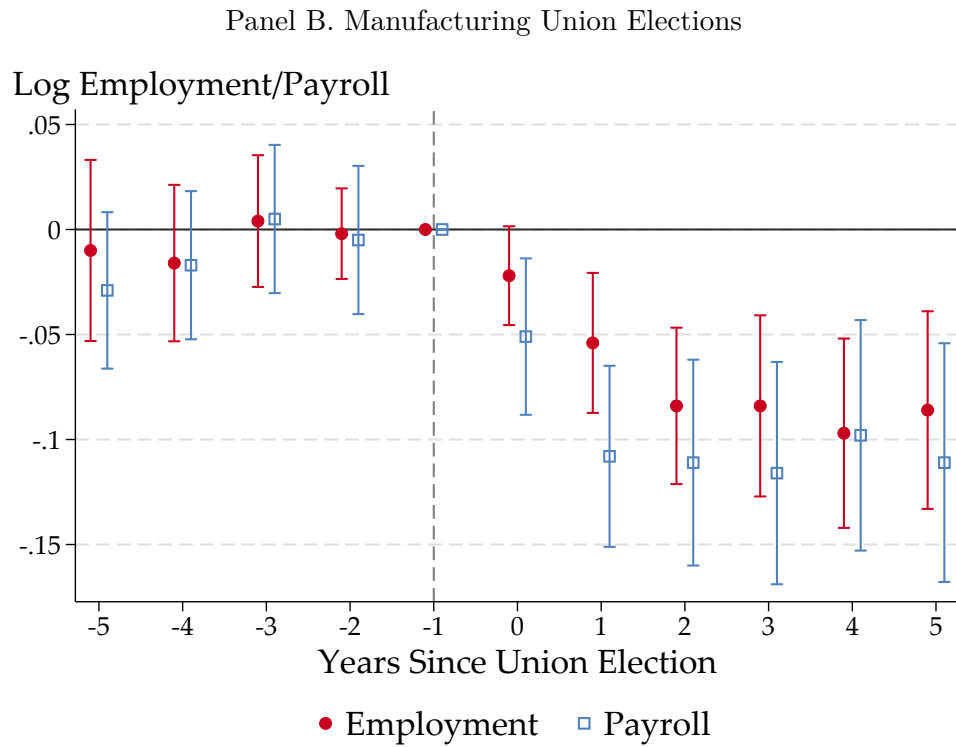
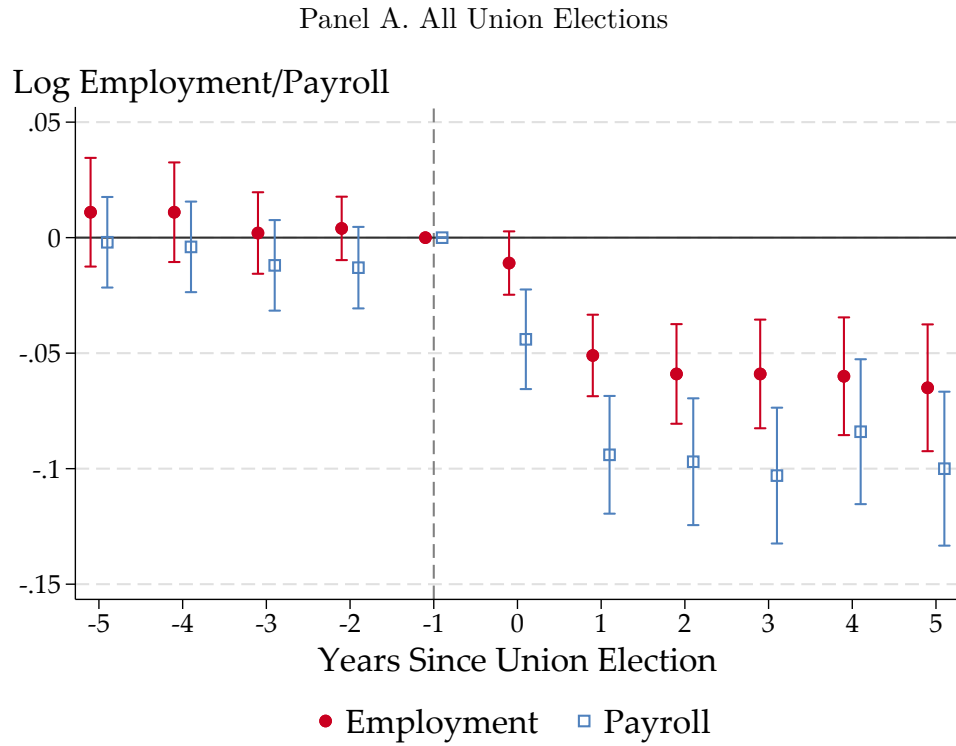
Note: This figure plots the total number of unique NLRB election cases each year in our data set and in the annual NLRB reports. These include all case types (e.g., ‘RC’ cases and non-RC cases). Our data set is from combining union election datasets from Henry Farber, J.P. Ferguson, and Thomas Holmes and publicly available data from the NLRB and picking one observation for each NLRB case number. See Appendix C for details on our data construction process.

Figure A2: Election Vote-Share Histogram, 50 + Vote Elections



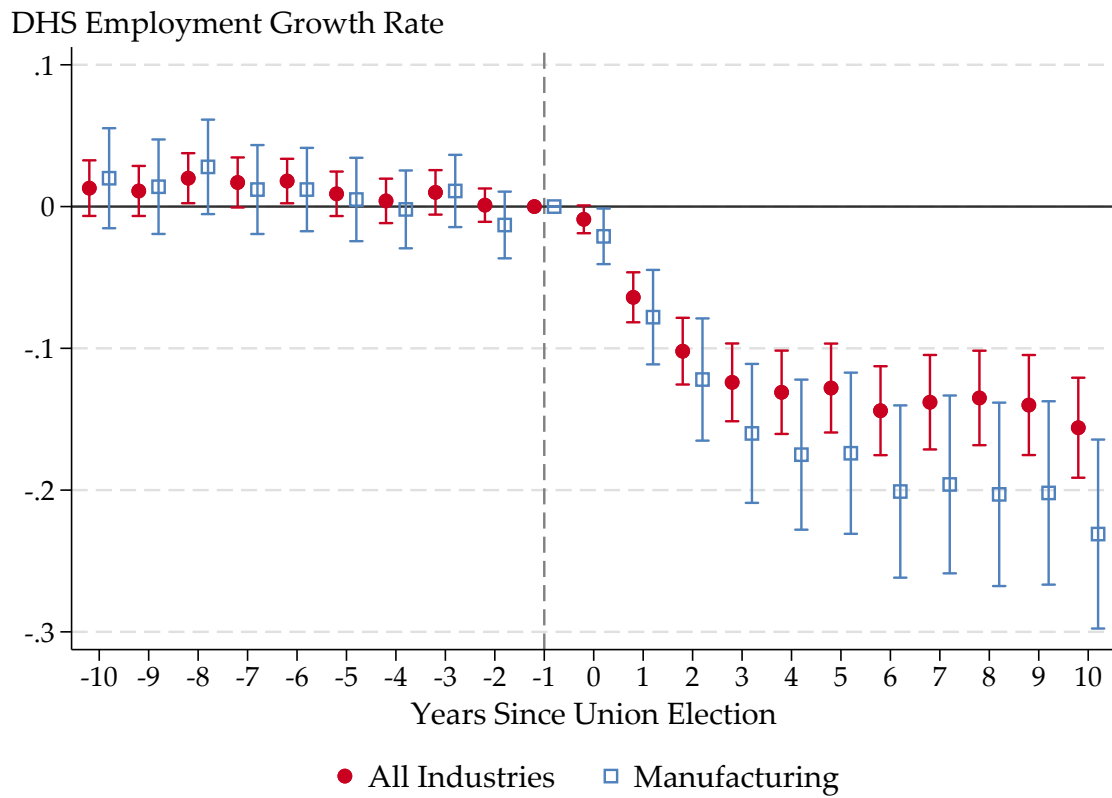
Note: This plots the vote-share histogram of elections with more than 50 total voters. Given the discreteness of the running variable and the fact that our sample includes elections with a small number of votes, it is difficult to detect manipulation from the vote-share density figure for the entire sample so we restrict the sample to elections with at least 50 votes. The figure was constructed using external union election data (e.g., not our final sample matched to the Census) but the sample was constructed to mirror the overall sample construction (see Appendix C for details). Note, there may still be a small bias from the “integer problem” described in [DiNardo and Lee \(2004\)](#) that could lead to an excess mass of elections right below 50 % but simulations suggest that it is quite small with at least 50 votes.

Figure A3: Log Employment and Payroll Estimates, 20-80 % Vote-Share Elections



Note: This figure plots estimates from the *Flexible Controls* specification presented in Figure 3 Panel B. and Figure 4 Panel B. The log employment estimates are identical to the estimates in Figures 3 and 4 but the log payroll estimates are not otherwise reported.

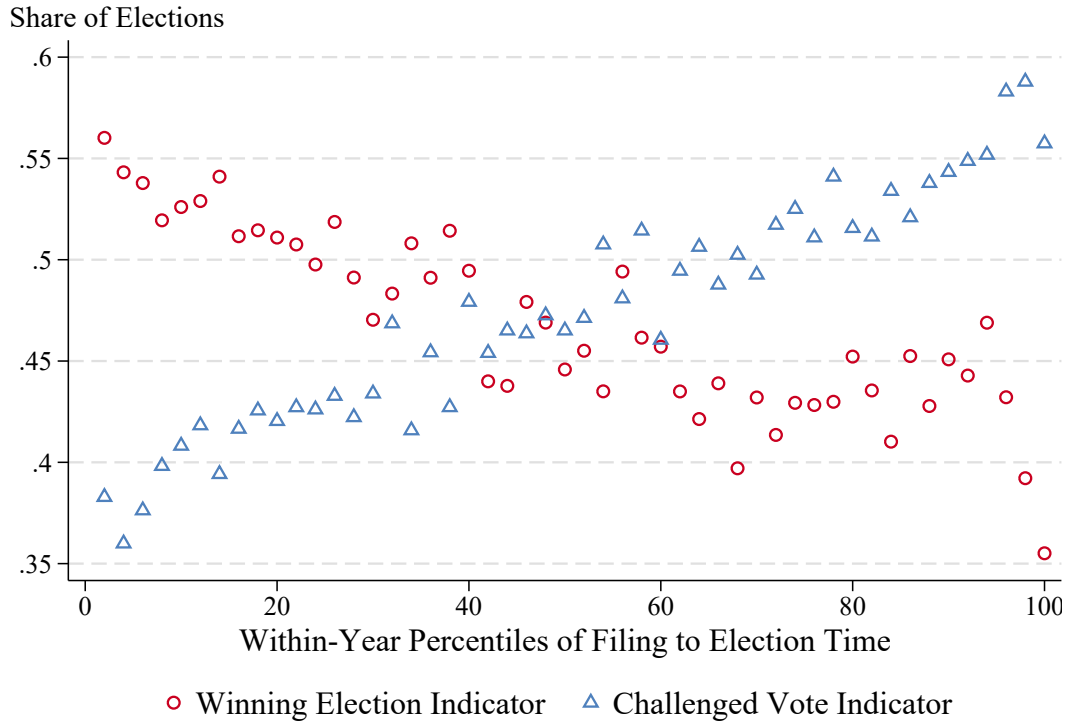
Figure A4: DHS Employment Estimates, 20-80 % Vote-Share Elections, 10 Yr Pre- and Post-Periods



Note: This figure plots the same DHS employment growth rate estimates as in Figure 3, Panel C and Figure 4, Panel C but includes the -10 to -5 pre-period estimates and the 6 to 10-year post-period estimates. Note, the panel is balanced from -5 years pre-election to 10 years post-election but not from -10 to -5 years pre-election. Consequently, each of the -5 to -10 point estimates average over slightly different cohorts.

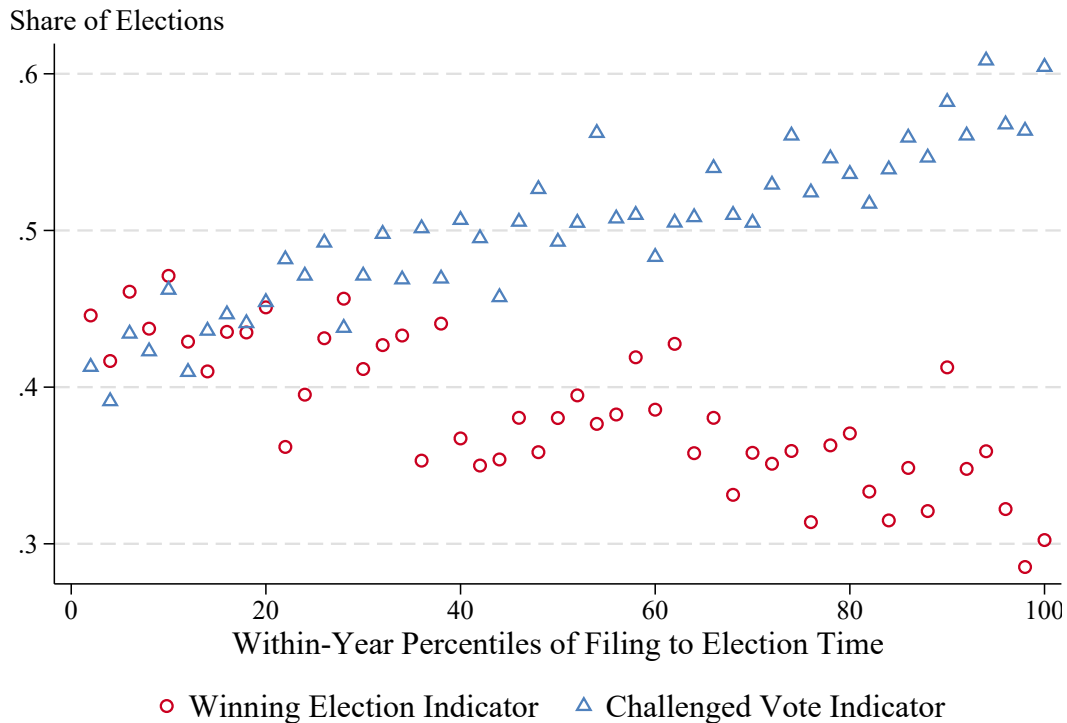
Figure A5: Election Win Rates and Challenged Vote Rates by Delay Time

Panel A. All Elections



Conditional Regression Coefficients x 100: Winning = $-.088$ (.007). Challenged = $.067$ (.007).

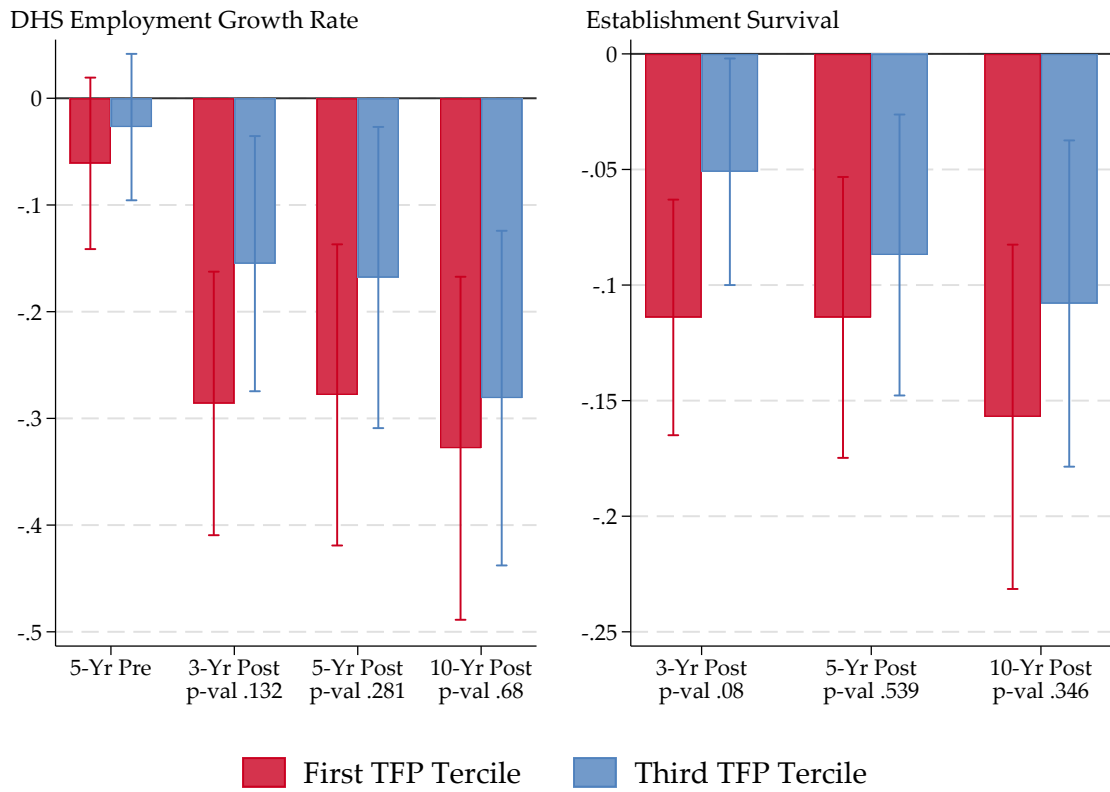
Panel B. Manufacturing



Conditional Regression Coefficients x 100: Winning = $-.063$ (.013). Challenged = $.054$ (.013).

Note: This figure plots the relationship between pre-election delay times, election win rates, and challenged votes in elections. Pre-election delay times are defined as the number of days between the election petition being filed and the election date. We then take the within-year percentiles of the election delay distribution and plot this on the x-axis. The share of elections with a challenged vote is defined as an indicator for any vote in the election being challenged. The sample of elections includes all elections in our “external elections dataset” described in Appendix C. The conditional regression coefficients are from regressing the election win indicator (or challenged vote indicator) on deciles of the number of eligible voters in the election, four-digit NAICS industry fixed effects, and election state fixed effects.

Figure A6: Establishment-Level Total Factor Productivity Heterogeneity, Multi-Establishment Firms



Note: This figure plots the same estimates as in Figure 11 except restricting the TFP comparison to only be between multi-establishment firms in different TFP terciles. As with the other heterogeneity tests, the sample includes all manufacturing elections and pools the controls across the entire sample.

B Appendix Tables

Table A1: Union Election Matched Sample Construction

| | All Elections | | Winning Elections | |
|---|---------------|-----------------|-------------------|-----------------|
| | Elections | Eligible Voters | Elections | Eligible Voters |
| Panel A: NLRB Election Sample | | | | |
| All Election, 1981-2005 | 94,824 | 5,991,865 | 44,492 | 2,461,138 |
| Representation Elections (RC) | 77,349 | 5,111,675 | 39,397 | 2,071,859 |
| > 5 Eligible Voters | 69,789 | 5,084,061 | 34,247 | 2,053,210 |
| Non-Contested Elections | 66,353 | 4,590,121 | 31,378 | 1,668,877 |
| Panel B: Final NLRB Sample Industry Shares | | | | |
| Manufacturing | 0.307 | 0.408 | 0.253 | 0.307 |
| Other | 0.266 | 0.186 | 0.263 | 0.177 |
| Services | 0.426 | 0.405 | 0.484 | 0.515 |
| Panel C: Matched Census Sample | | | | |
| Elections Matched to Census Establishments | 46,000 | | | |
| Final Establishment-Level Outcome Sample | 27,000 | | | |
| 20-80 % Election Sample | 19,000 | | | |

Note: This table illustrates how our specific sample restrictions change the number of elections and eligible voters we have in our sample. Panel A plots the total number of elections and eligible voters for all elections and specifically for winning elections. The first row in Panel A. includes all unique NLRB cases with filing dates between 1981-2005 (the main years in our sample). The second row only includes representation (RC) elections. The third row drops elections without more than five eligible voters. The fourth row only includes non-contested elections (e.g., elections with one union on the ballot). Panel B. presents the industry composition of the remaining elections from the fourth row of Panel A. Note we use the NLRB election industry codes here rather than the LBD industry codes but the overall industry shares are reassuringly similar to the industry shares in Table 4. The three columns represent the total shares of elections and eligible voters for all elections and winning elections. Panel C shows our final sample sizes from the matched Census data. The sample restrictions between "Elections Matched to Census Establishments" and "Final Establishment-Level Outcome Sample" include keeping (1) the first election at each establishment, (2) at least three years of pre-election survival, (3) non-missing employment, payroll, and other controls at event time $t = -1$.

Table A2: Post-Election Outcome Trends by Vote Share, 20-80 % Vote-Share Elections, Employment and Industry Ctrls.

| Industry Group: | All Industries | | Manufacturing | |
|---|----------------------|-------------------|---------------------|-------------------|
| Outcome: | DHS Emp | Survival | DHS Emp | Survival |
| <i>3-Year Post Election</i> | | | | |
| Event-Time \times 0-50 % Vote Share | -0.134 (0.100) | 0.021 (0.037) | -0.181 (0.159) | 0.042 (0.059) |
| Event-Time \times 50-100 % Vote Share | -0.361*** (0.126) | -0.052 (0.051) | -0.543** (0.250) | -0.035 (0.099) |
| <i>5-Year Post Election</i> | | | | |
| Event-Time \times 0-50 % Vote Share | -0.119 (0.116) | -0.009 (0.047) | -0.150 (0.187) | 0.023 (0.076) |
| Event-Time \times 50-100 % Vote Share | -0.450*** (0.141) | -0.085 (0.060) | -0.537* (0.275) | -0.033 (0.116) |
| <i>10-Year Post Election</i> | | | | |
| Event-Time \times 0-50 % Vote Share | -0.218 (0.133) | -0.052 (0.057) | -0.186 (0.224) | -0.020 (0.097) |
| Event-Time \times 50-100 % Vote Share | -0.354** (0.157) | -0.107 (0.070) | -0.676** (0.309) | -0.209 (0.140) |
| Exclude 50 % Elections | X | X | X | X |
| Industry + Employment Ctrls. | X | X | X | X |
| Flexible Ctrls. | | | | |
| Number of Elections | 19,000 | 19,000 | 6,000 | 6,000 |

Note: This table presents the same estimates as in Tables 3 but only includes the baseline industry and employment controls. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Employment and Survival Bargaining Unit Share Interaction, 20-80 % Vote-Share Elections

| Outcome: | DHS Employment | Survival |
|--|----------------------|----------------------|
| 3-Year Post Election \times Bargaining Unit Share | -0.109** (0.044) | -0.046*** (0.017) |
| 5-Year Post Election \times Bargaining Unit Share | -0.132*** (0.051) | -0.041* (0.021) |
| 10-Year Post Election \times Bargaining Unit Share | -0.057 (0.057) | -0.015 (0.025) |
| Industry + Employment Ctrls. | X | X |
| Flexible Ctrls. | X | X |

Note: This table presents estimates from the same specification as Figure 3 for DHS employment growth rates except that we add (1) an interaction between the event-time \times win indicators with the share of the establishment's employment covered by the bargaining unit and (2) an interaction just between event-time indicators and the bargaining unit share. We report the interactions in (1) for three, five, and ten years post-election. Consequently, this specification estimates how treatment effects increase with the bargaining unit share, accounting for overall post-election trends across all elections by bargaining unit share. A survival estimate of -0.05 means that increasing the share of the establishment covered by the bargaining unit by 10 % leads to an additional 0.5 pct. increase in establishment exit.

Table A4: Manufacturing versus Services Employment and Survival Estimates, Robustness Checks

| Specification: | Baseline | | Pooled Controls | | Good Matches | | > 25 % Barg Unit Share | | 30-70 % | |
|------------------------------|----------------------|----------------------|----------------------|---------------------|----------------------|----------------------|------------------------|---------------------|----------------------|---------------------|
| Outcome: | DHS Emp | Survival | DHS Emp | Survival | DHS Emp | Survival | DHS Emp | Survival | DHS Emp | Survival |
| 5-Year Difference | -0.117*** (0.037) | -0.021 (0.016) | -0.118*** (0.035) | -0.022 (0.015) | -0.144*** (0.044) | -0.024 (0.018) | -0.121*** (0.041) | -0.028* (0.017) | -0.132*** (0.043) | -0.026 (0.018) |
| 10-Year Difference | -0.172*** (0.043) | -0.058*** (0.019) | -0.159*** (0.04) | -0.05*** (0.018) | -0.196*** (0.05) | -0.061*** (0.022) | -0.193*** (0.047) | -0.06*** (0.021) | -0.171*** (0.049) | -0.054** (0.022) |
| Industry + Employment Ctrls. | X | X | X | X | X | X | X | X | X | X |
| Pooled Ctrls. | | | X | X | | | | | | |
| Flexible Ctrls. | X | X | | | X | X | X | X | X | X |

Note: This table presents robustness results for the differences between the service-sector and manufacturing results in Table 4. Specifically, it presents the differences between the five- and ten-year DHS employment growth rate and survival estimates for various alternative specifications. The first two columns present the differences for the estimates presented in Table 4. The "Pooled Controls" columns pool the controls across all cohorts as described in Section 4. The "Good Matches" columns restrict to election matches which we give a 95 % rating (see Appendix C for details). The "Barg Unit Share" columns restrict to elections where the bargaining unit is at least 25 % of the total establishment employment. The 30-70 % columns restrict to elections with 30-70 % of the vote share. For all specifications with restrictions, we still use the entire sample for controls but restrict the treated variables to be estimated from the restricted sample. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Single- Versus Multi-Establishment Firm Heterogeneity, Robustness Checks

| Specification: | Baseline | | Pooled Controls | | 30-70 % | |
|------------------------------|---------------------|---------------------|--------------------|----------------------|-------------------|---------------------|
| Outcome: | DHS Emp | Survival | DHS Emp | Survival | DHS Emp | Survival |
| 5-Year Difference | -0.068 (0.058) | -0.061** (0.024) | -0.063 (0.054) | -0.053** (0.023) | -0.034 (0.065) | -0.057** (0.027) |
| 10-Year Difference | -0.149** (0.066) | -0.093*** (0.03) | -0.13** (0.062) | -0.085*** (0.028) | -0.067 (0.075) | -0.06* (0.034) |
| Industry + Employment Ctrls. | X | X | X | X | X | X |
| Pooled Ctrls. | | | X | X | | |
| Flexible Ctrls. | X | X | | | X | X |

Note: This table presents robustness results for the differences between single- and multi-establishment firms presented in Figure 7. Specifically, it presents the differences between the five- and ten-year DHS employment growth rate and survival estimates for various alternative specifications. The first two columns present the differences for the estimates presented in Figure 7. The "Pooled Controls" columns pool the controls across all cohorts as described in Section 4. The 30-70 % columns restrict to elections with 30-70 % of the vote share. For all specifications with restrictions, we still use the entire sample to estimate controls but restrict the treated variables to be estimated from the restricted sample. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Unionized versus Non-Unionized Firm Heterogeneity, Robustness Checks

| Specification: | Baseline | | Pooled Controls | | Contracts since 1990 | | 30-70 % Elections | |
|------------------------------|----------------------|---------------------|----------------------|--------------------|----------------------|---------------------|---------------------|--------------------|
| Outcome: | DHS Emp | Survival | DHS Emp | Survival | DHS Emp | Survival | DHS Emp | Survival |
| 5-Year Difference | -0.187** (0.095) | -0.03 (0.041) | -0.139 (0.089) | -0.018 (0.039) | -0.179 (0.112) | -0.057 (0.049) | -0.197* (0.108) | -0.029 (0.046) |
| 10-Year Difference | -0.336*** (0.104) | -0.108** (0.048) | -0.287*** (0.097) | -0.09** (0.045) | -0.412*** (0.121) | -0.149** (0.058) | -0.305** (0.119) | -0.098* (0.055) |
| Industry + Employment Ctrls. | X | X | X | X | X | X | X | X |
| Pooled Ctrls. | | | X | X | | | | |
| Flexible Ctrls. | X | X | | | X | X | X | X |

Note: This table presents robustness results for the differences between multi-establishment firms with and without any unionized establishments presented in Figure 9. Specifically, it presents the differences between the five- and ten-year DHS employment growth rate and survival estimates for various alternative specifications. The first two columns present the differences for the estimates presented in Figure 9. The "Pooled Controls" columns pool the controls across all cohorts as described in Section 4. The "Contracts since 1990" column only classifies firms as unionized versus non-unionized starting in 1990. This gives all firms at least five years of pre-election FMCS contract data that we can use to define the firms' unionization status. The 30-70 % columns restrict to elections with 30-70 % of the vote share. For all specifications with restrictions, we still use the entire sample to estimate controls but restrict the treated variables to be estimated from the restricted sample. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Data and Matching Details Appendix

NLRB Union Election Data

Union Election Data Sources We combine datasets on NLRB elections from Henry Farber, J.P. Ferguson, and Thomas Holmes and publicly available data from the NLRB to give us a near-complete set of union elections from 1961-2019. Internet links for the [Ferguson](#), [Holmes](#), and [NLRB](#) are available. For more details about the sources of these data see JP Ferguson’s website [here](#).

NLRB Election Case Numbers The ID variable in the election data is an NLRB Case ID Number. This case number is assigned after an election petition is first filed. A single case number, however, could include multiple different vote counts. For example, there might be (1) multiple different tallies of the same election or (2) multiple elections for the same case number.⁶³ Additionally, there might be separate elections for multiple different bargaining units filed under the same case number (e.g., if a union initially filed for a petition for one bargaining unit but the NLRB then split bargaining unit). Consequently, it is important to pick the vote count that actually corresponds to the outcome of the certification election. Finally, since the different data sources cover overlapping time periods, we have multiple observations of the same case number in different datasets.

We deal with multiple observations per case number within datasets somewhat differently for the different data sources. For the public NLRB data (the “Public Data”) there is information indicating why there are multiple observations for a single case number. Consequently, for a given bargaining unit, we pick the final tally of the last election for each case number. This ensures that we take the vote tally that determines the unions’ certification for cases where there are multiple counts of the same election or multiple ordered elections for the same bargaining unit. Within each case number, we then take the results from the election at the largest bargaining unit in cases where there are distinct bargaining units for a single case. For the other datasets, there is somewhat less clarity about why there are duplicate observations within the same case number. For these datasets, we first pick the observation with the last election date and then the observation with the largest bargaining unit size.

This leaves us one observation per case number within each data set but duplicates across datasets. We take one observation per case number across datasets. For picking a single case number per dataset, we deprioritize observations in the Farber data given data irregularities in those data. Additionally, we prioritize the public data because we have more confidence that we are picking the correct observation across duplicates within the same case number.

Variables in the Election Dataset We define the following from the union election data that we use for our analysis and for our matching algorithm

⁶³There could be multiple tallies for the same election due to challenged votes (e.g., the first tally would not include challenged votes while the final tally would include challenged votes that were determined to be valid). There could be multiple elections for the same case number if an NLRB director orders a second election due to objections to the first election

- **Election City, State, and Address:** The data contain the city and state of the election that we use to match each election to an establishment in the LBD. For many observations, we also observe a street address that we also use for the matching.

For the “public data”, we observe an address for the employer and for the election site. There are two conceptual reasons why these addresses might be different. First, the election might not be held at the employers’ location.⁶⁴ This suggests that the employers’ address is better for name and address matching to Census establishments. Second, the listed address for the employer might be a corporate headquarters rather than the establishment where the bargaining unit works. This suggests that the election address is better for name and address matching. Since it is not conceptually clear which address to use, we check which address is more likely to match the text in the bargaining unit description (e.g., “all warehousemen at its Louisville, KY facility”). We find that the election site address is more likely to match address information in the bargaining unit description and consequently use the election site addresses when they disagree.

- **Election Vote Shares:** We define election vote shares as the number of votes for the union divided by the total number of votes in the election. This differs from the adjusted vote shares constructed in DiNardo and Lee (2004) and Frandsen (2021) to address the “integer problem” with constructing vote shares⁶⁵. We do not apply this adjustment for two reasons. First, the integer problem is especially problematic for regression discontinuity designs but less of an issue with our difference-in-differences design. Second, since we don’t impose any restrictions on the number of votes cast in the election, the adjustment proposed in DiNardo and Lee (2004) would lead to larger changes in our vote shares (e.g., a six-person election would be adjusted from 50 % to 41.7 %).
- **Contested Elections:** We define contested elections as elections with multiple unions on the ballot. We drop these elections for two reasons. First, these elections are often “union raids” where one union already represents a specific bargaining unit and another union challenges that union for representation (Sandver and Ready, 1998). Consequently, a winning election, in this case, would not lead to a switch from the establishment being non-unionized to unionized but instead just a switch in which union represents the bargaining unit. Second, the reported vote totals for multi-union elections may not actually represent the workers’ support for the union. In particular, for multi-union elections, if none of the options (e.g., “union 1”, “union 2”, or “no union”) receive the majority of the votes, a runoff election is held between the highest two options (Fraundorf, 1990). Consequently, the unions’ true support (the union vote share from the first election) may be different than the unions’ support in the observed runoff election results.

⁶⁴For example, when strikes, pickets, or lockouts are in progress, the election may be held at a neutral location (NLRB, 2020). As another example, when the employers’ location is different than the employees’ worksite (e.g., security guards), the election might be held at the work site

⁶⁵The integer problem refers to the fact that since vote shares are based on a discrete number of votes, there will be a mechanical discontinuity in the number of elections with exactly 50 % vote shares

- **Election Industry:** The election data contain industry codes indicating the industry of the election analysis. For our main analysis, we use the Census industry codes for the establishments we match each election to. For some of our analysis of the unmatched NLRB data (e.g., Figures 2 and A5 and Tables A1), we use the election industry codes to split up manufacturing and non-manufacturing elections. Since the industry codes in the election data come from different vintages (e.g., SIC versus NAICS industry codes), we use the modal employment-weighted industry crosswalks from Eckert et al. (2020) to crosswalk the industry codes to consistent NAICS 2012 industry codes.
- **Bargaining Unit Size and Share of Total Employment:** We define the **bargaining unit size** as the number of eligible voters from the NLRB election data. We define the **bargaining unit share of total employment** as the bargaining unit size divided by the establishment-level employment one year for the union election. Since we do not impose that the bargaining unit is smaller than the establishment, we cap the share at one.
- **Election Filing Date:** We define treatment timing based on the date that the election was filed. To maximize the number of observations that we observe election filing dates for, we pull the dates across case numbers when some observations are missing from one dataset (e.g., if the filing date is only available for a case in the Ferguson data but not the Farber data, we pull date from the Ferguson to Farber data). For five % of elections, we do not observe the filing date and instead use the election or case closing date.
- **Election Delay Time:** We define delay time as the number of days between the date the election petition was filed to the NLRB and the date the election was held. The availability of exact dates for these two concepts varies somewhat across time and datasets. Both dates are missing from the Farber data which is one reason why we prioritize the other datasets when duplicates across case numbers are available. However, as described above, we pull both dates across datasets when they are missing for some observations. For the Ferguson and Holmes data, the delay time is missing for cases that closed in 1982 and we only have a monthly measure for 1981 and part of 1983. These differences over time motivate our checks that the heterogeneity by delay time holds using both variation within-years (e.g., the within-year tercile measures) and across years (e.g., the continuous log specification). Additionally, there may have been some institutional changes over time that we do not want to include (e.g., the “Quickie Election Rule” decreased delay times but is not in our sample of elections).

FMCS Contract Data

We combine contract data from Thomas Holmes for 1984-2003 and from the FMCS for 1997-2019. The Holmes data are available [here](#) and the FMCS data are partially available [here](#) and the rest were obtained via a FOIA request. They include both notices of *initial contracts* (i.e., first-contract negotiation after an election) and *contract renegotiation or reopening* for existing contracts. There are

two reasons that these contract notices likely underrepresent the universe of unionized establishments in the U.S. First, these “notices of bargaining” are provided to the FMCS so it can be ready to provide mediation. Although filing is legally incentivized, underreporting is possible. For example, an employer changing the terms of employment or a union striking without first filing a notice could be violating labor law. Second, some contract notices may represent a contract covering multiple establishments as we always only match each contract to one establishment.⁶⁶

There are duplicate observations both across the Holmes versus FMCS datasets and within each dataset.⁶⁷ However, unlike the NLRB election data, we have no IDs to restrict the dataset to unique observations. Consequently, to deal with duplicates, we match all contract observations to the Census establishments in the LBD and drop duplicates when multiple contract observations match to the same Census establishment.

We use the contract data to define

- **Previous contract at an establishment:** for each election establishment, we define an indicator for whether the establishment has a previous FMCS contract ever matched to the same establishment (e.g., indicating that another bargaining unit was already unionized at this establishment). To avoid contract matches related to the union election, we only include matched contracts starting one year before the election.
- **Unionized versus Non-Unionized Firms:** we define a firm as being (partially) unionized if at time t any of the establishments in the same FIRMID had an FMCS contract match in the current or previous five years. For the unionized versus non-unionized firm heterogeneity check, we also include elections at establishments with a previous contract (defined above) as unionized firms.

FMCS Works’ Stoppage Data

For Figure 2 Panel D., we use works’ stoppage data from the FMCS from 1984-2005. The data are available [here](#). They include both strikes and employer-initiated lockouts. We match the works stoppages to the election data based on exact company names and cities rather than the *Soft TF-IDF* algorithm we use for the main analysis. Prior to matching, we use the same cleaning algorithms described below to clean the employer and city names in the FMCS works’ stoppage data.

Longitudinal Business Database

In Section 3, we mention potential concerns with how the LBD allocates employment across establishments at multi-establishment firms that could bias our results. To be more precise about the issue, while the LBD is an establishment-level dataset, some of the employment and payroll input

⁶⁶Sometimes the FMCS contract notices explicitly mention that they apply to multiple locations (e.g., the address indicating various locations). In these cases, we will still only match the contract notice to one establishment if there is alternative location data available.

⁶⁷The across-dataset duplicates come from the fact that the datasets overlap. The within-dataset duplicates could come from an employer and union submitting an FMCS notice for the same contract.

data are received at higher levels of aggregation (e.g., at the EIN level). For example, one source used to construct the LBD is *IRS form 941s* that provide annual employment and payroll at the EIN-level which can cover multiple establishments. The Census uses an imputation model to allocate these EIN-level measures across establishments. This model primarily imputes employment changes across establishments based on their past employment. Consequently, employment changes at an establishment part of a multi-establishment firm might initially be allocated across all establishments. Thus, the LBD would initially underestimate the establishment-level decrease in employment. To correct some of these mistakes, the Census receives establishment-level information from the Company Organization Survey (COS), Economic Censuses, and Annual Survey of Manufacturers (ASM) that provide more accurate measures of establishment-level employment and survival. These alternative surveys are not, however, conducted for all establishments annually (e.g., the Economic Census is only conducted every five years). So there might be a few years lag before the LBD reports the correct establishment employment and exit. This lag mirrors the spike in establishment births and deaths every five years during the economic census years when the Census has establishment-level data for each establishment ([Jarmin and Miranda, 2002](#)). See [Chow et al. \(2021\)](#) for details about these issues with the LBD construction.

We use the LBD to define the following establishment-level variables

- **Employment:** total number of employees who received wages or other compensation during the pay period that included March 12th.
- **Payroll:** total “wages, tips, and other compensation” for employees over the entire year.
- **Establishment Survival:** indicator for whether the establishment has positive employment for at least one year in the future and in the past. Consequently, an establishment that has 50 employees one year, 0 employees the next, and 50 employees the following year would be defined as a “survivor” in the intermittent year. Since the LBD only measures March 12 employment, these establishments could be true survivors (e.g., seasonal businesses).
- **Establishment-Level NAICS Codes:** We classify each establishment into a 2012 NAICS industry using the [Fort and Klimek \(2016\)](#) NAICS codes.

Plant-Level TFP from the Annual Survey of Manufacturers

We define plant-level productivity using inputs and outputs from the Annual Survey of Manufacturers (ASM) and TFP measures calculated by [Foster et al. \(2016\)](#). To classify each election into different terciles of the plant productivity distribution, we first take all ASM observations, with and without union elections, with non-missing TFP and calculate year by NAICS 6 industry TFP percentiles. For each of our manufacturing union elections, we then assign the election the plant’s most recent TFP percentile in the previous five years (e.g., if the establishment was sampled by the ASM in year $E_i - 2$ but not $E_i - 1$, we assign the establishment its $E_i - 2$ productivity rank). Based on the

election observations with defined TFP, we then classify the elections into within-year terciles based on these rankings.

Matching Elections, Contracts, and LBD Establishments

Our data on union elections and contract notices contain information on the name and location of the employer, but no unique identifiers (like EIN) that could use to directly link the establishments to administrative Census data firms. We instead use a fuzzy-matching algorithm to link each election or contract to its corresponding Census record from the Standard Statistical Establishment List/Business Register. The algorithm is based on the name and geographic similarity of establishments. Our algorithm is based upon the Soft TF-IDF approach used by [Kline et al. \(2019\)](#), but extends their approach to incorporate the additional address data.

Name and Address String Cleaning: We start by standardizing and cleaning the name and address strings. Our cleaning procedure builds on the `stnd_compname` and `stnd_address` Stata name standardization programs ([Wasi and Flaaen, 2015](#)). We clean addresses as follows:

1. Remove most symbols, non-numeric or letter characters, and non-standard ASCII characters.
2. Removed PO boxes, building/suite/room numbers, and company names at the start of addresses (e.g., **GENERAL SUPPLY COMPANY** 2651 1ST STREET.)
3. Standardize common address and city name strings (e.g., **ST** \Rightarrow **STREET**, **TWENTY FIRST** \Rightarrow **21ST**, and **LIC** \Rightarrow **LONG ISLAND CITY**) and correct common address and city misspellings.

We clean the employer names as follows

1. Remove most symbols, non-numeric or letter characters, and non-standard ASCII characters.
2. Remove the portion of company names in parentheses. The union election data often contain supplemental information in the parentheses portion of the name (e.g., (**wage employees only**)).
3. Remove the portion of company names following **DOING BUSINESS AS (DBA)** or **A DIVISION OF**
4. Combine consecutive singleton letters and symbols separated by spaces (e.g., **A T & T** \Rightarrow **AT&T** and **D R HORTON** \Rightarrow **DR HORTON**).
5. Remove company entity types (e.g., **CORP**, **INC**, etc.), articles, and standard common company names (e.g., **MANUFACTURERS** \Rightarrow **MANUFACTURING**).

Election, Contract, and Census Address Geocodes: We geocode all addresses. This allows us to construct measures of address similarity based on the geographic distance between two addresses. We use geographic distance rather than string distance to measure address similarity because there may be addresses with very similar strings that are very different addresses (e.g., 100 Main St. may be very far away from 10 Main St.).

For the election and contract data, we first try to geocode all addresses with the [Census Bureau’s Geocoding API](#) because these geocodes are the most likely to match the Census’s internal geocodes. For the observations where the Census’s geocoder cannot find a geocode, we try the [geocodio geocoder](#). When an observations’ street address is missing or we cannot geocode it, we take the city/state geocode or the zip-code geocode.

For the Census data, we use the geocodes in the SSEL/Business Register ([DeSalvo et al., 2016](#)). These geocodes, however, are only available since 2002 ([Akee et al., 2017](#)). For observations where we do not have a geocode we first try to match it to a geocoded address. If the same address was not geocoded from 2002-2016, we instead take the average geocode of all addresses we see in 2002-2016 in the same city/state or zip code.

Matching Algorithm We implement a matching algorithm based on the string similarity of the cleaned employer names and the geographic distance between geocoded addresses. The standard Soft TF-IDF algorithm computes a match score between two firm names that is increasing in their string similarity. The algorithm is particularly suitable for our application since it overweights similarities in uncommon words between the two names and discounts similarities in common words. Although it’s possible to match the unionization records to the Census data based on employer name similarity alone, the procedure is likely to generate false establishment matches (especially given that establishments at multiunit firms may all share the same name, like ”CVS” or ”Starbucks”). Consequently, we instead also incorporate the geography information to distinguish between these potential matches.

We implement our matching algorithm as follows

1. For each election, we take all Census establishments in the same state that share at least one common word.⁶⁸
2. For each election-establishment pair, we calculate the Soft TF-IDF similarity measure between the employer name strings. Specifically, let A_j be the set of all words in the election name string and B_k be the set of all words in the establishment name string. The total number of election names is J and the total number of Census names is K . The Soft TF-IDF distance is defined as

$$s_{jk} = \text{Soft TF-IDF}(A_j, B_k) = \sum_{w \in A_j} \text{weight}(w, A_j) \times \text{m-score}(w, B_k) \quad (\text{A1})$$

⁶⁸We require that the establishments share at least one common word because this vastly reduces the number of string and distance calculations we need to make. For single-word companies, we only require that the potential matches share the same first letter. This allows us to match single-word establishments even with misspellings

where $\text{weight}(w, A_j)$ is defined as

$$\text{weight}(w, A_j) = \text{TF}(w, A_j) \times \text{IDF}(w, A, B) \Bigg/ \left[\sum_{w' \in A_j} (\text{TF}(w, A_j) \times \text{IDF}(w, A, B))^2 \right]^{1/2} \quad \text{where} \quad (\text{A2})$$

$$\text{TF}(w, A_j) = \frac{\text{freq}(w, A_j)}{\sum_{w' \in A_j} \text{freq}(w', A_j)} \quad \text{and} \quad (\text{A3})$$

$$\text{IDF}(w, A, B) = -1 \times \log \left(\frac{\sum_{j'} \mathbb{1}[w \in A_{j'}] + \sum_{k'} \mathbb{1}[w \in B_{k'}]}{J + K} \right). \quad (\text{A4})$$

Intuitively, the TF portion of the weight gives higher weights to words part of shorter names. The IDF portion of the weight gives higher weights to less common words relative to all words included in any election or Census establishment name. We give higher weights to less common words because two names sharing a common word (e.g., **manufacturing**) is less likely to indicate a correct match than two words sharing a less common word (e.g., **wanaque**).

The m-score(w, B_k) is defined as follows

$$\text{m-score}(w, B_k) = \overline{m}(w, B_k) \times \text{weight}(\overline{w}, B_k) \times \mathbb{1}[\overline{m}(w, B_k) > \theta] \quad (\text{A5})$$

where $\overline{m}(w, B_k)$ is the highest Jaro-Winkler distance between the word w and any word in the name B_k

$$\overline{m}(w, B_k) = \max_{w' \in B_k} \text{Jaro-Winkler}(w, w') \quad (\text{A6})$$

and \overline{w} is the word in B_k that maximizes the Jaro-Winkler string distance. θ is a threshold below which the m-score is defined as zero. The Jaro-Winkler string distance is a measure of how similar two strings are. It considers the number of matching characters in the strings and the number of transpositions necessary to get the strings to match (e.g., **Boston** and **Bostno** require one transposition). Finally, it also places a higher weight on matching characters at the beginning of strings. See [Kline et al. \(2019\)](#) for details.

3. We calculate the Haversine distance between the election and Census establishment geocoordinates as follows

$$d_{j,k} = \min(\text{Haversine Distance}(\text{geo_coord}_j, \text{geo_coord}_k), \overline{d}). \quad (\text{A7})$$

where \overline{d} is our distance top code (e.g., distances above a certain threshold are unlikely to be informative).

4. We combine the string similarity measure and the distance measure for each pair of elections

and establishments as follows

$$\text{match score}_{jk} = (1 - \beta) \cdot s_{jk} + \beta \left[1 - (d_{jk}/\bar{d})^\gamma \right] \quad (\text{A8})$$

where β is the relative weight placed on distance versus string name similarity. γ is the relative weight placed on very close versus farther away matches (e.g., a very concave γ places much more weight on exact geographic matches than matches that are even slightly farther away).

5. For each election, we pick the Census establishment with the highest match score $_{jk}$. This yields a potential match for each election but these matches may be very low quality or incorrect.
6. We only keep matches where match score $_{jk}$ is above a minimum threshold p .

The matching algorithm has several tuning parameters that determine the relative weights placed on each component of the final match score. For the parameters used to calculate the Soft TF-IDF score and the final match score (e.g., θ , the p parameter in the JW string distance, and γ), we use details about our institutional setting to optimize these parameters in a principled manner. We first optimize the Soft TF-IDF parameters by matching each election record to at most one contract record. We then choose the parameters that maximize the discontinuity in the likelihood that an election record has a matching contract record across the 50% vote-share threshold.

To pick the minimum match score p , we exploit the fact that the size of the election bargaining unit in the election data and the number of employees at the Census establishment give us information about whether or not the match is correct. In particular, having a larger bargaining unit than the number of workers at the establishment indicates an incorrect match.⁶⁹ We first directly calculate the probability that an election record was matched correctly to a Census record (as a function of the records' match score) by comparing the bargaining unit size to the number of workers at the Census establishment. For a matched set of records with match score s , we define the average likelihood that the matched Census employment is at least as high as the number of recorded votes $m(s)$. On the other hand, the likelihood that the employment at random Census establishment is at least as high as the number of recorded votes is \underline{m} . We assume records where the name and geographic location match exactly are "true" matches, which correspondingly allows us to estimate that a pair of records with a match score of s is matched correctly with probability:

$$p(s) = \frac{m(s) - \underline{m}}{m(1) - \underline{m}}. \quad (\text{A9})$$

We include all record matches where the correct match probability $p(s)$ is at least 75%, and we select the geography weight that maximizes the number of elections that are matched in this process. We then use the same parameters to also match contract notices to the Census records.

⁶⁹There may be cases of larger bargaining unit sizes than establishment employment that actually are correct matches. For example, there may be data mistakes in the bargaining unit size or the measures may cover different time periods.

Appendix References

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