Establishment-Level Unionization at Large Firms: Evidence from the 21$^{\text{st}}$ Century *

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Abstract

We examine the effect of establishment-level unionization elections on the equity value of publicly listed firms between 1994 and 2023. Successful elections at individual establishments have two opposing effects on firm stock returns — there is a small decrease in returns the day an election is filed, and a countervailing increase on the day the election is closed. On net, we find a precise null effect of union victories on daily stock returns. This result is robust to alternative definitions of treatment, longer time horizons for returns, and alternative sample selection.

Keywords: Unionization, Stock Returns, Personnel Economics

JEL Codes: G10, J51

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1 Introduction

Union elections are a critical step toward achieving union representation in the private sector. Since their peak in the 1970s, union elections have become less frequent, involve fewer workers, and increasingly take place within larger firms. These changes parallel a steady decline in private-sector union membership.[1] Together, they have transformed the landscape for unions. This raises the question of how union elections affect firms in this new environment.

In this paper, we analyze the effect of establishment-level NLRB elections for union representation at private firms (henceforth, "elections") on the stock returns of those firms. Seeing how capital markets react to these elections can give insight into how efforts toward union representation are perceived to affect firms by investors. Previous studies examining elections and firm market valuation focus on the long-run effects of large elections prior to 2000 (e.g., Ruback and Zimmerman, 1984; Lee and Mas, 2012) and find sizable declines in firms' stock returns resulting from winning elections.

Our study adds to this literature in two ways. First, we focus on short-run outcomes by examining the daily effects of elections on stock returns. This allows us to differentiate between elections being *filed* with the NLRB, at which point the NLRB makes the election publicly known; and when they are *closed*, at which point the results are certified and made public. Our analysis period begins in 1994, as NLRB data prior to 1994 does not consistently report exact election dates. Second, we are the first to create the data and study the impacts of elections on stock returns through 2023.

To conduct our analysis, we create a novel dataset combining election data from the NLRB with equity market data on publicly-listed (henceforth, "public") firms from the Center for Research in Security Prices (CRSP) database. The result is a panel dataset covering elections from 1961 through 2023. We use a difference-in-differences (DiD) specification to measure the instantaneous impact of elections on the days that they are filed or closed. The analysis of contemporaneous effects is for the period 1994–2023; however, by documenting elections dating back to 1961, we can restrict our sample to firms that have had elections won by the union (henceforth, "winning elections" or "union victories") in this period. In this way, we avoid assuming that firms without any union victories are good controls for firms with union victories (which we view as unlikely). Rather, our strategy exploits the quasi-random *timing* of the official filing and closing of elections in order to identify the average treatment effects of unions on the treated firms in our sample.

[1] From 2021 to 2022, elections filed with the National Labor Relations Board (NLRB) increased by over 60 percent, suggesting a modest reversal in a decades-long decline.

We find a precise null result for the effects of union elections on firm stock returns (Section 5). In our preferred specification focusing on winning elections, we find that filing an election causes firm stock returns to decline by approximately 7 basis points. This initial decline is offset almost entirely by a positive effect of roughly 7 basis points when the election closes, resulting in a net effect close to zero. Both effects are precisely estimated (statistically significant at the five or ten percent level), but small in magnitude. The cumulative impact of filing and closing an election amounts to a statistically precise zero — we can reject that the net effect of winning elections is larger than 10.6 basis points in magnitude at the 95% confidence level. We also look at heterogeneity of short-run effects by seeing if effects vary by election size, union vote share, duration, and whether or not it is the first election at a firm. We check that these results are robust to the election outcome (union victory vs. union loss), alternative measures of stock returns, and less-restrictive sample selection criteria. Finally, we implement robust estimators suggested in de Chaisemartin and d'Haultfoeuille (2020) and de Chaisemartin and d'Haultfoeuille (2023a) to ensure our results are not subject to biases that may contaminate two-way fixed effects estimators in settings with treatment effect heterogeneity or multiple treatments.

Finally, section 6 proposes a tentative mechanism for the short-run results. We show that the volatility of monthly returns significantly rises in the four months after an election is filed. We view this as reflecting heightened investor uncertainty driven by elections. The potential for conflict between labor and management becomes priced in while elections are open and is offset once they are closed and the outcome is known. We posit that the risk of conflict or work stoppages is heightened during elections.

1.1 Existing Literature

Researchers have long been interested in how unionization impacts firms. Are unions purely distortionary, causing employers to have to make sub-optimal personnel decisions? Or are they beneficial, improving worker productivity and facilitating communication? By studying unionization and market value we can assess the extent to which unions harm employers, or at least are perceived by investors to harm employers.

Prior work in economics has investigated how unionization impacts wages, employment, worker productivity, and establishment survival. Freeman (1984) and Farber (1986) are some of the first studies to discuss the measurement and causes of union wage premia. Freeman and Medoff (1984) provides a taxonomy of the mechanisms by which the wage gains associated with unionization can be the result of improved efficiency in labor markets or simply the extraction of rents by unions acting as monopolistic suppliers of labor. Empirical evidence from early studies on the magnitude of the union wage premium are mixed. Robinson (1989), Card (1996), and Vella and Verbeek (1998) find large union wage premiums using data from the early 1980s in Canada and the United States. However, other studies (c.f. Freeman and Kleiner, 1990; Kuhn, 1998; DiNardo and Lee, 2004) find that unionization has small or insignificant effects on establishment-level wages.

More recent analyses indicate that unionization raises compensation through benefits and pension contributions (Knepper, 2020). At the establishment level, unionization can reduce wage bills, employment, and survival (Frandsen, 2021; Wang and Young, 2022). Barth, Bryson, and Dale-Olsen (2020) finds that increasing union density within firms leads to higher wages and productivity for Norwegian firms. Establishment-level effects are attributed to mechanisms such as firms shedding higher-paid workers in favor of lower-paid replacements (Frandsen, 2021) or shifting production away from unionized establishments (Wang and Young, 2022).

A separate stream of research focuses on how unionization affects firm (as opposed to establishment-level) outcomes, particularly through stock market reactions. Early studies find unionization (Ruback and Zimmerman, 1984) and collective bargaining agreements (Abowd, 1989) have negative effects on stock returns. Bronars and Deere (1994) extend Ruback and Zimmerman (1984) and show union elections have negative spillover effects on other firms in the same industry.

Our study builds on the work of Lee and Mas (2012) and Kim, Zhang, and Zhong (2021) and Hofmann and Schoonjans (2023), who use event study specifications to estimate the dynamic impacts of union elections on stock prices. Campello et al. (2018) show that while unionization does not increase bankruptcy risk, it raises bankruptcy costs, reflecting unions' influence as unsecured corporate creditors. Others have investigated unionization's impact on innovation, showing reductions in R&D expenditure (Bradley, Kim, and Tian, 2017), as well as on firm-specific price risk of large daily stock price declines (Kim, Zhang, and Zhong, 2021).

We contribute to the literature by using a newly constructed dataset encompassing all elections between 1961 and 2023. We also provide evidence on the heterogeneous effects of unionization, both at the intensive and extensive margins, and offer suggestive evidence that the stock price impacts may be linked to investor uncertainty surrounding open elections.

2 Empirical Motivation: Starbucks and the Characteristics of New Union Elections

As a motivating example, consider Starbucks. Workers at a Starbucks location in Buffalo, New York, filed for an election on August 30, 2021. Since that initial filing and through the end of 2023, there has been a wave of 430 elections. These elections are relatively small, covering an average of 25 workers in each election. In total, they cover just under 11,000 workers — less than 3% of Starbucks' 402,000 employees.[2] Nonetheless, the market appears to have penalized Starbucks for these elections. Panel (A) of Figure 1 shows Starbucks' cumulative returns around the time these elections took place compared to a value-weighted benchmark of firms in the same size decile and on listed on the same exchange. Panel (B) tracks the number of open elections at Starbucks during the same period. Prior to the initial election, Starbucks' stock returns aligned with the benchmark. However, as the number of elections ballooned during the first half of 2022, Starbucks' cumulative returns fell by up to 20% relative to the benchmark.

Starbucks is a unique case, having held over 400 elections over less than two years. Yet it demonstrates how small elections can have meaningful effects on returns. Are the negative impacts of union elections on returns a phenomenon unique to Starbucks or does it generalize across all firms?

The Characteristics of Elections over Time It is well established that private-sector unionization rates in the United States are below their secular highs in the 1960s and 1970s (Dinlersoz and Greenwood, 2016; Farber et al., 2021). This decline in the share of workers with union representation coincided with a decrease in elections. The number of NLRB elections fell from a high of over 37,000 over the five year period between 1970-1974 to under 7,000 between 2015-2019 (see Table H.1). Elections also frequently occur outside of manufacturing; the percent of elections occurring in manufacturing has declined steadily from roughly 70% of elections in 1961 to only 20% in 2019. Conversely, over the same period, elections in the services and utilities industries have seen substantial growth (see Figure H.1).

Not only are elections less frequent, but they involve fewer workers and occur at larger firms. The figures below highlight the changing characteristics of NLRB elections at publicly traded firms: (1) they are smaller, as measured by the number of employees eligible to vote in each election (i.e., the size of the 'bargaining units'); (2) they are held at larger firms, measured by the total number of employees; and (3) they comprise a smaller share of each firm's total employment. Panels (A)–(C) of Figure 2 illustrate these changes. (We tabulate

[2] This is the number of employees at Starbucks in 2022 and is from the Compustat/CRSP dataset provided by Wharton Research Data Services (WRDS)

these numbers in Table H.1.)

Only those employees that are part of the bargaining unit are eligible to vote in elections. Panel (A) of Figure 2 shows how bargaining units have declined in size. Prior to to 1980, bargaining units typically covered well-over 100 workers at a given firm. The size of bargaining units has shrunk to the point where, between 2020 and 2023, they include only 35 people on average. Panel (B) illustrates the growing size of firms that have elections. The black line plots the median size at firms with union elections in each year since 1960. Data on the reported number of employees at public firms is from the Compustat/CRSP dataset provided by Wharton Research Data Services (WRDS). The relationship here is not monotonic, but is instead "U" shaped: The average number of employees at public firms with elections shrank between 1960 and 1990 and rose steadily after 2000.

Combined, these trends result in substantially fewer elections comprising large shares of workers. Panel (C) shows a time series of "big" elections, which we — following Lee and Mas (2012) — define as elections where the bargaining units have at least 100 eligible voters and 5% of the firms' total workforce. Lee and Mas (2012) find that between 1961 and 1999, union victories in a large elections lead to a 10 percentage point decline in average cumulative abnormal stock returns over the following two years."

However, the sample selection criteria used by Lee and Mas (2012) is no longer relevant. Even if big elections continue to have negative effects on firms, the occurrence of these elections has fallen to the extent that they now comprise less than 1% of all elections; the number of big elections from 1970 through 1974 was 317; this century there have been only 136, with only six big elections occurring from 2020 through 2023 (see Appendix H for a complete tabulation of "big" elections). Rather than limit the analysis to big elections, we examine if negative effects also materialize for the overwhelming number of smaller elections that are increasingly prevalent.

3 Unions and Firm Data

This section describes our data, its sources, and the procedure for constructing our main sample. The final dataset is a panel that tracks NLRB elections filed and closed at publicly traded firms in the United States from 1961 through 2023. We begin with a brief overview of the NLRB election process, followed by a discussion of the various data sources used to compile elections during different periods. Lastly, we provide an overview of how we match election firms to stock returns.

3.1 Unions and the Election Process

Unions advocate on behalf of employees to establish and improve favorable employment terms. By coordinating collective representation, unions enhance workers' bargaining power, benefiting employees through increased cohesion and solidarity (Freeman and Medoff, 1984). Unions primarily negotiate with employers for better wages and working conditions and may organize strikes if negotiations fail. Private sector unions in the United States are regulated by the NLRB, an independent federal agency established in 1935 under the National Labor Relations Act (also known as the Wagner Act, amended in 1947 and 1956). The NLRB is the primary institution safeguarding workers' rights, handling union elections, collective bargaining agreements, petitions for union representation, and cases of unfair labor practices (NLRB, 2024b).

The NLRB election process is detailed in Figure 3. To uphold timeliness and fairness, the NLRB mandates that employers inform employees about the election and ensure they can vote freely. Our analysis focuses on the date a petition is filed and the date the NLRB certifies the election results.[3]

Elections are held when an employee submits a petition to the NLRB signed by at least 30% of the employee-defined bargaining unit. Upon receiving the petition, the NLRB assesses whether the proposed bargaining unit is valid DiNardo and Lee (2004). If deemed valid, the NLRB formally files the petition, notifies the involved parties, and posts case information publicly on its website. The NLRB then collaborates with both parties to create a timeline and determine voter eligibility (NLRB, 2024a). The election concludes when the NLRB counts the votes and certifies the results, again making this information publicly available.

A union that receives a simple majority of votes within a bargaining unit is certified as the bargaining representative and must be recognized by the employer as the exclusive bargaining agent for the employees in that unit. Any refusal by the employer to negotiate with the union at this stage constitutes an unfair labor practice (ULP). The average election timeline is approximately eight weeks, with six weeks between filing and tallying, and two weeks between tallying and closing. However, there is significant variation in election durations. Elections are often extended when allegations of misconduct are made (Ferguson, 2008). These allegations can arise from perceived unlawful behavior during the election process or by first disputing individual ballots (NLRB, 2024a).[4]

[3] Throughout the paper, we use phrases like 'filing a win' or 'filing a loss,' recognizing that the election outcome is unknown at the time of filing. 'Filed a win' indicates that the firm filed for an election that eventually resulted in a win.

[4] The NLRB makes ULP data publicly available on their website National Labor Relations Board, 2024. In this data, the vast majority of complaints filed during elections are directed against employers, with the two most common complaints being failure to bargain with the union and retaliatory actions, such as firing

Critically, the certification of a union representative (the result of a successful election) does not ensure the union will reach a collective bargaining agreement (CBA) with the employer. Ferguson (2008) finds that of 22,382 elections filed between 1999 and 2004, 44% did not lead to a contract. We attempt to check if elections result in a union agreement using F-7 Notices of collective bargaining activity from the Federal Mediation and Conciliation Service (FMCS).[5]. We also use Occupational Safety and Health Administration (OSHA) data, which contains data on annual inspections going back to 1970. For our purposes, it includes variable if an inspected establishment is unionized.[6]. We matched NLRB election data to CBA and OSHA data using firm name and address. Of the 4,902 winning elections that we were able to match, for 3,705 of these we found evidence of union representation. The other 1,197 were listed as not unionized by OSHA. If a union and firm do form an agreement within a year of election closure the union may be de-certified via a subsequent petition and election resulting in a loss.[7] Going forward our (and other measures) in the literature measures the effects of elections, which does not per se capture the effects of any collective bargaining outcome.

3.2 Data Sources

The NLRB data comes from two sources. We filed a FOIA request with the NLRB to obtain data on elections after 2000 (see Appendix I).[8] For elections prior to 2000, we rely on data created by J.P. Ferguson (Ferguson, 2016) and Thomas Holmes (Holmes, 2024).

We use stock data from the CRSP and firm size and industry data from the CRSP/Compustat (CCM) merged dataset. These data contain stock information for firms listed on all major US exchanges from 1926 through the present. Our main outcome of interest is daily abnormal returns. To construct abnormal returns, we take the difference in daily stock returns and a benchmark. For the benchmark, we use a value-weighted portfolio of firms in the same size-decile and listed on the same exchange. Stock returns are *cum* dividend returns over a holding period (in our case, daily). It is computed as the percent change in closing prices from one day to the next. It incorporates cash and price adjustment factors to account for

or threatening employees, for union-related activities.

[5]These notices are supposed to be filed for all collective bargaining activity, however it is likely they are under reported (Wang and Young, 2022) The FMCS provides data on these notices going back to 2015 on their website (Federal Mediation and Conciliation Service, 2024)

[6]This data was downloaded directly from the OSHA website (U.S. Department of Labor, 2024)

[7]Although this may seem like an obvious goal for firms once a union is formed, unions are rarely decertified.

[8]While these records are ostensibly publicly available in PDF form for FY2001 onward, a number of clerical and administrative errors made them intractable for our purpose. The FOIA enabled us to directly access tabulated data from the two disjoint databases maintained by the NLRB that separately cataloged elections for the FY2000–FY2010 and FY2011–2023 periods.

corporate actions like stock splits, dividends, or stock rights offerings. Data on benchmark returns, along with daily stock returns, are taken directly from the CRSP. As an alternative benchmark, we also use daily Fama and French (1993) factors available on Kenneth French's web-page (Fama and French, 2023).

3.3 Matching Procedure and Dataset Construction

This section outlines the process for creating our panel of elections and stock returns. The task was to match NLRB election data, which contain firm names, to CRSP stock data, which contain firm names and a unique firm identifier ("PERMCO"). In addition to dealing with typos and inconsistencies (e.g. "First Student" and "First Student Inc.") inherent to string matching, the task was further complicated by name changes (e.g. "Facebook" becoming "Meta"), mergers and acquisitions, delistings, and subsidiary firms. Thus, while names were used for matching, it is PERMCO values, which are unique and persist across time that are essential for our analysis.

Name cleaning and fuzzy matching We cleaned all firm names by removing capitalization and punctuation. We also removed common words — those often found in firms' official titles that are superfluous for identification. These words are: "firm", "corporation", "agency", "limited", "incorporated", and abbreviations for these, e.g.,"co", "llc", "corp", etc. We also switch the following words to their singular tenses, "services", "systems", "communications", "industries", "enterprises", "electronics", and "technologies." Finally, we removed all spaces from the firm name. Doing this reduced the number of unique names from 173,458 to 146,013 in the NLRB data and from 440,136 to 339,584 in the CRSP data. There are just under 26,000 PERMCOs across all the names.

WRDS has data on subsidiaries beginning in 1994. It contains parent company name and PERMCO and subsidiary firm names gathered from all forms filed at the Security Exchange Commision. There is a median of 8 subsidiaries per parent firm (average of 36). Subsidiaries often change parents and may have more than one parent.

Once we had a list of company names and PERMCOs we created a crosswalk linking each company to a PERMCO on a date. We also used a fuzzy matching algorithm to match names that are not exact matches due to typos or other small inconsistencies. These two steps are described in Appendix A.

Table 1 lists the number of names and elections matched for 5-year periods. The percentage of elections matched stays around 10% to 15%, with a slight decline between 1985 and 1995 when we match only 5% of elections. Of course, even if we were able to match perfectly, the match rate would not be 100% because not all elections happen at publicly traded firms. The increased match rate post-1995 is due to the introduction of the subsidiary dataset. These matches are for elections which occur *while* the establishment is owned by a public firm. If we focus on name alone the match rates increase to around 20% (see Appendix A).

3.4 Summary Statistics

Between 1961 and 2023, approximately 238,000 union representation elections took place across 147,000 firms. Of these elections, 26,647 elections are held at 3,769 publicly traded firms (at the time of the election). Unions win just over half of all elections (54%), with an average vote share of 57%. These elections tend to involve relatively small bargaining units, with an average size of 67 employees. While the NLRB does provide information on firm size, we find that for elections we matched to employee count data, roughly 4% of a firm's workforce, on average, are in the bargaining unit.

Table 2 reports summary statistics for NLRB elections since 1961. We create the same table only using 1994 onward in Table H.2. Columns (1) and (2) of Table 2 separate elections into those that take place while a firm is public ("matched"), and those that do not ("non-matched"). Most elections take place at non-matched firms. Elections at matched firms are substantially larger than at non-matched. This is unsurprising given that public firms are typically larger than private ones (Dinlersoz et al., 2018). The average percent of workers voting in favor of the union and average length of elections are the same in both groups at 57% and 92 days. Columns (3) and (4) separate elections at matched firms into winning and losing elections. We match just over 14,000 wins and 12,000 losses, across 2,500 and 2,600 firms. (Note that firms often have multiple elections.) The average size of a bargaining unit when the union wins the election is 85 employees compared to 137 employees when the union loses. Additionally, losing elections are about 3 weeks longer on average. Others have argued that long elections are caused by complaints filed by either party, with the majority being filed against employers (Ferguson, 2008). Losing elections have larger bargaining units on average and are at firms with fewer employees.

Table 3 reports the average number of employees, daily return, and daily benchmark return. Standard deviations are reported in brackets. Returns are *cum* dividend and are calculated as the percentage change in closing price between days. The benchmark is a value-weighted portfolio of firms in the same size decile and listed on the same exchange. Both are measured in basis points (units of 0.01 percentage point). Column (i) is firms that we did not match to an election. Column (ii) includes firms that have a winning election at any point from 1961 onward, either before or during their time as a public company. These

3,165 firms make up our estimating sample. Within this sample, there are nearly 900 firms that have a win since 1994 — this is our treated group (Column (iv)). Those firms that had wins before but not after 1994 are our "never treated" group. Firms with a win since 1961 have far more employees than non-election firms (approximately 2,600 compared to approximately 19,600); firms with wins are about three times as large as firms without wins. Average returns are comparable across all groups, ranging from 6.12 basis points for firms with wins after 1994 to 8.28 basis points for firms with no elections. We also tabulate the proportion of firms that delist, merge, acquire, or liquidate at any time during the sample period. Firms without elections are far more likely to delist than firms without elections; the prominence of restructuring is similar for all firms, however non-election firms are more likely to be acquired or liquidated.

4 Identification

Our strategy for recovering the causal effects of winning elections on stock returns relies on the quasi-random timing of election filings and closures among firms where unions win at least one election. This differs from previous approaches that use regression discontinuity (RD) designs (c.f. DiNardo and Lee, 2004; Sojourner et al., 2015; Campello et al., 2018; Kim, Zhang, and Zhong, 2021), where identification requires that election outcomes within a narrow margin of the 50% cutoff are as good as randomly determined. Recent research argues that this is unlikely to hold for union elections. Knepper (2020) and Frandsen (2021) give evidence of nonrandom selection ("manipulation") around the 50 percent vote share cutoff in NLRB elections; we show in Appendix B.1 that continuity in the distribution of vote shares at the 50 percent threshold does not hold in our sample of elections. Frandsen (2021) shows this issue can be overcome by using a "difference-in-discontinuities" approach, which allows the treatment effect of union victories to be identified under the assumption that the traits on which selection into unionization occur are time invariant. However, it is difficult to reconcile either RD approach in our setting with establishment-level treatment and firm-level outcomes.

Thus, instead of an RD design, we use a difference-in-differences framework, restricting our main estimating sample to firms with at least one winning election. This allows us to estimate the average treatment effect of union victories among the treated firms (i.e., the *ATT*). Our strategy is closest to that of Wang and Young (2022), who use a modified version of the "difference-in-discontinuities" design to estimate the effects of unionization for firms not exactly at (or around) the 50% vote share cutoff.

4.1 Identifying Assumptions and Notation

Let $i \in \{1, \ldots, I\}$ and $t \in \{1, \ldots, T\}$ index our panel of stock returns across the I firms in our sample. Let $\{v_{i,f,c}\}$ denote the set of all establishment-level elections that unions wins at firm i. Each victory $v_{i,f,c}$ occurs at a given firm, i, and consists of two dates: a filing date, f, and a closing date, c. Let

$$W_{it}^{f} = \left| \left\{ v_{i,f,c} : f = t \right\} \right| \tag{1}$$

$$W_{it}^{c} = \left| \left\{ v_{i,f,c} : c = t \right\} \right| \tag{2}$$

denote the total number of winning elections that were filed and closed, respectively, at firm i on trading day t. Finally, let $\mathbf{W}_{it}$ be the vector of $[W_{it}^{f}, W_{it}^{c}]$ of the total number filings and closings at firm i in trading day t and let $\mathcal{W}$ be the set of values $\mathbf{W}_{it}$ can take.

We use firm-level daily abnormal stock returns, AR_{it}, as our outcome variable. We measure abnormal returns for firm i on day t are defined as the difference between firm stock returns, R_{it}, and the return on a matched benchmark, Ret_{it}^{BM}:

$$AR_{it} = R_{it} - Ret_{it}^{BM} \tag{3}$$

where, following Lee and Mas (2012), Ret_{it}^{BM} is the return on a value-weighted portfolio of firms in the same size decile and listed on the same exchange as firm i on day t.[9] This benchmark return is a proxy for the mathematical expectation of returns that period, $\mathbb{E}[R_{it}]$, so that abnormal returns then proxy for how much a firm outperforms expectations.

We adopt a potential outcome framework, where $AR_{it}(\mathbf{w})$ gives the potential abnormal return on the stock of firm i at time t when the number of elections filed and closed is $\mathbf{W}_{it} = \mathbf{w}$. Our treatment variables are the values, $\mathbf{W}_{it}$, firms may realize each trading day; our outcomes of interest are the causal effects from unit changes in a given treatment variable on potential returns (Rubin, 1974). In addition to the standard stable unit treatment variable assumption, we follow the framework in de Chaisemartin and d'Haultfoeuille (2023a) and make the following two assumptions:

Assumption 1. *Strong Exogeneity: For all firm, time pairs (it) with $t > 1$,*

$$\mathbb{E}\Big[AR_{it}(\mathbf{0}) - AR_{it-1}(\mathbf{0}) \mid \mathbf{W}_{i1}, \ldots, \mathbf{W}_{iT}\Big] = \mathbb{E}\Big[AR_{it}(\mathbf{0}) - AR_{it-1}(\mathbf{0})\Big] \tag{4}$$

[9] Table 8 along with Appendix G shows our results under alternative measures of abnormal returns are virtually unchanged from the main specification.

where $\mathbf{0}$ *is the zero vector.*

Assumption 1 is the technical condition for treatment exogeneity in our setting: The sequence of elections (the design), should not contain any information about the expectation of firm returns absent treatment. This requires that random shocks that would affect that firms' untreated returns be mean-independent of the sequence of daily election filings and closures experienced at firm i. It formalizes the notion that we require the timing of election filings and closings to be quasi-random in the sense that they be must unrelated to daily stock price variation that would occur absent an election. We believe that quasi-random timing of filing dates is plausible as it's unlikely that union organizers or the NLRB choose specific dates to file and close elections based on day-to-day fluctuations in firm stock returns.

Assumption 2. *Common Trends: For all firm, time pairs* (i, t) *with* $t > 1$*,*

$$\mathbb{E}\Big[AR_{it}(\mathbf{0}) - AR_{it-1}(\mathbf{0})\Big] \tag{5}$$

does not vary across firms.

The second assumption for identification requires that the change in potential untreated returns between periods t and $t-1$ be equal in expectation across all firms, for all periods $t > 1$. Assumption 2 is a standard parallel trend assumption on firms' untreated outcomes being similar across treated and control firms over time. Testing this assumption would require knowledge of abnormal returns at firms with winning elections had they not had the winning election, which is unknowable. However, we can lend support to the assumption by testing whether treated and non-treated firms see similar trends in abnormal returns in the periods before treatment. We report these results along with our event study estimates in Section 5.2.

4.2 Sample Restriction

To further lend credibility to Assumptions 1 and 2, we restrict the estimating sample to firms where at least one establishment has certified an election victory since 1961. Restricting our sample to these firms avoids using firms where unions never win a control group. The controls in our setting are instead firms that do not experience a successful filing or closure on that particular trading day. Narrowing the estimating sample to the set of "ever-winners" allows time fixed effects in our model to capture aggregate shocks that affect only firms which have characteristics that lead to selection into unionization.

To illustrate why this selection criterion bolsters the parallel trends assumption, consider a hypothetical scenario in which the United Auto Workers (UAW) holds a national rally for automotive workers at time t, during which its leaders call for a doubling of wages in the automotive manufacturing sector. Take, for example, three firms that could appear in our sample: Ford, Mercedes, and Tesla. Assume the relevant Mercedes establishment is located in Vance and Woodstock, Alabama, covering Mercedes' U.S. operations. Alabama, known for its anti-union stance, recently enacted legislation withdrawing all state and local support for firms that voluntarily recognize unions (Stephenson, 2024). In contrast, suppose the Tesla factory in question is located in California, a state traditionally more supportive of labor organizing.

The parallel trends assumption (Assumption 2) requires that, absent treatment, the change in abnormal returns would be similar across the three firms. For Ford and Tesla, the parallel trends assumption is plausible. Even without unionization efforts at Tesla, the market might anticipate that the UAW's wage demands could impact wages at Tesla similarly to Ford, given California's pro-union institutions and labor force. In contrast, it is less likely that the UAW's announcement would affect Mercedes in the same way. Mercedes is a non-American firm with U.S. operations mostly in states with right-to-work laws and less union-friendly environments, insulating it from union pressures. This event could negatively impact Ford's and Tesla's stock returns (due to exposure to UAW demands) while having little to no effect on Mercedes. Our sample restriction excludes firms like Mercedes, where unions are unlikely to succeed, from the control group. Consequently, we better identify the average treatment effect on treated firms (ATT). This sample also lessens attenuation bias, as the majority of firms remain in the sample through the end of the sample period.[10]

5 Results

In this section, we examine the daily effects of filing and closing winning elections on stock returns among firms that ever have a winning election. Our primary specification is the following two-way fixed effects model:

$$AR_{it} = \beta_f W_{it}^f + \beta_c W_{it}^c + \alpha_i' X_t + \gamma_t + \delta_i + \varepsilon_{it} \tag{6}$$

where W_{it}^f and W_{it}^c are count variables for the number of elections filed and closed. AR_{it} are abnormal returns as defined in Equation (3).[11]

[10] In Appendix E we confirm that our results hold when including all firms with elections or all firms listed in the CRSP.

[11] See Appendix G for alternative measures of abnormal returns.

Table 4 displays the results from estimating Equation (6). Because returns can be correlated across firms on the same date and across dates at the same firm, we use two-way clustered standard errors and cluster by date and firm (Cameron, Gelbach, and Miller, 2011). For readability, explanatory variables have been scaled by 1/10000 so coefficient estimates are reported in basis points (one-hundredth of a percentage point). Our preferred specification in Column (2) is the baseline two-way fixed effects model shown in (6). Column (3) adds firm-level Fama-French factor loadings (Fama and French, 1993). We find two results. First, there is a modest negative effect of about 7.4 basis points (or a change in returns of 0.074 percentage point) on firm stock returns the day an additional win is filed. Second, these effects are counteracted by a positive effect of 6.9 basis points the day it is closed. These effects are small: 7 basis points is slightly more than the average daily return among the firms in our estimating sample. Both effect sizes are less than five percent of a standard deviation of firm-level returns returns. The effect of closing or filing an additional election are precisely estimated and statistically significant at the 5% or 10% level across all three specifications.

In the bottom row of Table 4, we show p-values for the test statistics of the null hypothesis that the coefficients on filings and closings sum to zero; that is, on net, filing and closing have no effect on stock returns. We fail to reject the null in all three specifications. For the preferred specification in column (2), we can reject a net negative effect of union victories larger than 10.6 basis points (or, 0.16%) at the at the 95% level. An effect this size would be large enough to offset 1.6 days' worth of average daily returns in our sample, and is about 3% of the standard deviation of daily returns. Thus, at the daily frequency, we find the net impact of establishment level unionization efforts on contemporaneous returns to be a precise zero. In Appendix C, we show that if we instead consider the effects of victories on returns over a longer time horizon, we can reject net effects of wins larger than 12, 38, and 29 basis point decreases (in magnitude) on total stock returns at the 95% level for weekly, monthly, and annual returns respectively. The magnitude of these lower bounds on the negative effects on returns is less than 3, 3, and 1 percent of the respective standard deviations of returns for firms at the same frequency in our sample. Finally, Appendix D shows that results are similar if treatment variables are indicator variables for any wins filed and closed. While Appendix E shows the results hold when using less-restrictive samples consisting of all firms with elections (Table E.1) or all firms in the CRSP database (Table E.2).

5.1 Differentiated Effects by Election Outcomes

We next consider how the effects of filing and closing elections may differ by outcome. We re-estimate a version of Equation (6) where the regressors filed_{it} and closed_{it} are count variables of all elections, winning elections, or losing elections filed and closed:

$$AR_{it} = \beta_f \text{ filed}_{it} + \beta_c \text{ closed}_{it} + \gamma_t + \delta_i + \varepsilon_{it} \tag{7}$$

Column (1) of Table 5 shows the estimated values of the β_f and β_c coefficients when we consider *all* elections. Our results indicate that the direction of the effects does not depend on outcome. This is consistent with earlier findings in Ruback and Zimmerman (1984) and Bronars and Deere (1994). Column (2) reports effects when using all wins (this is the same estimation we estimate in Table 4). Column (3) shows estimated coefficients when the variables $filed_{it}$ and $closed_{it}$ are counts of *losing* elections filed and closed. We find that, across all columns, filings result in small declines that are subsequently offset by closings. In all cases, the magnitude of the individual effects are small and we cannot reject the null hypothesis of zero net effects.

Column (4) of Table 5 includes variables for filing and closing wins (W_{it}^f and W_{it}^c) and losses (L_{it}^f and L_{it}^c), and allows us to formally test whether the effects of filing or closing elections differ:

$$AR_{it} = \beta_{wf} W_{it}^f + \beta_{wc} W_{it}^c + \beta_{lf} L_{it}^f + \beta_{lc} L_{it}^c + \gamma_t + \delta_i + \varepsilon_{it} \tag{8}$$

When the effects of wins and losses are jointly estimated in Equation (8), we find similar effects (Column (4)) to when they are considered in isolation (Columns (2) and (3)).

Table 6 provides p-values from Wald tests of the joint significance for combinations of the estimated coefficients in Equation (8). The first two rows show that we cannot reject the null hypothesis that the effects of filing (Row (i)) or closing (Row (ii)) an additional election differ by outcome (p-values of 0.56 and 0.28, respectively). Rows (iii) and (iv) show that we still cannot reject the null hypothesis of no net effects (p-values of 0.94 and 0.62 for wins and losses, respectively). Finally, the last two rows report the equality of coefficients for each outcome. Row (v) is for a test of equality between the effects of filing a win and closing a win. We reject this equality at the 1% confidence level (p-value $= 0.005$) suggesting that our main specification does find precise and distinguishable differences between these events. Row (vi) is the same test of equality, but for losing elections; we cannot statistically distinguish between the effects of filing a loss and closing a loss.

All together, the tests in Table 6 indicate that the contemporaneous effects of union wins and losses on stock returns are not statistically distinguishable from each other, and that the net effects of either event, considered separately or together, are small.

5.2 Dynamic Effects

To test for anticipatory or lagged effects from filing and closing winning elections, we estimate the following dynamic difference-in-differences equation:

$$AR_{it} = \sum_{T_0:k\neq-1}^{T_1} \beta_k \times \mathbb{I}\{t - \text{event}_{it} = k\} + \phi_i + \gamma_t + \varepsilon_{it} \tag{9}$$

where the coefficients of interest are the values of β_k for each k inside of a window $\{T_0, \ldots, T_1\} \setminus -1$. All observations from outside of the window, $[T_0, T_1]$, around each filing or closing — "events" — are removed.[12] The variable event_{it} marks the calendar time, t, when an event occurs at firm i. Values of $k \leq 0$, are "pre-event" coefficients, and represent the days prior to the event. These capture any anticipatory effects. Likewise, "post-event" coefficients, denoted by $k \geq 0$, capture effects in the days after an event. The terms ϕ_i and γ_t are firm and trading day fixed effects. The day before the event, $k = -1$, is excluded, so that all β_k coefficients are interpreted as average differences (between treated and control firms) relative to the day prior to the event.

Along with capturing dynamic effects, estimating Equation (9) can help assess the validity of Assumption 2: insignificant coefficients on the pre-event terms indicate that we cannot reject a null hypothesis of no pre-treatment differences in abnormal returns between control and treated firms. This is not a full test of Assumption 2, which requires knowing how stock returns *would have* evolved had the event not occurred. This is a counterfactual scenario and is thus unobservable to the econometrician; no evidence of pre-event differences between treated and control firms does however lend Assumption 2 plausibility.

Figures 4 and 5 show estimates and 95% confidence intervals for each β_k term in a 20 day window around events (i.e., $-T_0 = T_1 = 10$). Looking at individual coefficients, there is not evidence of pre-trends before either event. Motivated by the work of Roth (2022), we also perform a more-restrictive joint test of significance (rather than relying on individual confidence intervals). The p-values for the joint significance of the pre-period coefficients are 0.31 for filing and 0.29 for closing, while the post-period p-values are 0.06 for filing and 0.24 for closing. We include all filings (3,760 days with filings) and closures (3,786 days with closures).[13] Other firms that do not have elections in the 20 day window around the event

[12] Miller (2023) gives a taxonomy of approaches to selecting specifications for event studies including the one we use here.

[13] The results are similar if we restrict the sample to events that do not have other elections filed at the same firm in the 20 day window.

are included as control firms.

5.3 Heterogeneity

Election effects may vary based on specific election characteristics. Previous research has found that effects of elections differ by election characteristics. Lee and Mas (2012) find that large elections lead to greater declines in profitability compared to smaller elections. Wang and Young (2022) argue that initial union efforts within a firm are more strongly opposed by employers, and that this opposition leads to larger negative effects of union victories on establishment survival and employment. Longer elections and elections decided by narrow margins often result from employer interference in the election process (Frandsen, 2021; Knepper, 2020; Wang and Young, 2022). This interference, when done through illicit means, carries risk of punishment. If employers only resort to these tactics when they deem it worth the risk, then longer elections may be the ones where losing is especially costly. Each of these channels may lead to differentiated effects of union victories for these elections.

To probe treatment effect heterogeneity, we first construct binary indicators equal to one on days that wins are filed ("Win filed dummy") or closed ("Win closed dummy") at a firm. We regress abnormal returns on these binary indicators as well as interaction terms with each of the election characteristics described above. In Column (1) of Table 7 we interact our two treatment dummies with a continuous measure of the percent of the firms' employees eligible, calculated as the sum of eligible voters across all elections filed or closed at the firm on that day divided by the firms' total employment. This specification allows for elections which cover a larger share of a firms' total employees to have different effects on abnormal returns. We find that a one percentage point increase in the percent of employees eligible leads to additional declines in returns of -1.4 and -0.8 basis points on the days that winning elections are filed and closed, respectively. Given that the average percent of employees eligible in elections in our sample is roughly 2%, we conclude that effects are small even when accounting for election size, as measured by the percent of employees eligible.

Similarly, Column (2) shows estimates for the effects of filings and closures interacted with election duration. We construct a variable "Duration", measured as the average number of days between filing and closing for all elections filed or closed on a day. We find that there is a negligible effect of election duration on filings. Conversely, we find a larger and positive (0.054 basis points) effect of duration on abnormal returns the day the election is closed. This effect is statistically significant (at the 10% confidence level). This result is consistent with long elections resulting from disputes between employers and their employees or the unions; from the perspective of investors, closing a long, tumultuous election may resolve more uncertainty than closing a short, uneventful one.

The last two columns of Table 7 allow for differentiated effects of "first" and "narrow" wins. Column (3) shows results when we interact treatment with an indicator $\mathbb{I}\{\text{First}\}$ for days where the first union win at a firm is filed or closed. First elections experience larger effects (in magnitude) of both filings and closures; returns are about 25.5 basis points higher on days when a first election is closed. The perceived risk of further disruptions and potential spread to other establishments is likely more salient to investors during first elections, which may explain the spike in returns when these elections conclude. In column (4), we interact treatment with an indicator $\mathbb{I}\{\text{Narrow}\}$ equal to one if the total number of votes for the union divided by the total number of votes on that day, across all elections, is between 45% and 55%. Narrow elections also appear to have larger effects (in magnitude) for both filings and closures, however these estimates are imprecise. In summary, while we do detect a limited degree of heterogeneity (and some that is significant at standard levels), the magnitude of the effects are small and on the whole do not materially affect our conclusion that the *net* effects of unionization are near zero.

5.4 Robustness to Alternative Measures of Abnormal Returns

Figure 6 shows robustness of our results to different methods of constructing Ret_{it}^{BM} in Equation (3). We re-estimate Equation (6) using four other benchmarks: fitted values from Fama-French factors; the risk-free rate; an equally-weighted portfolio of firms in the same size decile and industry; and a value-weighted portfolio of firms in the same size decile across all exchanges. We also report coefficients using a value-weighted portfolio of firms in the same size decile and on the same exchange (our preferred benchmark). The point estimates and 95% confidence intervals for the coefficients on filings and closures are plotted in Figure 6. The top (bottom) panel shows the estimated coefficients when we define treatment as union wins (union losses). We find qualitatively similar results across all benchmarks. Filing (closing) a win causes a negative (positive) effect between 6 and 8 basis points on abnormal returns. Table 8 reports regression results.

5.5 Robustness to Alternative Estimators

A recent strain of econometrics literature shows standard two-way fixed effects (TWFE) estimators can be biased even when Assumptions 1 and 2 hold (de Chaisemartin and d'Haultfoeuille, 2023a).[14] de Chaisemartin and d'Haultfoeuille (2023a) show that the two-

[14] For a review of recent advances in heterogeneity-robust difference-in-difference estimators see de Chaisemartin and d'Haultfoeuille (2023b).

way fixed effect estimands in Equation (6), β_f and β_c, are weighted sums of both their own ATTs as well as the ATTs of the other treatment in the regression, with the latter referred to as "contamination" terms. To check the extent to which contamination and negative weights may bias our estimates, we employ the test suggested in de Chaisemartin and d'Haultfoeuille (2020). We find that for the baseline specification (Equation (6)), only the former source of bias may threaten our identification. The portion of the estimates $\hat{\beta}_f$ ($\hat{\beta}_c$) that are a sum of estimated firm-level average treatment effects of filings (closures) all receive positive weights. However, both estimators also contain weighted sums of the other treatment effect. A majority of these so-called "contamination terms" enter into the estimators negatively.[15] This contamination implies some bias in our TFWE estimates even when Assumptions 1 and 2 hold.

de Chaisemartin and d'Haultfoeuille (2023a) suggest an alternative estimator, the DiD_M^f, that is robust to treatment effect heterogeneity and multiple treatments. Their estimator provides an unbiased estimate of the weighted average effect of incremental increases in one non-binary treatment variable (additional victories filed on a given trading day) on stock returns, while holding a second treatment variable (number of victorious elections closed at that firm) constant. This is, in essence, an unbiased estimator for a weighted average of the treatment effects among firms where, between two days, either the number of closures *or* filings changes.[16] Identification requires analogous conditions for parallel trends and quasi-random election timing. The analogous condition to Assumption 1 requires that the counterfactual expected evolution of stock returns between a given two adjacent periods is the same for firms with equal treatment levels on the first trading day. The analogue to Assumption 2 is that evolution of potential outcomes for firms whose treatment does not change from a given level $\mathbf{W}_{\cdot,t-1}$ in time t be mean-independent of the number of closures and filings which are realized at that firm in all periods outside of $t-1$.

Table 9 shows the results from re-estimating Equation (6) with only firm and date fixed effects as controls using the DiD_M^f estimator. The coefficients are interpreted as a weighted average of the effect of an additional (winning) election being filed (closed) on returns when the number of closures (filings) remains unchanged from the previous day. The estimates suggest an additional winning establishment-level election being filed (closed) is associated with returns rising by 6.0 (9.4) basis points. The alternative estimates substantially less-precise than our findings in Section 5, and the coefficient on filings switches signs. Using the bootstrapped standard errors from the DiD_M^f estimator, we can to reject a negative effect

[15]We note that these weights are relatively small in magnitude – the sum of the absolute value of these weights on the contamination terms is 10.4 percent of the magnitude of those on the treatments of interest.

[16]In contrast, the ATT is the expected effect of a given treatment (closures or filings) across *all* treated firms regardless of how other treatment statuses change.

larger than 11 basis point decline on net.

6 Elections and Stock Return Volatility

We now turn to a potential mechanism behind the daily effects of filing and closing a winning election. We use stock return volatility as a measure of uncertainty during union elections. Again, we find off-setting effects from filing and closing: stock volatility increases after elections are filed and declines when they close. This temporary increase in price volatility may be responsible for the one time rise (fall) in returns around filings (closures). To formally test the effects of elections being filed and closed on return volatility before, during, and after the election event, we employ a dynamic difference-in-difference specification. We use the estimator proposed in de Chaisemartin and d'Haultfoeuille (2024) which is robust to treatment effect heterogeneity and allows for non-binary treatments. Our outcome is at the monthly level, with stock return volatility being measured as the the standard deviation of monthly abnormal returns. We limit the analysis to a 6 month window around events (three pre- and post- periods).

Figure 7 shows the effects of election filings on volatility. The coefficients represent the change in volatility caused by an additional filing in month t relative to when the filing takes place, $t = 0$. While the pre-period coefficients are noisy, we fail to reject the joint null hypothesis of no treatment effects in the pre-treatment period ($p = 0.10$). The post-period coefficients indicate that an additional establishment-level filing increases return volatility. An additional filing has an average total effect of 41.4 additional basis points on the standard deviation of stock returns across the four months during and after the event (the 95 percent confidence interval for the average effect over the four-month period is [11.5, 64.0]).[17]

Around closings, we find a negative but imprecise effect on volatility. Our point estimates suggest an additional firm-level election closure in a given month reduces volatility by 4.5 basis points on average for the four months during and after closing an election, but the confidence intervals are wide (the 95 percent CI is [-25.9,16.7]). Figure 8 shows the associated dynamic estimates for the six month window around election closures. Although the coefficients are all negative in the pre-period, we cannot reject the null hypothesis of no treatment effects in the three months before closings are filed with the NLRB ($p = 0.27$).

[17]The total average treatment effect from de Chaisemartin and d'Haultfoeuille (2024) which we report here does not readily translate into a single effect on the volatility of returns for the entire period during and after elections. This effect is the sum of the increase in monthly standard deviations of daily stock returns (measured in basis points) caused by election filings, for each of the four months including and following an additional filing, across each of these individual months.

7 Conclusion

This paper presents new evidence on how NLRB union representation elections impact firm stock returns. We begin by constructing a novel dataset that links election events to stock returns. To assemble a comprehensive record of all NLRB elections from 1961 to 2023, we merge existing NLRB data with new data obtained through a FOIA request. We then compile a list of all publicly listed firms and their subsidiaries using datasets from WRDS, merging this with our election data based on firm names to create a longitudinal panel of elections and stock returns. This dataset allows us to measure contemporaneous effects of election events on stock returns from 1994 and 1961, respectively, through 2023.

To identify the causal impact of union elections on stock returns, we implement a DiD design using firms that have experienced at least one successful election since 1961. This narrower sample, unlike prior approaches relying on regression discontinuity (RD) or DiD with all firms (Dinlersoz, Greenwood, and Hyatt, 2017; Frandsen, 2021), reduces potential selection biases. Specifically, if firms with successful elections differ systematically from those without, including all firms as a comparison group could conflate treatment effects with differences arising from firms being able to selectively prevent unions from winning elections.

We find that, across a broad set of specifications, markets do not punish firms for individual elections. Under our benchmark specification, an additional election filing causes a decline in firm stock returns by roughly 7 basis points. However, when the election is certified with the NLRB, there is a positive effect of 7 basis points, which offsets the negative effect of filing. We can reject that the net effect of filing and closing an election larger than 10.6 basis points in magnitude (less than two days' of average returns) at the 95% level. This qualitative pattern is robust to alternative formulations of the treatment variables, the set of firms we use as controls, and the measure of stock returns used. Furthermore, in conducting heterogeneity analysis by election characteristics, we find that effects are qualitatively similar across different election types. The one exception to this pattern are relatively large, positive effects from closing first elections.

We close the paper with suggestive evidence that the initial drag and subsequent rebound of stock returns is driven by increased volatility in returns in the wake of filing an election. In the months during and following a filing, we see a significant increase in the volatility of stock returns at treated firms. This is partially counteracted by a decline in volatility after elections close (although the point estimates are not significant).

Previous research suggests that multi-establishment firms adjust production in response to union activity, which could help explain our firm-level null result. Firms may reallocate production across plants (Wang and Young, 2022) or shed longer-tenured employees (Frandsen, 2021) in response to unionization, actions that could mitigate any drag on stock returns from union victories. Another possible explanation is a reduction in the union wage premium during our study period. Farber et al. (2021) shows that the union wage premium declined in the 2000s relative to the peak levels of the 1970s and 1980s. These trends may further explain the difference between our recent null results and the negative effects observed in studies of earlier periods (Lee and Mas, 2012).

Our study provides new evidence that may inform the long running political discourse surrounding organized labor, which has traditionally framed labor unions and firm owners as adversaries.[18] Trade unions have gained renewed prominence, with recent studies linking their decline to rising income inequality (Dinlersoz and Greenwood, 2016; Farber et al., 2021) and highlighting their reemergence at the forefront of the American political economy (Naidu, 2021).[19] Our evidence suggests that, contrary to popular narratives, new unionization efforts do not pose a large threat to returns on the equity in affected public firms. Future work examining the mechanisms by which firms circumvent negative effects of unionization on profits or showing how countervailing forces, such as increasing productivity, could explain the null effects we find.[20] Until then, our findings suggest that the perceived negative effect of unionization on stock prices as portrayed in popular narratives is difficult to reconcile with realized outcomes in the 21st century.

[18] Indeed, Marx and Engels (1848) write explicitly in their manifesto that trade unions form "against the bourgeois" so as to lower the profits earned by firms.

[19] See Kaplan and Naidu (2024) for a survey of the current literature on unions and political economy.

[20] Barth, Bryson, and Dale-Olsen (2020) find unions do increase productivity at Norwegian firms.

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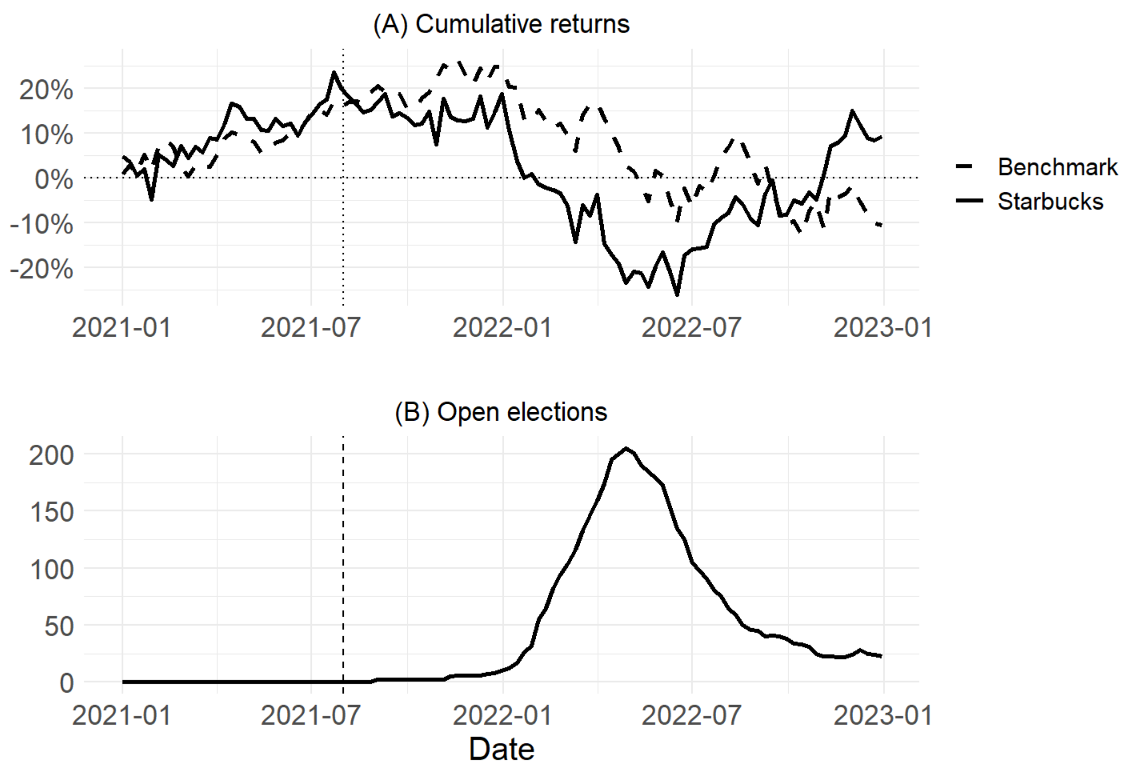
Wang, Sean and Samuel Young (2022). "Unionization, Employer Opposition, and Establishment Closure". *Essays on Employment and Human Capital, PhD diss. MIT*.

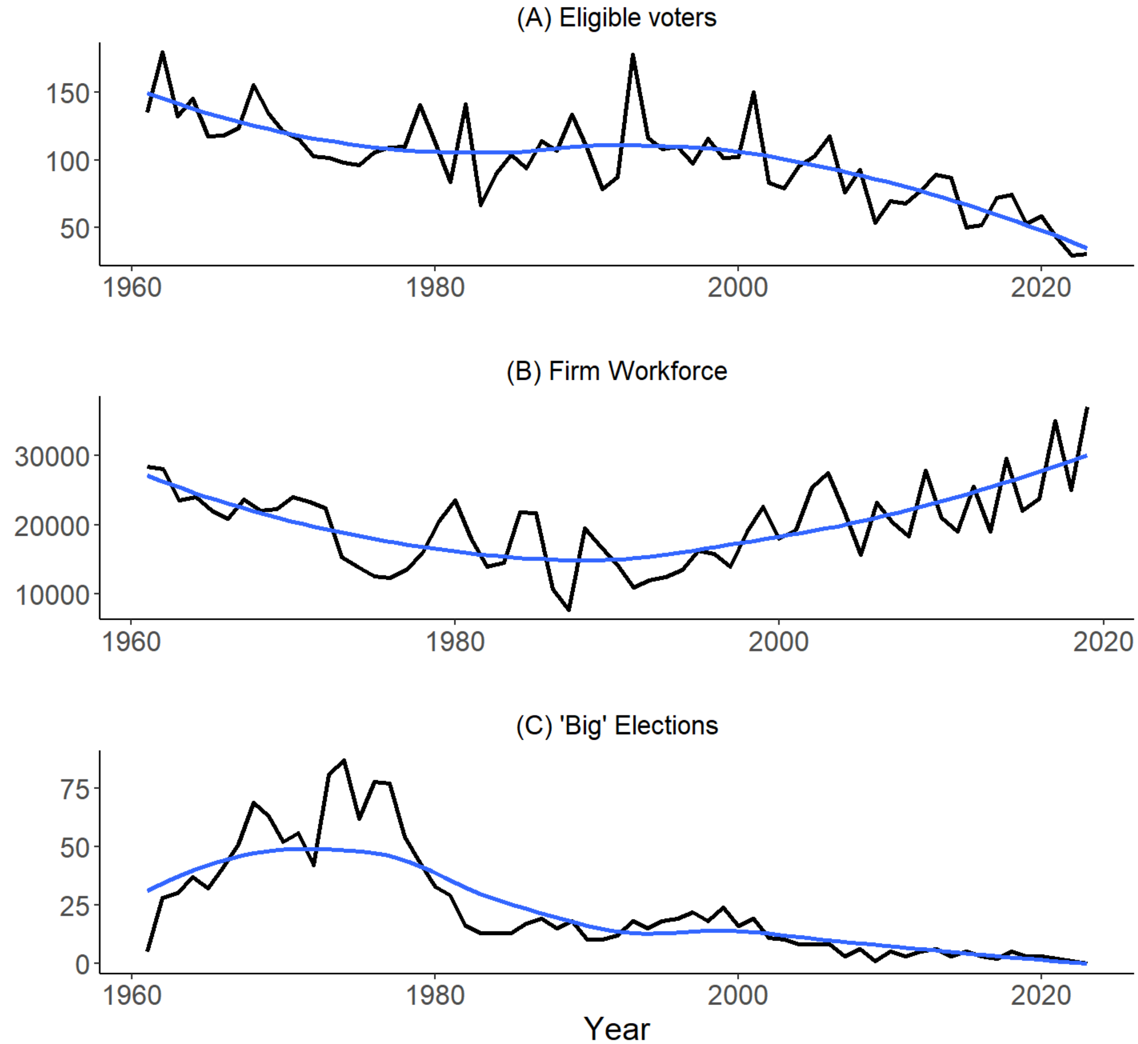
8 Figures

Figure 1: Union elections and stock returns at the Starbucks Corporation



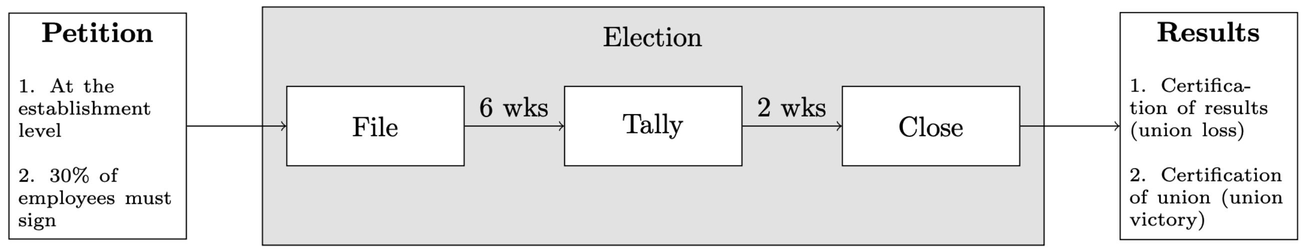
Note: This figure shows cumulative returns (Panel (A)) and open union elections (Panel (B)) at Starbucks between in 2021 and 2022. The benchmark is a value-weighted portfolio of firms in the same size decile and on the same exchange as Starbucks

Figure 2: Number of eligible voters, firm employees, and "big" elections, NLRB elections at public firms



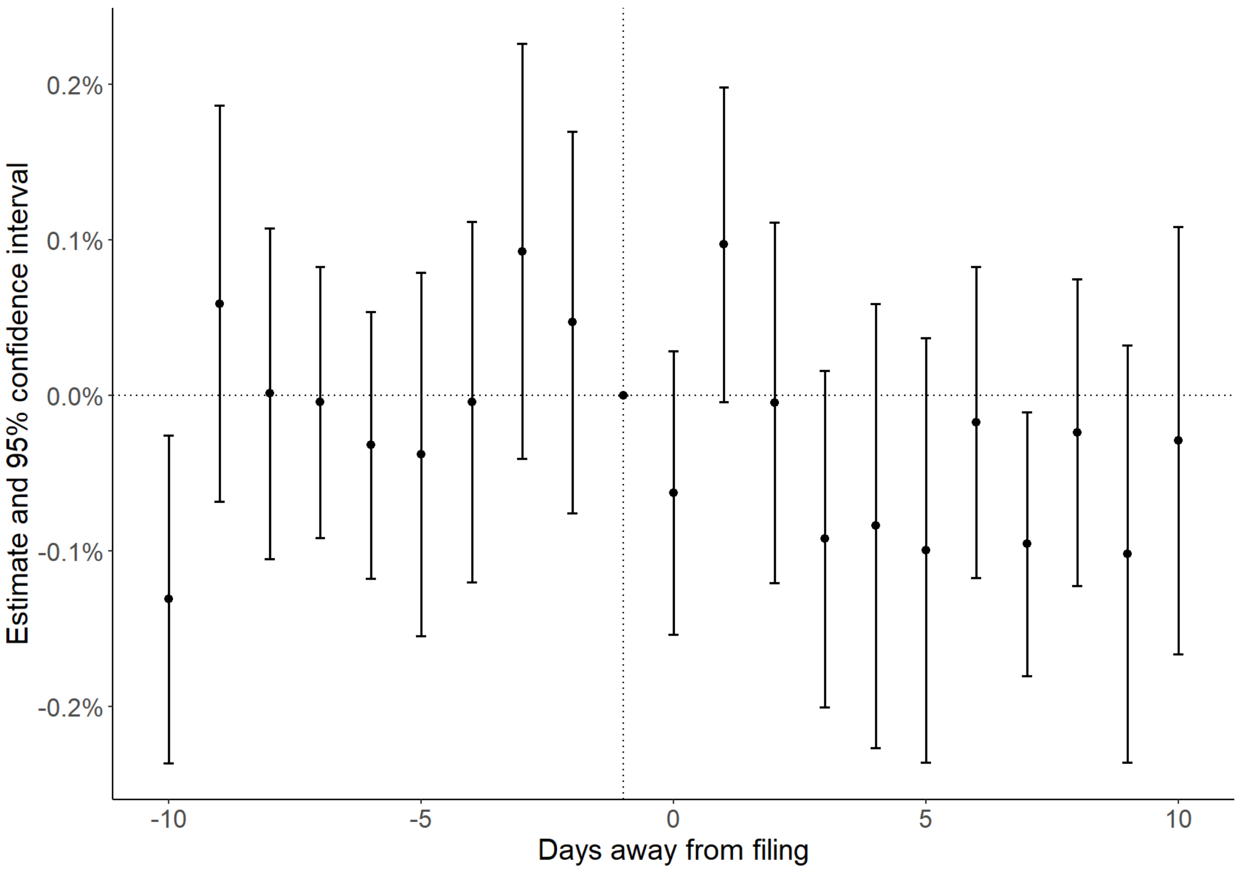
Note: This figure shows the changes in the size of elections and the firms where they occur from 1961 through 2023. Panel (A) displays the average size of bargaining units in NLRB elections at public firms. Panel (B) displays the number of employees at these firms using data from the Compustat/CRSP dataset provided by Wharton Research Data Services (WRDS). Panel (C) displays NLRB elections with at least 100 eligible voters and 5% of the firm's workforce eligible to vote. These are the elections deemed big by Lee and Mas (2012). The black line shows raw count data from each year while the blue line shows the fitted values from a non-parametric regression.

Figure 3: NLRB union representation election process



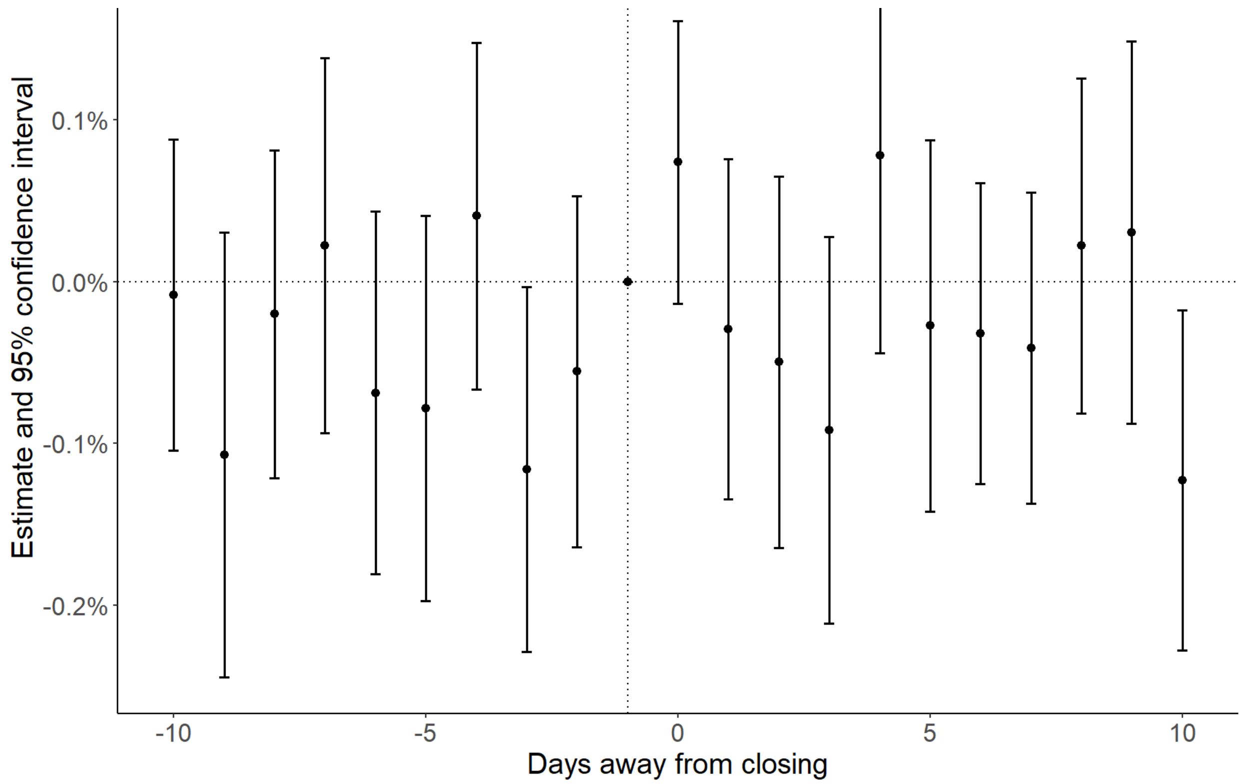
Note: This figure summarizes the NLRB election process. A election is filed when 30% of employees within a bargaining unit sign a petition to the NLRB. The NLRB then conducts the election. On average, there are 6 weeks between filing and the tallying of votes, and 2 weeks between tallying and closing, when the NLRB certifies the outcome.

Figure 4: Dynamic difference-in-differences, 10 days before and after filing a winning election



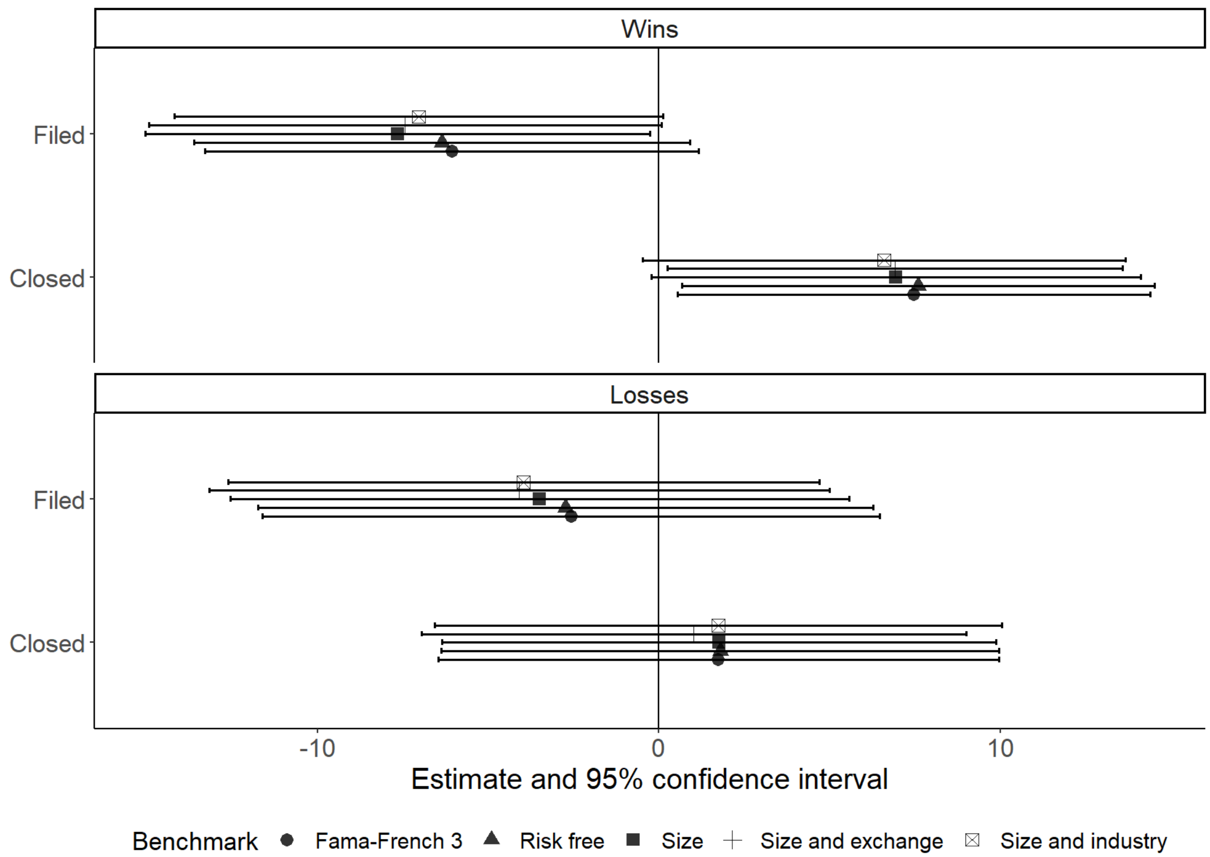
Note: This figure plots point estimates and 95% confidence intervals on the β_k coefficients from Equation 9 for the 20 days around filing a winning election. The day before filing is left out so that all estimates are relative to this day. Multi-way standard errors are clustered at the firm and trading day level. The p-value for joint significance of the pre-period coefficients are 0.31. The p-value for joint significance post-period p-values is 0.06.

Figure 5: Dynamic difference-in-differences, 10 days before and after closing a winning election



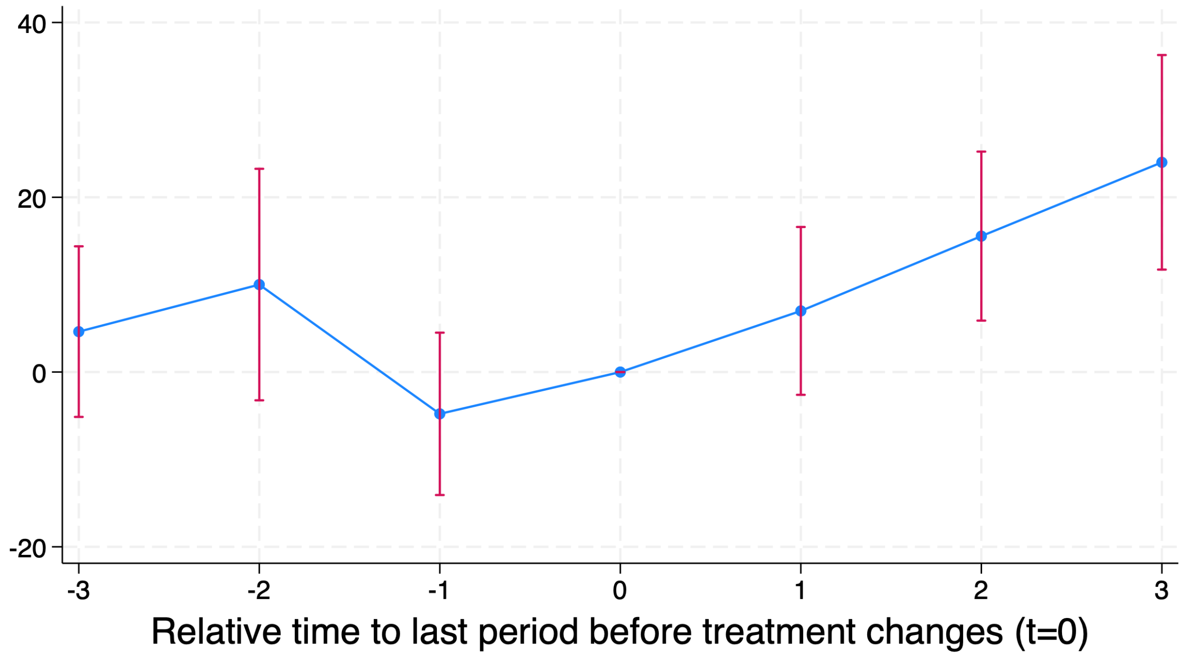
Note: Point estimates and 95% confidence intervals on the β_k coefficients from Equation 9 for the 20 days around closing a winning election. The day before closing is left out so that all estimates are relative to this day. Multi-way standard errors are clustered at the firm and trading day level. The p-value for joint significance of the pre-period coefficients are 0.29. The p-value for joint significance post-period p-values is 0.24.

Figure 6: Robustness to different benchmarks



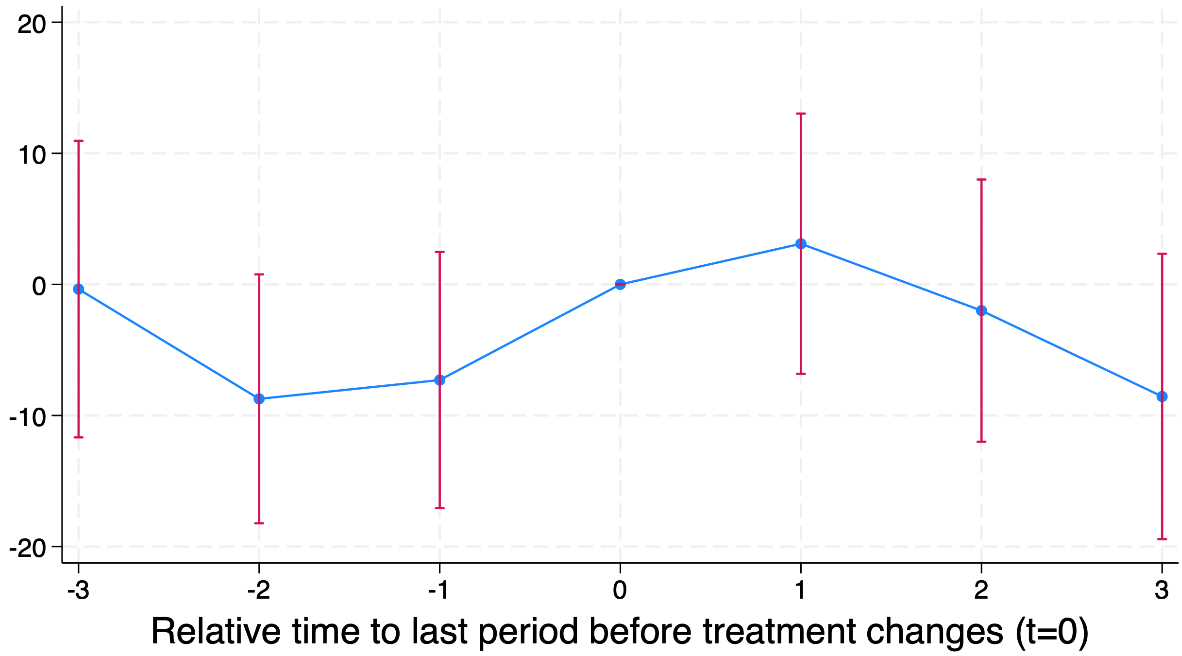
Note: This figure displays point estimates for β_f and β_c from Equation 6 when abnormal returns, AR_{it}, are calculated using five different benchmarks. All estimates are reported in basis points (one-hundredth of a percentage point). Parentheses show multi-way standard errors clustered at the firm and trading day level. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Figure 7: Monthly return volatility around filings



Note: Event study estimates for the three months around the month that an election is filed on the daily standard deviation in firm-level stock returns using the *did_multiplegt_dyn* estimator of de Chaisemartin et al. (2024) with filings as the treatment of interest and closures added as a control variable. Red bands on each point show 95 percent confidence intervals.

Figure 8: Monthly return volatility around closures



Note: Event study estimates for the three months around the month that an election is closed on the daily standard deviation in firm-level stock returns using the *did_multiplegt_dyn* estimator of de Chaisemartin et al. (2024) with closures as the treatment of interest and filings added as a control variable. Red bands on each point show 95 percent confidence intervals.

9 Tables

Table 1: Matched elections, 1961–2023

Years	Distinct firm names	Elections	Matched firm names	Matched elections	Names matched (%)	Elections matched (%)
1961–1964	17,133	21,220	552	1,981	3%	9%
1965–1969	25,014	34,662	915	4,717	4%	14%
1970–1974	27,143	37,846	1,315	5,485	5%	14%
1975–1979	26,834	35,666	1,249	4,089	5%	11%
1980–1984	18,329	22,663	710	1,746	4%	8%
1985–1989	14,238	16,848	437	849	3%	5%
1990–1994	12,295	15,118	391	745	3%	5%
1995–1999	11,704	15,110	934	1,855	8%	12%
2000–2004	9,584	12,428	852	1,697	9%	14%
2005–2009	5,912	8,131	545	991	9%	12%
2010–2014	4,968	7,016	525	945	11%	13%
2015–2019	4,270	6,623	542	1,131	13%	17%
2020–2023	3,155	5,012	153	687	5%	14%

Note: This table lists the number of firm names and elections in the NLRB data along with percents matched to the CRSP data over 5-year periods. Distinct firm names are all distinct names (after string cleaning) in the NLRB data. Elections are the total number of elections at these firms during each period. Matched firm names are the total number of names that we matched to a public company at the time of the election. Matched elections are the total number of elections at firms with a matched name.

Table 2: Summary statistics, NLRB elections 1961–2023

Average	Non-matched elections	Matched elections	Matched elections: Wins	Matched elections: Losses
	(1)	(2)	(3)	(4)
Avg. % vote for union	57%	58%	80%	32%
	[28]	[28]	[17]	[14]
Avg. num. eligible	62	109	85	137
	[28]	[28]	[17]	[14]
Avg. pct. eligible	NA	4%	4%	4%
		[128]	[166]	[61]
Avg. election length	92	92	81	104
	[141]	[151]	[123]	[177]
Avg. year	1982	1982	1982	1982
	[15]	[17]	[18]	[15]
Avg. firm size	NA	56,746	60,746	52,231
		[103705]	[104186]	[102978]
Total number	211,424	26,647	14,285	12,362

Note: This table lists average values for all NLRB elections between 1961 and 2023. Standard deviations are reported in brackets. Columns (1) and (2) are computed for elections not matched to the CRSP ("non-matched") and elections matched to the CRSP ("matched"). Columns (3) and (4) split matched elections by outcome.

Table 3: Summary statistics, daily CRSP data 1994–2023

Average	Firms with		Firms with wins with	
	No elections since 1961	With wins since 1961	No wins after 1994	Wins after 1994
	(1)	(2)	(3)	(4)
# Employees	3,237	21,872	13,525	33,364
	[19,817]	[71,719]	[31,795]	[102,985]
Return	8.28	6.51	6.79	6.12
	[560.73]	[347.2]	[365.79]	[319.42]
Benchmark Return	4.13	4.74	4.81	4.64
	[126.28]	[122.32]	[120.91]	[124.26]
Delisted	0.272	0.123	0.118	0.137
Merged	0.437	0.239	0.226	0.274
Acquired	0.004	0.012	0.009	0.022
Liquidated	0.020	0.001	0.000	0.002
Firms	12,927	3,165	2,288	877
Observations	23,101,866	8,043,903	4,688,270	3,355,633

Note: This table lists average values for returns and benchmark returns. Numbers are displayed in basis points (units of 0.01 percentage point) and are calculated at the daily level. Returns are the cumulative dividend return between trading days. The benchmark is a value-weighted portfolio of firms in the same size decile and listed on the same exchange. Standard deviations are in brackets. Columns (2) and (3) are CRSP firms that are not matched to the NLRB data ('No elections') and those that are ('With elections'). Columns (4) and (5) split matched firms into those with and without wins after 1994, i.e., our treated and never-treated groups.

Table 4: The effect of filing and closing winning elections on daily abnormal stock returns

Dep. Variable: AR_{it}	(1)	(2)	(3)
Wins filed	-7.654**	-7.436*	-6.734*
	(3.711)	(3.831)	(3.732)
Wins closed	7.115**	6.908**	6.382*
	(3.342)	(3.402)	(3.403)
Fixed-effects			
Date	No	Yes	Yes
Firm	No	Yes	Yes
Firm-level FF3	No	No	Yes
Observations	8,042,828	8,042,828	8,042,828
$p(H_0 : \beta_c + \beta_f = 0)$	0.91	0.92	0.95

Note: This table shows the results from estimating Equation 6. Estimates are reported in basis points (one-hundredth of a percentage point). Parentheses show multi-way standard errors clustered at the firm and trading day level. Our preferred specification is in Column (2). Firm-level "FF3" allows for firm-level loadings α_i on the daily Fama-French (1993) factors X_t. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Table 5: The effect of union elections on abnormal stock returns by election outcome

Dep. Variable: AR_{it}	(1)	(2)	(3)	(4)
Elections filed	-5.832*			
	(2.994)			
Elections closed	4.437*			
	(2.394)			
Wins filed		-7.436*		-7.293*
		(3.831)		(3.797)
Wins closed		6.908**		6.938**
		(3.402)		(3.420)
Losses filed			-4.089	-3.824
			(4.633)	(4.634)
Losses closed			1.016	0.7465
			(4.069)	(4.090)
Fixed-effects				
Date	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Observations	8,042,828	8,042,828	8,042,828	8,042,828
$p(H_0 : \beta_f + \beta_c = 0)$	0.71	0.92	0.64	—

Note: This table shows the results from estimating Equation 8. All estimates are reported in basis points (one-hundredth of a percentage point). Parentheses show multi-way standard errors clustered at the firm and trading day level. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Table 6: Tests of joint significance

Test	p-value
(i) $\beta_{wf} = \beta_{lf}$	0.56
(ii) $\beta_{wc} = \beta_{lc}$	0.28
(iii) $\beta_{wf} + \beta_{wc} = 0$	0.94
(iv) $\beta_{lf} + \beta_{lc} = 0$	0.62
(v) $\beta_{wf} = \beta_{wc}$	0.01
(vi) $\beta_{lf} = \beta_{lc}$	0.46

Note: This table shows the results of Wald tests of various linear combinations of the estimated coefficients from Equation (8). Each row shows the hypothesis we test and the associated p-values. Multi-way standard errors are clustered by firm and date.

Table 7: Heterogeneity by election characteristics

Dependent Variable:	AR_{it}			
Model:	(1)	(2)	(3)	(4)
Win filed dummy	-3.797	-8.227*	-4.740	-5.272
	(4.255)	(4.611)	(4.579)	(4.414)
Win closed dummy	8.659*	3.936	2.067	7.956*
	(4.534)	(4.958)	(4.617)	(4.648)
Pct. Elig × Win filed dummy	-1.409			
	(1.390)			
Pct. Elig × Win closed dummy	-0.7540			
	(1.777)			
Duration × Win filed dummy		0.0290		
		(0.0222)		
Duration × Win closed dummy		0.0544*		
		(0.0300)		
$\mathbb{I}\{\text{First}\}\times$ Win filed dummy			-5.086	
			(11.17)	
$\mathbb{I}\{\text{First}\}\times$ Win closed dummy			27.42**	
			(12.51)	
$\mathbb{I}\{\text{Narrow}\}\times$ Win filed dummy				-9.853
				(15.15)
$\mathbb{I}\{\text{Narrow}\}\times$ Win closed dummy				5.498
				(18.20)
Fixed-effects				
Date	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Observations	8,037,295	8,037,308	8,037,308	8,037,308

Note: "Win filed dummy" and "win closed dummy" are dummies equaling one if any wins are filed or closed on a day. "Pct. Elig." is the sum of all eligible voters in elections filed or closed divided by the number of firm employees. "First" is a dummy if the winning election is the first one at the firm. "Narrow" is a dummy equal to one if the percent of voters voting for the union is between 45% and 55% across all elections. All estimates are reported in basis points (one-hundredth of a percentage point). Parentheses show multi-way standard errors clustered at the firm and trading day level. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Table 8: Effects of union elections under different benchmarks for expected returns

Model:	(1)	(2)	(3)	(4)	(5)
Benchmark used:	Size & exchange	Size & industy	FF 3 Factor	Risk free	Size
Wins filed	-7.436*	-7.037*	-6.063	-6.365*	-7.658**
	(3.831)	(3.647)	(3.688)	(3.707)	(3.772)
Wins closed	6.908**	6.596*	7.464**	7.597**	6.941*
	(3.402)	(3.605)	(3.535)	(3.535)	(3.658)
Losses filed	-4.089	-3.968	-2.570	-2.741	-3.495
	(4.633)	(4.417)	(4.613)	(4.597)	(4.626)
Losses closed	1.016	1.731	1.745	1.791	1.761
	(4.069)	(4.239)	(4.191)	(4.167)	(4.141)
Fixed-effects					
Firm	Yes	Yes	Yes	Yes	Yes
Date	Yes	Yes	Yes	Yes	Yes
Observations	8,042,828	8,043,901	8,004,785	8,036,956	8,042,828

Note: This table reports point estimates for β_f and β_c from Equation 8 when abnormal returns, AR_{it}, are calculated using five different benchmarks. All estimates are reported in basis points (one-hundredth of a percentage point). Parentheses show multi-way standard errors clustered at the firm and trading day level. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Table 9: Filings and Closures, DiD_M^f Estimator

Dep. Variable: AR_{it}	
Wins filed	6.022
	[9.676]
Wins closed	9.381
	[8.836]

Note: This table reports estimated coefficients for wins filed and wins closed using estimators from de Chaisemartin and d'Haultfoeuille (2023a). Bootstrapped standard errors clustered at the firm level are shown in brackets.

Appendix

Contents

A Fuzzing Matching and Crosswalk Between NLRB Elections and Firms in the CRSP

Fuzzy matching After doing an exact match on clean firm names, we take the remaining names and use a fuzzy merge, resulting in an additional 6,306 matches. To do so, we used the `R` package `Fuzzyjoin` (Robinson, 2020). This package contains a function that uses a Jaro-Winkler Distance (JWD) value to select suitable matches. JWD evaluates dissimilarity between strings on a scale from 0 to 1, with 1 being assigned to strings with no characters in the same position, and 0 for exact matches. We evaluated different JWD thresholds and compared the number of added correct matches and the number of false-positives, that is the number of names that it identified as matching even although we judged them to be different firms. Based on this exercise, we chose a threshold of 0.06 (the maximum, or most lenient value is 1, a value of 0 selects only exact matches).

Crosswalk creation To start, we create a complete list of firm name and PERMCO combinations in the CRSP. For this, we use three datasets in the CRSP. We use the "Subsidiary Data" for firm names and linking data from the "CRSP/Compustat merged" dataset. The subsidiary data identifies parent firms and subsidiaries for firms filing with the SEC between 1994 and 2022. Subsidiaries themselves are not often publicly traded, so their names may not appear in the CRSP Stock dataset — in fact, the majority of names are from the subsidiary dataset. With this in mind, our matching procedure contains three steps:

1. **CRSP Panel** We first created a panel of every date between first and last dates for PERMCO/name combinations. With the subsidiary data, we take the first time a firm is listed as a subsidiary on an SEC filing up until a filing that does not list the firm as a subsidiary — this assumes that the firm was a subsidiary from the moment it is first listed until the date it is no longer listed. For every day that a firm has a PERMCO, either directly or from a parent, there is an observation. Likewise, for firms in the Stock data, we fill in all dates between the first appearance of a PERMCO/name combination and the last observation. To be clear, there may still be gaps in a firm's CRSP data: a firm name and or PERMCO may be in the data for a period of time before leaving the data and reappearing with a different name or PERMCO.

2. **NLRB panel** We fill in all dates between the first election filed and the last election closed for each cleaned firm name. Next, we create cumulative sums of elections and wins filed and closed and total counts for each name.

3. **Crosswalk** We append the two panels, so that we have a list of all dates between elections for each firm name and all dates that have a PERMCO associated with a

name. If a name has elections prior to having a PERMCO, that is, prior to entering the CRSP data, then these elections will be attributed to the PERMCO associated with the name upon entering the CRSP. We do not assign PERMCOs to firms outside of the periods when they are explicitly tied to a PERMCO in the CRSP. Once we have the crosswalk, it is simple to merge in election and stock data.

Table 1 displays the number of firm names that we match to the CRSP *while the firm is public or is a subsidiary of a public firm.* To further assess our matching accuracy, we match based on name alone. We remove all elections that have fewer than 100 eligible voters, leaving us with just under 36,000 elections. We also remove universities and hospitals/medical centers, as these are unlikely to be publicly traded. Table A.1 reports the results from this matching exercise. We typically match between 20 and 30 percent of names. These names constitute between roughly 20 and 30 percent of eligible voters.

Table A.1: Names matched between the NLRB and CRSP

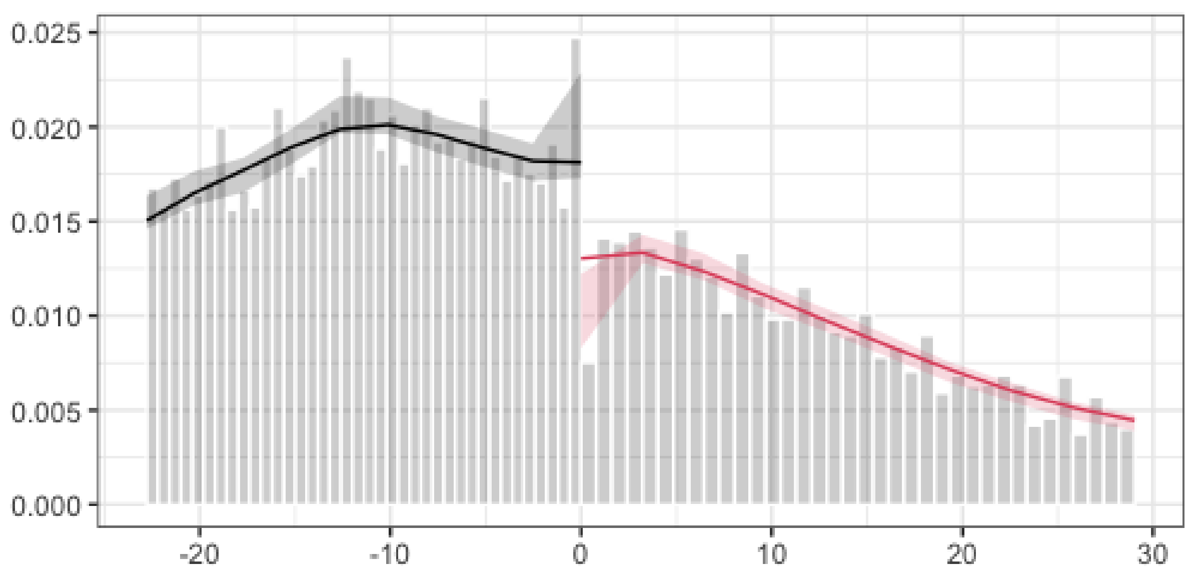
Years	Unique firm names	Matched firm names	Matched eligible voters	Tot. eligible voters	Pct. firm names matched	Pct. voters matched
1961–1964	2,337	398	152,992	649,256	17%	24%
1965–1969	3,317	704	236,919	891,428	21%	27%
1970–1974	2,927	701	247,682	778,577	24%	32%
1975–1979	2,634	605	180,420	624,457	23%	29%
1980–1984	1,816	380	109,742	445,928	21%	25%
1985–1989	1,524	285	79,899	338,475	19%	24%
1990–1994	1,424	306	78,832	327,472	21%	24%
1995–1999	1,667	390	108,270	395,221	23%	27%
2000–2004	1,184	263	68,314	270,230	22%	25%
2005–2009	604	120	30,131	150,372	20%	20%
2010–2014	662	175	47,306	154,874	26%	31%
2015–2019	539	128	29,002	128,628	24%	23%
2020–2023	291	56	16,199	84,259	19%	19%

Note: Firm names matched from the NLRB to the CRSP. We only includes elections with at least 100 workers eligible. We also removes hospitals, medical centers, and schools/universities, which are not publicly listed firms.

B Test of Continuity in Running Variables for Regression Discontinuity Design Identification

A regression discontinuity (RD) design is not identified when the distribution of the running variable is not continuous around the treatment threshold. McCrary (2008) was the first to popularize a test of continuity around the treatment threshold. Figure B.1 gives graphical evidence of a discontinuity in vote shares around the 50% cutoff. The fitted values local polynomial from the local polynomial estimator generated by the *rddensity* function (Cattaneo, Jansson, and Ma, 2018) are overlaid on the graph; the test proposed by (Cattaneo, Jansson, and Ma, 2018) rejects the null hypothesis of no sorting around the cutoff of the running variable (vote shares) at the 95 percent level. These results indicate that RD estimates for the effect of union elections may not have a causal interpretation.

Figure B.1: Density of union vote share



Note: Density plot of percent of vote received by the union created using the procedures described in Cattaneo, Jansson, and Ma (2018). Only includes elections at public firms with at least 30 eligible voters. We reject the null hypothesis of continuity at the threshold (p-value = 0.02), confirming that there is a discontinuity in the density of firms around the 50% voting threshold.

C Weekly, Monthly, and Annual analysis

We re-estimate Equation (6) for weekly, monthly and annual returns. Estimates for the effects of monthly and annual returns should be interpreted with caution as there are other factors that are likely impacting returns when considering wider time frames. Again, we limit the sample to 1994 onward and only firms with at least one winning election after 1961. We estimate the following equation:

$$\bar{AR}_{i\tau} = \beta_f \bar{W}^f_{i\tau} + \beta_c \bar{W}^c_{i\tau} + \alpha_i' X_\tau + \gamma_\tau + \delta_i + \varepsilon_{i\tau} \tag{C.1}$$

where τ are weeks, months, or years. For the weekly analysis we aggregate daily returns for firms and the benchmark using the following formula:

$$\bar{AR}_{i\text{weekly}} = \left(\exp\left(\sum_{t\in\tau}\log(1+AR_{it})\right) - 1\right)$$

Then we compute weekly abnormal returns as the difference between weekly firm returns and weekly benchmark returns. For the monthly analysis, we use monthly data directly from the CRSP. For election counts we simply take the sum of daily counts over each week or month, we denote these aggregated variables using overhead bars.

Tables C.1, C.2, and C.3 show the alternative results for returns over a longer time horizon. Each table contains three columns that mirror the various specifications at the daily level in Table 4. We can reject net treatment effects larger than 16, 39, and 28 basis points on weekly, monthly, and annual returns at the 95 percent level. These effects are no larger than 3, 39, and 3 percent of firms' average weekly, monthly, and annual returns.

When we examine effects at the monthly frequency, we do find some evidence of a persistent, one time negative effect from filings. The point estimate of the net effect is small but highly significant ($p < 0.01$). An additional election is associated with a decline in abnormal returns of 25 basis points, or about 39 percent of average returns among all firms in our sample at the monthly frequency.

Table C.1: The effect of winning elections on weekly abnormal returns

Dep. Variable: $AR_{i\tau}$	(1)	(2)	(3)
Wins filed	-6.811	-7.828	-7.478
	(5.797)	(6.212)	(6.141)
Wins closed	4.174	4.679	4.579
	(7.019)	(6.644)	(6.792)
Fixed-effects			
Date	No	Yes	Yes
Firm	No	Yes	Yes
Firm-level FF3	No	No	Yes
Observations	1,664,003	1,663,879	1,656,129
$p(H_0 : \beta_c + \beta_f = 0)$	0.70	0.65	0.94

Note: Estimates of Equation at the weekly level. All estimates are reported in basis points (one-hundredth of a percentage point or 0.0001). Parentheses show multi-way standard errors clustered at the firm and year level. Firm-level "FF3" allows for firm-level loadings α_i on the daily Fama-French (1993) factors X_t. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Table C.2: The effect of winning elections on monthly abnormal returns

Dep. Variable: $AR_{i\tau}$	(1)	(2)	(3)
Wins filed	-28.592***	-28.867***	-30.384***
	(5.994)	(5.501)	(5.807)
Wins closed	8.816	5.960	5.172
	(6.738)	(5.748)	(6.218)
Fixed-effects			
Date	No	Yes	Yes
Firm	No	Yes	Yes
Firm-level FF3	No	No	Yes
Observations	435,096	434,884	433,040
$p(H_0 : \beta_c + \beta_f = 0)$	0.00	0.00	0.00

Note: Estimates of Equation at the monthly level. All estimates are reported in basis points (one-hundredth of a percentage point or 0.0001). Parentheses show multi-way standard errors clustered at the firm and year level. Firm-level "FF3" allows for firm-level loadings α_i on the daily Fama-French (1993) factors X_t. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Table C.3: The effect of winning elections on annual abnormal returns

Dep. Variable: $AR_{i\tau}$	(1)	(2)	(3)
Wins filed	96.298	-11.031	-105.586
	(73.937)	(73.542)	(91.464)
Wins closed	-113.549	-0.630	92.631
	(79.765)	(74.518)	(94.868)
Fixed-effects			
Date	No	Yes	Yes
Firm	No	Yes	Yes
Firm-level FF3	No	No	Yes
Observations	39,402	39,119	39,119
$p(H_0 : \beta_c + \beta_f = 0)$	0.34	0.22	0.12

Note: Estimates of Equation at the annual level. All estimates are reported in basis points (one-hundredth of a percentage point or 0.0001). Parentheses show multi-way standard errors clustered at the firm and year level. Firm-level "FF3" allows for firm-level loadings α_i on the daily Fama-French (1993) factors X_t. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

D Binary Treatment Specification

There are 161 instances of more than one win filed at a firm on a single day ($W_{it}^f > 1$) and 176 instances of more than one win closed on a day ($W_{it}^c > 1$). It's possible that these days exhibit a large degree of leverage when estimating Equation (6). Table G.2 shows our results from re-estimating Equation (G.1) using binary variables equaling one if any elections filed or closed on a day rather than counts. The coefficients on filing become less precise but the magnitudes and signs of all estimates are similar. Table D.2 shows results from estimating Equation (7) using binary treatment variables. Again, we find negative effects from filing and positive effects from closing. These effects are qualitatively similar to the effects when using count variables.

Table D.1: Estimates of Equation (6) with binary treatment

Dependent Variable: AR_{it}	(1)	(2)	(3)
Win filed dummy	-6.863	-6.136	-5.753
	(4.688)	(4.663)	(4.657)
Win closed dummy	8.044*	7.938*	7.958*
	(4.409)	(4.435)	(4.331)
Fixed-effects			
Date	No	Yes	Yes
Firm	No	Yes	Yes
Firm-Level FF3	No	Yes	Yes
Observations	8,042,828	8,042,704	8,003,588
$p(H_0 : \beta_c + \beta_f = 0)$	0.85	0.78	0.72

Note: Estimates of Equation 6 using dummy variables for any wins filed or closed on a day. All estimates are reported in basis points (one-hundredth of a percentage point or 0.0001). Parentheses show multi-way standard errors clustered at the firm and year level. Firm-level "FF3" allows for firm-level loadings α_i on the daily Fama-French (1993) factors X_t. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Table D.2: Estimates of Equation (7) with binary treatment

Dependent Variable: AR_{it}	(1)	(2)	(3)	(4)
Election filed dummy	-5.270			
	(3.465)			
Election closed dummy	4.678			
	(2.928)			
Win filed dummy		-6.136		-6.069
		(4.663)		(4.641)
Win closed dummy		7.938*		7.943*
		(4.435)		(4.449)
Loss filed dummy			-3.003	-2.900
			(4.819)	(4.811)
Loss closed dummy			1.015	0.8591
			(4.413)	(4.421)
Fixed-effects				
Date	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Observations	8,042,828	8,042,828	8,042,828	8,042,828

Note: Estimates of Equation (7) using dummy variables for any elections, wins, and losses filed and closed on a day. All estimates are reported in basis points (one-hundredth of a percentage point or 0.0001). Parentheses show multi-way standard errors clustered at the firm and year level. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

E Estimating Samples with All Election Firms or All CRSP Firms

Table E.1: Estimates of Equation (6) including all firms with elections

Dep. Variable: AR_{it}	(1)	(2)	(3)
Wins filed	-7.268*	-6.971*	-6.307
	(3.796)	(3.924)	(3.822)
Wins closed	6.959**	7.075**	6.532
	(3.340)	(3.409)	(3.413)
Fixed-effects			
Date	No	Yes	Yes
Firm	No	Yes	Yes
Firm-level FF3	No	No	Yes
Observations	9,767,770	9,767,612	9,720,066
$p(H_0 : \beta_c + \beta_f = 0)$	0.95	0.98	0.97

Note: Estimates from β_f and β_c in Equation (7) including all firms with elections since 1961. All estimates are reported in basis points (one-hundredth of a percentage point). Parentheses show multi-way standard errors clustered at the firm and trading day level. Firm-level "FF3" allows for firm-level loadings α_i on the daily Fama-French (1993) factors X_t. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Table E.2: Estimates of Equation (6) including all CRSP firms

Dep. Variable: AR_{it}	(1)	(2)	(3)
Wins filed	-8.410**	-6.731*	-5.775
	(3.712)	(3.994)	(3.795)
Wins closed	5.794*	8.124**	7.001
	(3.306)	(3.318)	(3.346)
Fixed-effects			
Date	No	Yes	Yes
Firm	No	Yes	Yes
Firm-level FF3	No	No	Yes
Observations	31,121,162	31,120,989	30,773,987
$p(H_0 : \beta_c + \beta_f = 0)$	0.59	0.78	0.81

Note: Estimates from β_f and β_c in Equation (7) including all CRSP firms. All estimates are reported in basis points (one-hundredth of a percentage point). Parentheses show multi-way standard errors clustered at the firm and trading day level. Firm-level "FF3" allows for firm-level loadings α_i on the daily Fama-French (1993) factors X_t. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Table E.3: Estimates of Equation (8) including all election firms

Dep. Variable: AR_{it}	(1)	(2)	(3)	(4)
Elections filed	-6.489**			
	(2.965)			
Elections closed	5.164**			
	(2.444)			
Wins filed		-6.971*		-6.744*
		(3.924)		(3.878)
Wins closed		7.075**		7.082**
		(3.409)		(3.428)
Losses filed			-6.489	-6.277
			(4.642)	(4.649)
Losses closed			2.763	2.502
			(4.205)	(4.226)
Fixed-effects				
Date	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Observations	9,767,770	9,767,770	9,767,770	9,767,770

Note: Estimates of Equation 8 including all firms with elections since 1961. All estimates are reported in basis points (one-hundredth of a percentage point). Parentheses show multi-way standard errors clustered at the firm and trading day level. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

Table E.4: Estimates of Equation (8) including all CRSP firms

Dep. Variable: AR_{it}	(1)	(2)	(3)	(4)
Elections filed	-6.403**			
	(2.977)			
Elections closed	5.643**			
	(2.420)			
Wins filed		-6.731*		-6.494*
		(3.994)		(3.945)
Wins closed		8.124**		8.145**
		(3.318)		(3.340)
Losses filed			-6.636	-6.463
			(4.645)	(4.655)
Losses closed			2.474	2.161
			(4.213)	(4.231)
Fixed-effects				
Date	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Observations	31,121,162	31,121,162	31,121,162	31,121,162

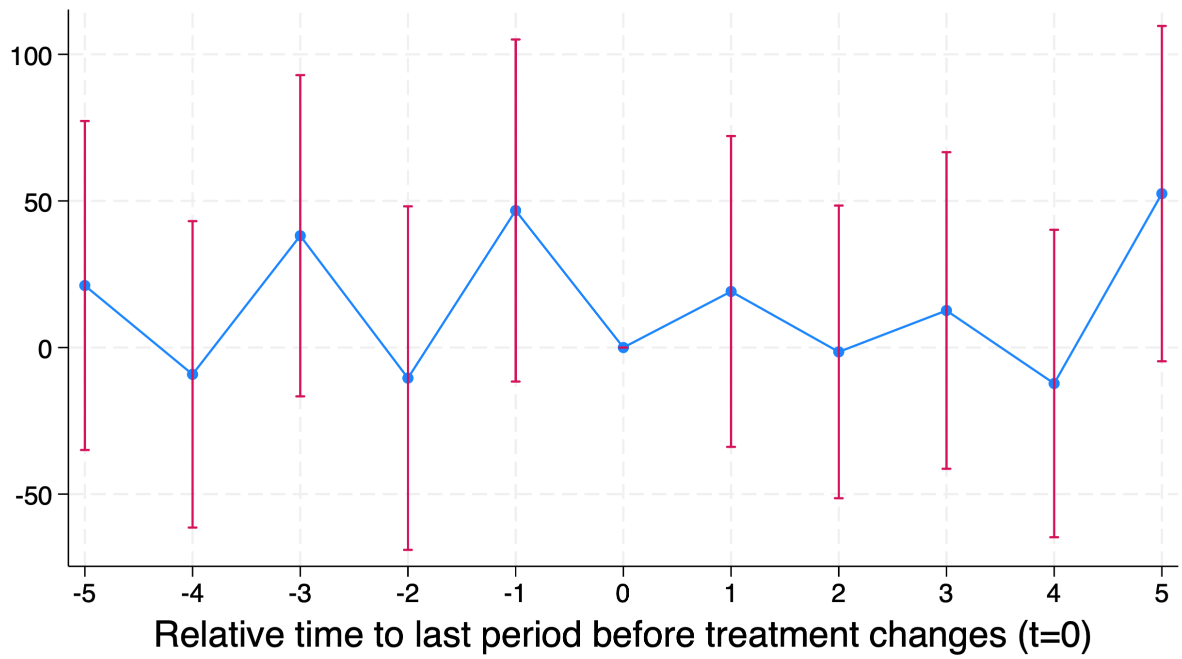
Note: Estimates of Equation 8 including all CRSP firms. All estimates are reported in basis points (one-hundredth of a percentage point). Parentheses show multi-way standard errors clustered at the firm and trading day level. Significance levels: ***: 0.01, **: 0.05, *: 0.1.

F Heterogeneity-Robust Dynamic Estimates

In this section, we test whether election filings and closings have longer-run effects on abnormal returns using heterogeneity-robust dynamic difference-in-difference (DiD) estimators. Like in Section 5.5, we use the dynamic DiD estimator proposed in de Chaisemartin and d'Haultfoeuille (2024), which is robust to treatment effect heterogeneity and captures the average total effects. In this section we measure effects of winning elections the week during and five weeks after filing or closing.

Figure F.1 displays the results of these estimators for a ten week window around filings of winning elections. We cannot reject the joint null hypothesis of no treatment effects in the five weeks before filings ($p = 0.21$). We find an average total effect of 65.5 basis points in the week of and five weeks after filing a win. This effect, although positive (in line with our daily findings using robust DiD estimators in the main text), is imprecisely estimated; We can reject a total negative effect larger (in magnitude) than 126 basis points (1.26 p.p.).

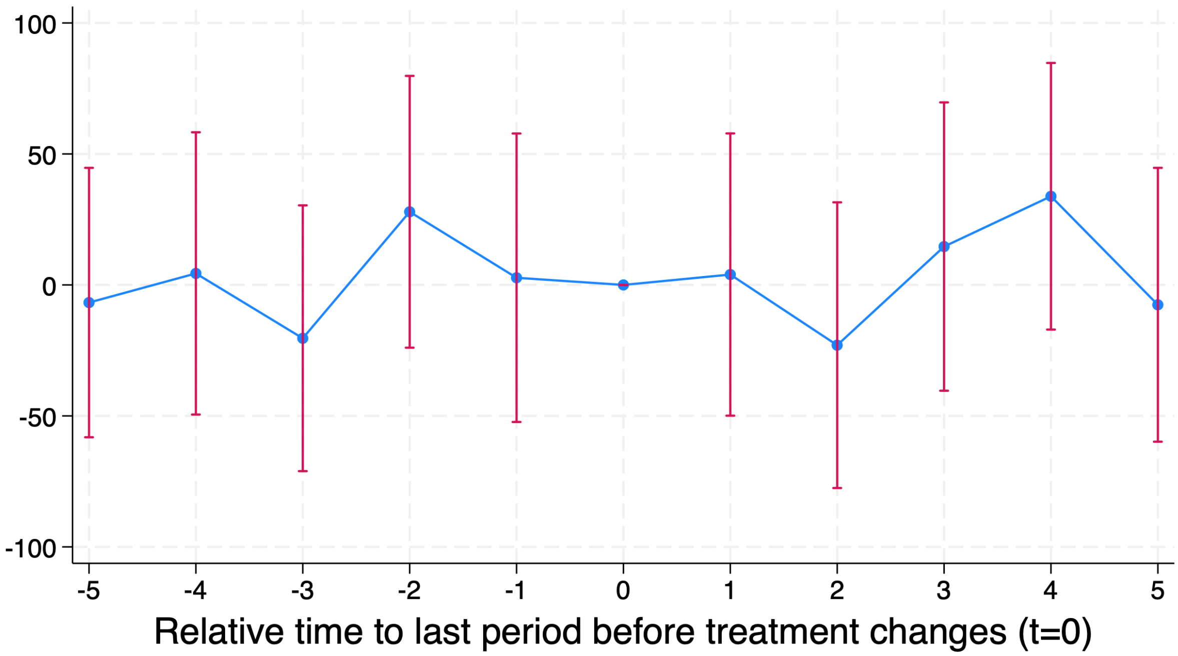
Figure F.1: Dynamic treatment effects in the weeks around filing a winning election using estimators from de Chaisemartin et al. (2024)



Note: Event study estimates in the 10 weeks around filing a winning election. The outcome is weekly abnormal returns. We use estimators from de Chaisemartin et al. (2024). All estimates are reported in basis points (one-hundredth of a percentage point). Confidence intervals are constructed using heteroskedasticity-robust standard errors.

Figures F.2 point estimates and confidence intervals for treatment effects in the 10 weeks around closing a winning election. We find an average total effect of a 20.1 basis points increase in returns in the week of and five weeks following an additional election closure. Although our point estimate is positive (in line with the results in both 5.5 and our main specification), the standard errors are quite large (95.3). We cannot reject the joint null hypothesis of no treatment effects in the five weeks before filings ($p = 0.21$).

Figure F.2: Dynamic treatment effects in the weeks around closing a winning election using estimators from de Chaisemartin et al. (2024)



Note: Event study estimates in the 10 weeks around closing a winning election. The outcome is weekly abnormal returns. We use estimators from de Chaisemartin et al. (2024). All estimates are reported in basis points (one-hundredth of a percentage point). Confidence intervals are constructed using heteroskedasticity-robust standard errors.

G Alternative Measure of Stock Returns

In this section, we examine whether our findings in Section 5 are robust to alternative definitions of our outcome variable. The results here reveal a lack of substantive effects of union elections on returns as measured by this alternative outcome. This holds true across several different treatment specifications.

G.1 Main Specification

For the remained of this section, our outcome of interest is firm-level daily returns in excess of the risk free rate that day:

$$R_{it} - R_{rf,t}$$

This alternative measure of returns follows from the capital asset pricing model literature (Fama and French, 2004). Table G.1 shows results from estimating variations of the following equation:

$$R_{it} - R_{rf,t} = \beta_f W_{it}^f + \beta_c W_{it}^c + \alpha_i' X_t + \gamma_t + \delta_i + \varepsilon_{it} \tag{G.1}$$

where W_{it}^f and W_{it}^c are counts of the number of winning elections filed or closed on day t at firm i. The α coefficients are firm-specific factor loadings on a vector of the three standard Fama-French factors X_t each day take from French's website (Fama and French, 2023). As in the main text, all explanatory variables are divided by 10,000 so that estimates are in basis points.

Table G.1: Estimates from Equation (G.1), $R_{it} - R_{rf,t}$ as Outcome

Dep. Variable: $R_{it} - R_{rf,t}$	(1)	(2)	(3)
Wins Filed	-8.226**	-6.365	-6.650
	(3.841)	(3.707)	(3.688)
Wins Closed	5.844	7.597**	7.631**
	(3.997)	(3.483)	(3.375)
Fixed-effects			
Date	No	Yes	Yes
Firm	No	Yes	Yes
FF3	No	No	Yes
Observations	8,036,956	8,036,832	8,004,661
$p(H_0 : \beta_f + \beta_c = 0)$	0.67	0.81	0.85

Note: Estimates of W_{it}^f and W_{it}^c in Equation G.1. Parenthesis show multi-way standard errors clustered at the firm and trading day level. Firm-level "FF3" allows for firm-level loadings α_i on the daily Fama-French (1993) factors X_t. Signif. Codes: ***: 0.01, **: 0.05, *0.10.

As reported in Table G.1 this alternative definition of stock returns has similar results to our main specification in Table 4. We are again unable to reject the net effect of filing and closing an election being different from zero.

G.2 Alternative Treatment: Indicators of Elections vs. Count Variables

Table G.2 shows our the results of estimating Equation (G.1) when we use a binary definitions of treatment. Results remain virtually unchanged across all specifications. We cannot reject the null hypothesis that filings and closures together have no net effects on stock returns. We reject net contemporaneous effects of union elections on daily returns less than negative 7 basis points at the 95% level.

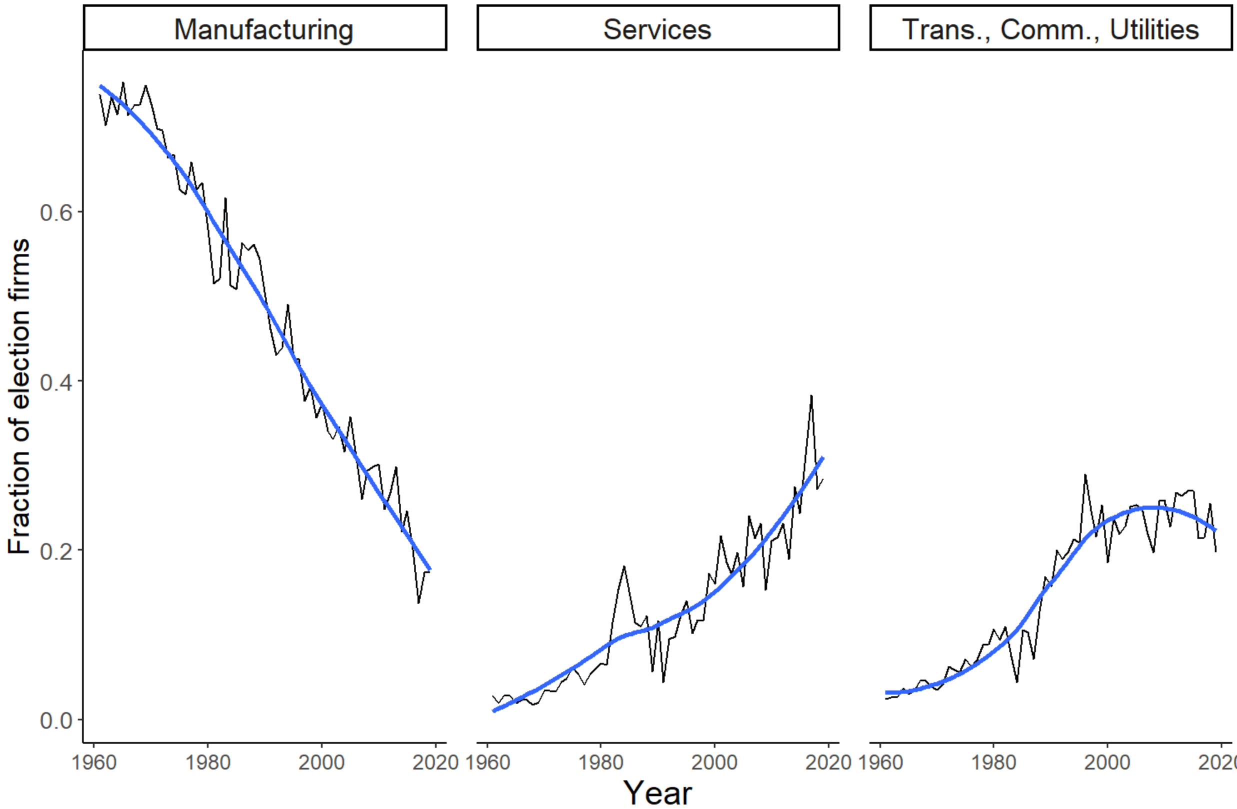
Table G.2: Binary Treatments

Dep. Var: $R_{i,t} - R_{rf,t}$	(1)	(2)	(3)
Win filed dummy	-7.467	-5.339	-5.537
	(5.145)	(4.640)	(4.533)
Win closed dummy	7.829	9.107**	8.892**
	(4.965)	(4.552)	(4.384)
Fixed-effects			
Date	No	Yes	Yes
Firm	No	Yes	Yes
FF3	No	No	Yes
Observations	8,036,956	8,036,832	8,004,661
$p(H_0 : \beta_f + \beta_c = 0)$	0.60	0.44	0.39

Note: Estimates of coefficients on binary treatment variables in Equation G.1. Parenthesis show multi-way standard errors clustered at the firm and trading day level. Firm-level "FF3" allows for firm-level loadings α_i on the daily Fama-French (1993) factors X_t. Signif. Codes: ***: 0.01, **: 0.05, *0.10.

H Miscellaneous

Figure H.1: Share of elections occurring in manufacturing, services, and utilities industries



Note: Share of elections occurring in manufacturing, services, and utilities industries between 1961 and 2019. These are the three sectors with the highest numbers of elections. They comprised more than 70% of elections in 2019.

Table H.1: Election counts, matched CRSP-NLRB

Years	Elections	Big elections	Firms	Avg. firm size	Avg. emps. elig.	Avg. pct. elig.
1961–1965	1,510	97	364	46,493	155	2%
1965–1970	4,125	254	726	53,498	130	2%
1970–1975	5,095	315	1,160	52,507	108	2%
1975–1980	3,824	309	1,142	44,248	112	3%
1980–1985	1,597	101	657	56,218	103	3%
1985–1990	792	80	398	51,153	110	5%
1990–1995	716	61	354	44,347	117	3%
1995–2000	1,709	100	630	40,962	107	2%
2000–2005	1,545	60	535	54,667	103	2%
2005–2010	886	22	341	54,509	95	1%
2010–2015	799	23	291	51,921	80	1%
2015–2020	932	16	277	64,933	60	1%
2020–2023	651	6	109	291,339	35	0%

Note: Counts by 5-year periods. "Big" elections are those with at least 100 eligible voters where this set of workers comprised at least 5 percent of the firm's workforce. Size data is gathered from CCM and merged into the CRSP data.

Table H.2: Summary statistics, NLRB elections 1994–2023

			Matched Elections	
	Non-matched elections	Matched elections	Wins	Losses
Avg. % vote for union	62%	58%	80%	32%
	[29]	[28]	[17]	[15]
Avg. num. eligible	65	86	58	120
	[29]	[28]	[17]	[15]
Avg. pct. eligibile	NA	3%	3%	3%
		[38]	[46]	[25]
Avg. election length	94	95	83	108
	[181]	[196]	[158]	[232]
Avg. year	2005	2006	2008	2005
	[9]	[9]	[9]	[8]
Avg. firm size	NA	73,196	86,948	57,066
		[119384]	[130042]	[103231]
Total number	49,709	7,260	3,942	3,318

Note: Average values for all NLRB elections between 1994 and 2023. Standard deviations are reported in brackets. Columns (i) and (ii) are computed for elections not matched to the CRSP ("non-matched") and elections matched to the CRSP ("matched"). Columns (iii) and (iv) split matched elections by outcome.

I FOIA Request for Election Dates after FY1999

UNITED STATES GOVERNMENT
NATIONAL LABOR RELATIONS BOARD
FREEDOM OF INFORMATION ACT BRANCH
Washington, D.C. 20570

Via email

December 11, 2023

Re: FOIA Case No. NLRB-2023-00336

Dear Alexander Abajian (University of California, Santa Barbara):

This is in response to your request, under the Freedom of Information Act (FOIA), 5 U.S.C. § 552, received on November 21, 2023, in which you seek "a digitized list of the filing, tallying, and closing dates for all NLRB representation elections carried out between 1962 and 2015." You assumed financial responsibility for the processing of your request in the amount of $100.00 and sought expedited processing.

We acknowledged your request on November 21, 2023. Your request for expedited processing was denied for the reasons described in separate correspondence dated December 11, 2023.

Your request is granted in part and denied in part, as described below.

A search of FOIA requests previously processed by this office was conducted for representation election data. This search located the requested data for RC, RD, RM, and UD representation cases and is deemed responsive to your request. We are providing you with three Excel files via the SecureRelease portal.

The first Excel file named "NxGen-RC RD RM UD Election data 10-1-2010 to 9-30-2023.xlsx" contains election data for the above-listed case types for representation cases that were closed and certified between October 1, 2010 and September 30, 2023. The data in this Excel file was originally compiled from searches in the Agency's current electronic casehandling system, NxGen, which generally maintains case records and data from Fiscal Years 2011 to the present.

The second and third Excel files named "CATS-RC RD RM elections-cases closed FY2000-2010.xlsx" and "CATS-UD elections-cases closed FY2000-2010.xlsx" contain election data for the above-listed case types for cases that were closed and certified between October 1, 1999 and September 30, 2023. The data in these Excel files were originally compiled from searches in the Agency's legacy database, Case Activity Tracking System (CATS), which generally maintained case data from Fiscal Years 2000 through 2010. CATS was decommissioned and taken off-line during Fiscal Year 2018, so no additional searches can be conducted by the Agency for data in the CATS database.

Please note for data in all three spreadsheets: If a case has a closing reason or disposition of "Certification of Representative," that means the union was elected to represent the bargaining unit (the union won). If a case has a closing reason or disposition of "Certification of Results," that means the union was not elected to represent the employees (the union lost). No information has been withheld from these records.

Your request is denied to the extent it seeks representation election data for cases that closed prior to Fiscal Year 2000. No additional digitized election records are in the Agency's possession for this time frame, and we are providing a "no records" response.

Pursuant to the Agency's record retention and disposition policy, records are retained for a six-year period, which commences at the close of the calendar year during which the case is closed. The records are then destroyed, unless they are selected for permanent retention based on their legal significance.

Data for representation elections for the time period of 1984 through 2000 were previously maintained in the Agency's Case Handling Information Processing System (CHIPS). However, per the Agency's record retention policy, the raw data tables for CHIPS are now in the possession and control of the National Archives and Record Administration (NARA). If you wish to obtain the CHIPS records from NARA, please visit the NARA website at https://www.archives.gov/research. For guidelines and contact information regarding Services for Off-site Researchers, Research Support Services, or conducting your research on site at NARA, see https://www.archives.gov/research/start/plan-your-visit.

Please be advised that paper records for elections that occurred between 1960 through 1994 were previously maintained by the Agency's Library Services Division. However, the bound volumes containing those records were sent off-site for digitization, and the process is not yet complete. The Agency plans to release the digitized records as PDF-accessible documents on the Agency's website in 2024. Unfortunately, at this time, the records are not available.

For the purpose of assessing fees, we have placed you in Category B, which generally covers educational institutions that operate a program or programs of scholarly research, NLRB Rules and Regulations, 29 C.F.R. § 102.117(d)(1)(vi). We have placed you in this category, because you are a student at an educational institution seeking records to further scholarly research. Accordingly, there is no charge assessed for this request.

You may contact Jodilyn Breirather, the FOIA Specialist who processed your request, at (414) 930-7208 or by email at Jodilyn.Breirather@nlrb.gov, as well as the Agency's FOIA Public Liaison, for any further assistance and/or to discuss any aspect of your request. The FOIA Public Liaison, in addition to the FOIA Specialist, can further explain responsive and releasable agency records, suggest agency offices that may have responsive records, and/or discuss how to narrow the scope of a request in order to minimize fees and processing times. The contact information for the FOIA Public Liaison is:

Kristine M. Minami, FOIA Public Liaison
National Labor Relations Board
1015 Half Street, S.E., 4th Floor
Washington, D.C. 20570
Email: FOIAPublicLiaison@nlrb.gov
Telephone: (202) 273-0902
Fax: (202) 273-FOIA (3642)

After first contacting the Agency, you may additionally contact the Office of Government Information Services (OGIS) at the National Archives and Records Administration to inquire about the FOIA dispute resolution services it offers. The contact information for OGIS is:

Office of Government Information Services
National Archives and Records Administration
8601 Adelphi Road-OGIS
College Park, Maryland 20740-6001
Email: ogis@nara.gov
Telephone: (202) 741-5770
Toll free: (877) 684-6448
Fax: (202) 741-5769

You may obtain a review of this determination under the NLRB Rules and Regulations, 29 C.F.R. § 102.117(c)(2)(v), by filing an administrative appeal with the Division of Legal Counsel (DLC) through the SecureRelease Portal (using the "Create Appeal" button on the "Details of Request" page) or by mail or email at:

Nancy E. Kessler Platt, Chief FOIA Officer
National Labor Relations Board
1015 Half Street, S.E., 4th Floor
Washington, D.C. 20570
Email: DLCFOIAAppeal@nlrb.gov

Any appeal must be postmarked or electronically submitted within 90 calendar days of the date of this letter. Any appeal should contain a complete statement of the reasons upon which it is based.

Please be advised that contacting any Agency official (including the FOIA Specialist, FOIA Officer, or the FOIA Public Liaison) and/or OGIS does not stop the 90-day appeal clock and is not an alternative or substitute for filing an administrative appeal.

Sincerely,

/s/ *Kristine M. Minami*

Kristine M. Minami
Acting FOIA Officer