Establishment-Level Unionization at Large Firms: Evidence from the 21st 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 firms' stock prices - a negative effect on the date when an union election is filed with the NLRB, and a countervailing positive effect when the election is closed. We find no evidence that establishment-level unionization has material affects on stock returns in the short-run horizon regardless of the initial firm-level share of employees unionized, the size of the election taking place, or the sector in which the firm operates. Firms with large elections see a negative effect in the long-run of up to 5% compared to the benchmark, but these results are not robust to different samples or specifications.

Keywords: Unionization, Stock Returns, Personnel Economics

JEL Codes: G10, J51

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1 Introduction

The current decade has seen a marked uptick in union organizing efforts, gaining national attention with high-profile cases such as the Staten Island Amazon warehouse union drive in April 2021. Fueled by pandemic conditions and a tight labor market, union approval reached a 50-year high by October of that year; at the same time, new filings of election petitions with the National Labor Relations Board (NLRB) surged by over 60 percent between 2021 and 2022. This resurgence has coincided with increased attention to labor policy at the federal level, with Congress and the President seeking to bolster the influence of organized labor through support of Federal legislation such as the Protecting the Right to Organize Act.

This new unionization activity occurs in a setting substantially different from previous decades. Union elections are now less common, involve fewer workers, and are concentrated at larger firms compared to when union membership was at its height in the late 1960s and early 1970s.[1] Many of these new elections are small and take place at individual establishments of large, multi-establishment firms. This raises important questions about how multiple, smaller-scale elections affect investor perceptions of future returns at large firm, particularly around the timing of key events in the unionization process like the filing and closure of elections with the NLRB. Understanding how capital markets have reacted to recent union activity, characterized by elections at multi-establishment firms, will offer new insights into the dynamics between organized labor and firm performance in a contemporary setting.

In this paper, we examine the impact of establishment-level union elections on the stock prices of their associated firms. While prior research has concentrated on longer-term impacts from large, highly visible union elections before 2000 (c.f. Ruback and Zimmerman, 1984; Lee and Mas, 2012), we broaden our examination to the impacts of both small and large elections after 1993. This raises several important questions. First, do more recent large elections matter to investors as much as past large ones have? Second, do the increasingly-frequent small elections at large firms have comparable effects on investors' beliefs? One reason for investors to care about elections (regardless of size) is if they suspect that small elections create momentum, or contagion, leading to further elections.[2] Alternatively, an investor might see an initial small election fail and view this as evidence of the robustness of a company to resisting employee's demands for higher wages.

In the following analysis, we show several pieces of evidence that allow us to reject a narrative other than a precise null for the immediate effect of elections — regardless of

[1] See Figures 1, 2, and3.

[2] Indeed, this is precisely what occurred at Starbucks, which has seen over 470 elections since August, 2021.

size — on stock returns. This finding challenges assumptions about the financial relevance of smaller elections and prompts further questions about the long-term strategic value of firms' anti-union efforts. While we find no evidence of small elections having significant *net* effects on returns, we find new evidence on how the timing of the union election process affects how these null effects emerge. First, on the day when an establishment-level election is filed with the NLRB, we observe a small negative effect on the firms' stock price. We view this as reflecting new, heightened investor uncertainty due to unforeseen union activity. Investors may be newly concerned about potential labor disruptions, increased operational costs, or the prospect of ongoing conflicts between labor and management, which drive down stock prices as these risks remain unresolved. However, when an election closes, we find an equal and offsetting positive effect on stock returns. The market responds positively to the election process closing regardless of actual outcome of the election (i.e., whether the union is victorious). The uncertainty initially caused by the election being filed is offset when the election is resolved. Whether the union wins or loses, the closure signals to investors that the period of uncertainty is over, allowing them to reassess the firm's future with greater clarity. Perhaps surprisingly, when the union wins, the positive effect is slightly more pronounced, however this effect is statistically indistinguishable from when the union loses.

To measure the effects of union elections on stock returns, we combine election data from the National Labor Relations Board (NLRB) with equity market data on publicly-listed companies in the Center for Research in Security Prices (CRSP) database. The result is a panel dataset with the number of elections from 1961 through 2023. Our main analysis of contemporaneous effects is restricted to 1994 onward due to data limitations: this is when exact dates for elections are consistently populated. We use a difference-in-differences (DiD) specification to measure the instantaneous impact of elections on the day that they are filed or closed. Critically, we restrict our estimating sample to firms that have had at least one establishment unionize since 1961. Rather than assuming that firms without a union victory are good controls for firms that have union victories (which we view as unlikely), our strategy exploits the quasi random timing of the filing and closing of elections. This provides us with both more-credible identification due to relaxing the burden placed on the parallel trends assumption, and a setting where establishment level treatment is not forced to be a staggered or binary outcome for firms.

Our central finding is the failure to detect economically meaningful effects of union elections on firm-level stock returns across a broad set of treatment definitions and sample selection criteria. In our preferred specification, the filing of an establishment-level election causes the associated firm's stock returns to decline by roughly 7 basis points that day. However, the subsequent election closure offsets this effect entirely; firms experience a roughly

7 percent increase in returns on the day the same election is closed. These effects are small in isolation; 7 basis points is about 1.1 times average daily returns among the firms in our estimating sample. Both effects are precisely estimated (significantly different from zero), but small in magnitude. The cumulative effect of elections – the sum of the effects from both filings and closings – sum to a statistically-precise zero. We can reject a net effect of an establishment level union victory on firm level returns larger than 10.4 basis points in magnitude at the 95% level. For a sense of scale, this rules out net effects larger than two days' worth of average returns in our sample period.

We conduct heterogeneity analysis looking at different types of elections by order (initial or "first" elections versus subsequent elections within a given firm), size, vote share, and length to test whether our null effect is driven by a specific subset of the elections we consider. We find that while the effects of elections remain small, initial elections (i.e., the first election filed at a given firm) are significantly more influential than later elections. There are also slightly larger (although statistically insignificant) effects for elections that are larger, decided by a narrower margin, or longer in length. These findings are in-line with our narrative about union elections and uncertainty. All results listed above are robust to alternative definitions of the treatment we consider (i.e., what indicators or count variables change during when an election is open with the NLRB), the sample period we consider, and what we use to benchmark individual firm-level returns. As alternatives to our sample of only firms with an election victory, we use all firms that ever experience elections, not just winning elections, and all firms in the CRSP. Finally, we implement the test and robust estimators of De Chaisemartin and d'Haultfoeuille (2020) and De Chaisemartin and D'haultfœuille (2023) to ensure our results do not suffer from potential bias in two-way fixed effects estimators induced by heterogeneous treatment effects.

In a second set of analyses, we show that our null result is also robust over longer time horizons as opposed to the outcome of instantaneous returns. Prior work by Lee and Mas (2012) finds that unionization at firms between 1961 and 1999 leads to a 10 percentage point decline in cumulative abnormal returns over the 24-month period after elections are closed with the NLRB. We reevaluate this finding using evidence from the 21st century, and find that this effect no longer occurs. While some of this decline may be driven by a much lower frequency of large establishment level elections, we fail to find evidence for large negative effects even among the large elections that do occur during our sample period. Under our preferred specification, we fail reject the null hypothesis that unionization leads to no significant difference in cumulative returns at any time horizon between 0 and 24 months after an election is closed.

1.1 Existing Literature

Our study contributes to two broad themes in the personnel economics literature. First, we contribute to the existing body of work characterizing the long-run dynamics of organized labor in the United States (Farber and Krueger, 1992; Dinlersoz and Greenwood, 2016; Farber et al., 2021) in harmonizing data on the universe of NLRB elections held between 1961 and 2023. Second, we add to a more-specific strain that focuses on how unionization affects firms, which as a topic alone garnered well over a century of academic work. Our study follows in the modern tradition of studying unions in matching establishment-level election data from the NLRB with firm-level outcomes to identify the causal effects of unionization.

Prior work in economics predominantly focused on how unionization impacts employment, wages, and firm survival. Freeman (1984) and Farber (1986) were some of the first studies to discuss the measurement and causes of union wage premia. Freeman and Medoff (1984) provide a taxonomy of the mechanisms by which the wage gains associated with unionization could be the results of either improved efficiency in labor markets or simply the extraction of rents by unions as monopolistic suppliers of labor. Empirical evidence from early studies on the magnitude of the union wage premium are mixed. Robinson (1989) 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 such as Freeman and Kleiner (1990) and Kuhn (1998) and DiNardo and Lee (2004) find that unionization had small or insignificant effects on establishment-level wages.

More-recent analyses exploiting more granular matched data and newer identification strategies indicate that unionization raises total compensation (as opposed to wages alone) primarily through benefits and pension contributions (Knepper, 2020). At the establishment level, the most recent evidence suggests that unionization can reduce wage bills, employment, and even firm survival (Frandsen, 2021; Wang and Young, 2022). Other evidence from quasi-experimental studies of unionization outside the United States finds increasing union density within firms lead to higher wages and productivity for firms in Norway (Barth et al., 2020). These 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 locations within multi-establishment firms (Wang and Young, 2022). Our findings that unionization has minimal effects on firm-level stock returns complement this body of work examining how establishment-level outcomes are affected. The adjustments firms make in response to unionization, such as reallocating production (Wang and Young, 2022) or altering their workforce composition (Frandsen, 2021), are may serve as channels for firms to offset mitigate any negative effects on overall profitability from direct union activity. The ability for establishment-level responses by firms to union activity may also

serve as evidence to shareholders that the uncertainty surrounding election outcomes is of more concern than the resulting changes in how establishments operate.

A separate stream of research focuses on how unionization affects firm's (as opposed to establishment-level) outcomes, particularly through stock market reactions. Early studies found unionization (Ruback and Zimmerman, 1984) and union-driven adjustments to collective bargaining agreements governing wages (Abowd, 1989) had negative effects on firm stock returns. Our study builds on the work of Lee and Mas (2012) and Kim et al. (2021) and Hofmann and Schoonjans (2023), who used event study specifications to estimate the dynamic impacts of union elections on stock prices. In one of the first studies that compiled a comprehensive panel of matched establishment-level election and firm-level returns, Lee and Mas (2012) found that unionized firms experienced a 10% decline in equity prices in the 24 months following successful elections from 1961 to 1999. Extending this research to the 2011–2019 period, Hofmann and Schoonjans (2023) found smaller, though still negative, stock price effects around the closure of NLRB elections. Similarly, Campello et al. (2018) showed that while unionization does not increase bankruptcy risk, it raises bankruptcy costs, reflecting unions' influence as unsecured corporate creditors. Other scholars have investigated unionization's impact on innovation, showing reductions in R&D expenditure (Bradley et al., 2017), as well as on firm-specific crash risk (Kim et al., 2021). Our contribution diverges from these studies by examining how unionization impacts multi-establishment firms' stock prices. We also provide new 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

It is well-established that private sector unionization rates in the United States are below secular highs seen in the 1940s and 1950s, and have been falling more-rapidly since the 1980s (Dinlersoz and Greenwood, 2016; Farber et al., 2021). This decline in the share of workers who were members of a trade union coincided with a downward trend in the number of workers covered by new unionization efforts generally. The number of union elections fell from a high of over 37,000 over the five year period between 1970-75 to under 7,000 between 2015-20 (Table 1). Within the more narrow scope of activity we consider – establishment-level filings at publicly-listed firms – the number of petitions filed fell from over 4,000 to under 1,000 over the same period.

The makeup of the establishments filing petitions the NLRB and of the publicly-listed firms to which they belong petitions has changed considerably since 1961. The figures below

use the universe of union elections held at publicly-listed firms to document three stylized facts about how the characteristics of establishments attempting to unionize have changed over time: (1), they are becoming smaller (as measured by the size of bargaining units) over time; (2), they occur at larger firms (measured by number of employees); and (3), they comprise a smaller share of firms' total employment. A full tabulation of the values reported below is relegated to Appendix table B.1.

Figure 1 documents the first stylized fact, namely that the size of establishments that attempt to unionize (as proxied by the size of the bargaining unit that would be covered) has declined by a factor of almost two-thirds over the past 50 years. The black line shows the raw data while the blue line is a loess smoothed fit. 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 comprised only 35 people on average.

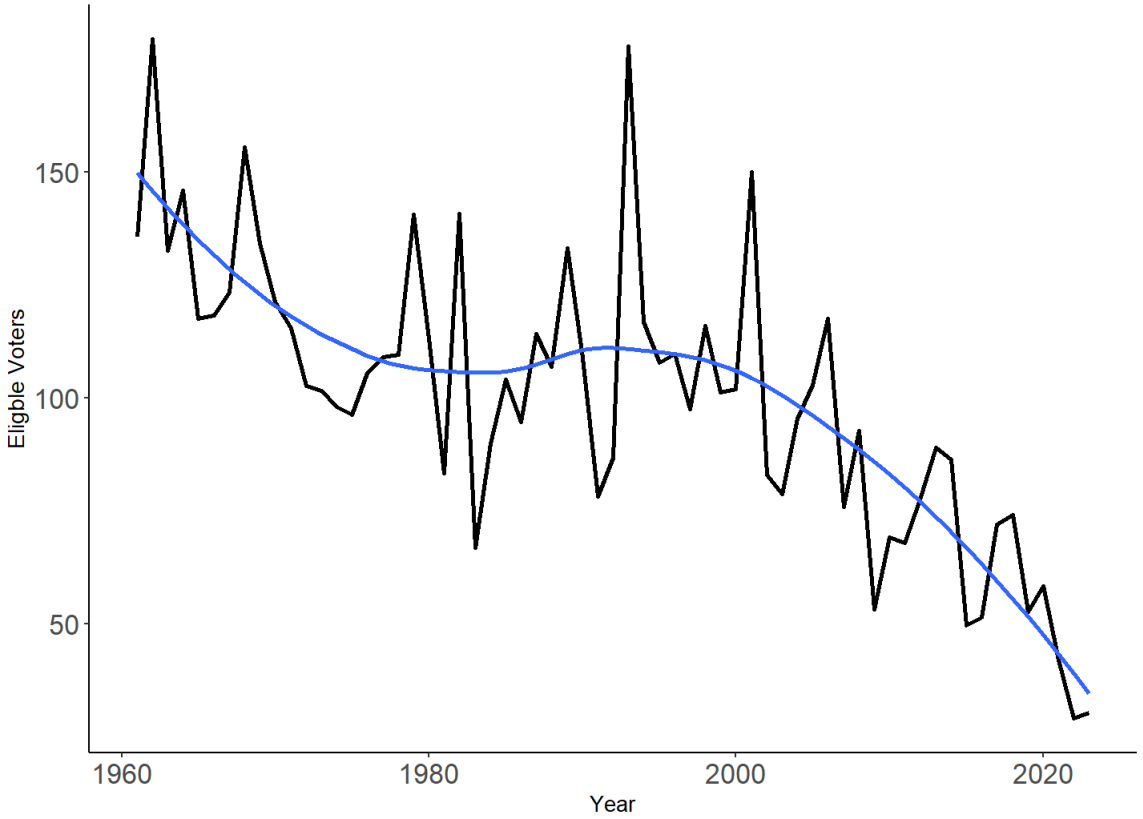


Figure 1: Number eligible voters, matched CRSP-NLRB

Note: Average size of bargaining units among elections filed with the NLRB that we pair with firms in the CRSP dataset between 1961 and 2023. The black line shows raw count data from each year while the blue line shows the fitted values from a LOESS regression.

Figure 2 illustrates how the medium number of employees among firms where establish-

ments file unions has changed since 1960. Unlike with unionization at large or the number of elections, the relationship here is not monotonic and instead follows an U shaped pattern. The average number of firm-wide employees at public firms where unions filed elections shrank between 1960 and 1990 and rose steadily after 2000.

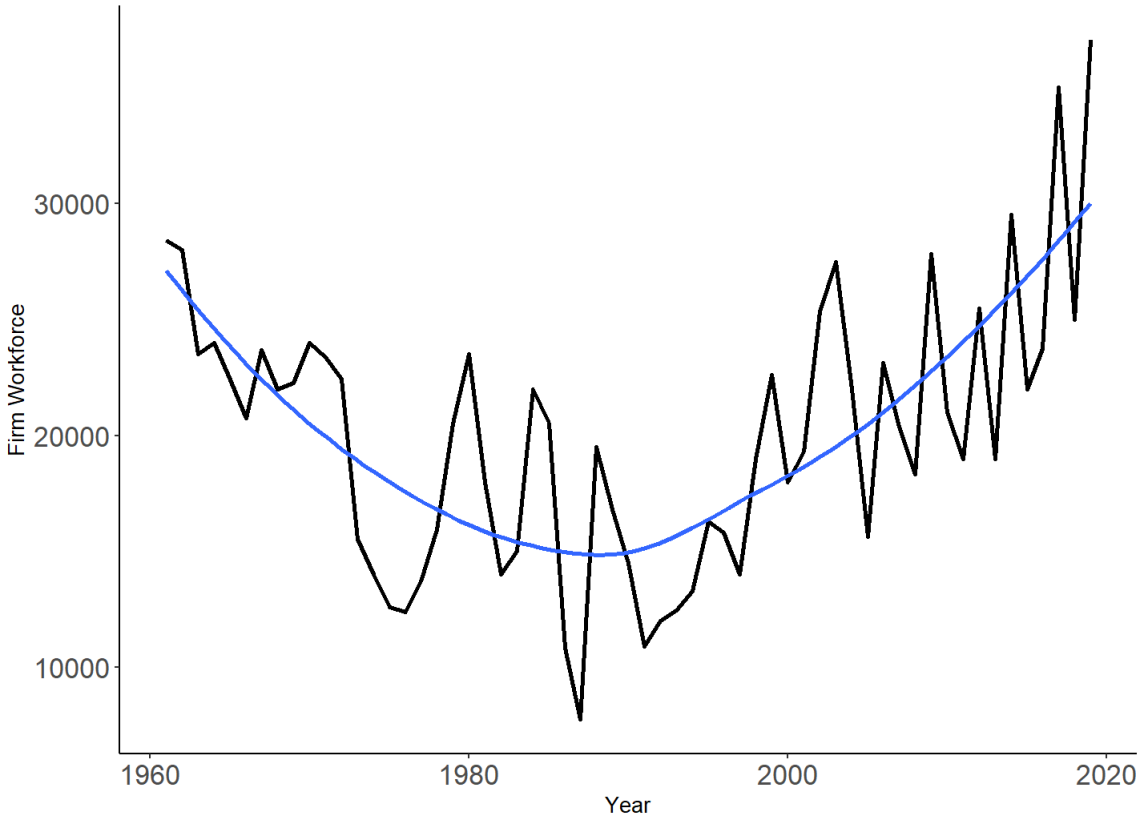


Figure 2: Firm size, matched CRSP-NLRB

Note: Median number of employees among publicly-listed companies that were matched to NLRB election data each year between 1961 and 2023. Data on the number of employees at the firm level are based on the Compustat/CRSP dataset provided by Wharton Research Data Services (WRDS). The black line shows raw count data from each year while the blue line shows the fitted values from a loess regression.

Combined, these trends result a much lower prevalence of "big" elections, which are defined by Lee and Mas (2012) as elections with bargaining units comprising at least 100 eligible voters and 5% of the firms' total workforce. This especially pronounced downward trend in large elections is illustrated in Figure 3.

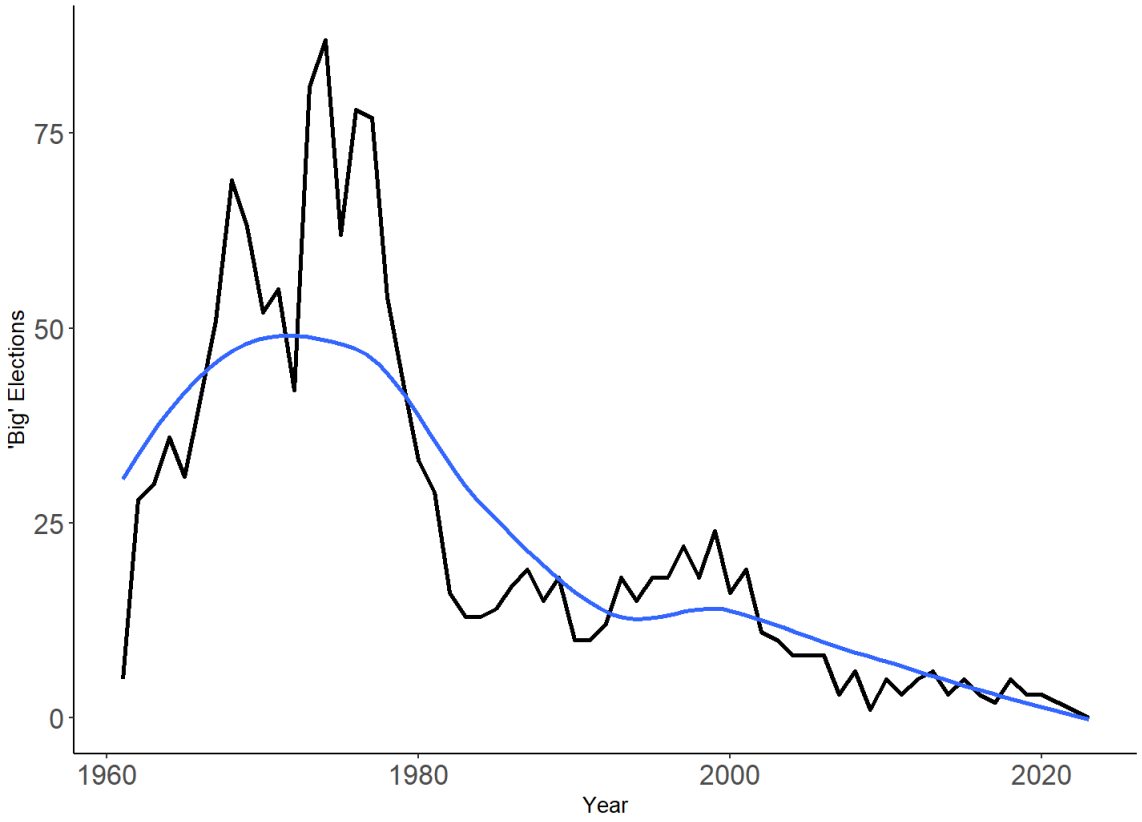


Figure 3: "Big" Elections Filed Annually

Note: "Big" elections are defined as elections with at least 100 eligible voters and at least 5% of the firms' workforce eligible to vote in the election following Lee and Mas (2012). The black line shows raw count data from each year while the blue line shows the fitted values from a LOESS regression.

Counts for number of elections, "big" elections, and election firms, along with averages of number of employees, number of eligible employees, and share of workers in the bargaining unit are listed in Table B.1. We aim to shed light on whether these dynamics have diminished the impact of unionization on firms stock returns.

3 Unions and Firm Data

This section describes the sources for the data we use in this study as well as the procedure used to form our main estimating sample. Our end product is a panel dataset that tracks the number of establishment level union elections filed and closed across publicly-listed firms in the United States. We begin this section with a brief description of the NLRB election process. We then discuss the combination of sources we use to catalogue the universe of establishment-level petitions filed with the NLRB between 1961 and 2024 as well as how

we construct firm-level stock returns for listed firms. Finally, we provide a brief overview of the matching procedure used to match individual establishment-level elections with listed firms.

3.1 Unions and the Election Process

Unions are entities that act on behalf of employees in order to reach or expand preferable terms of employment. Union representation benefits workers because it promotes coordination and cohesion; by acting on behalf of all represented employees collectively, unions increase employee bargaining power. Unions' primary roles include negotiating with employers (often over higher wages or better working conditions), and, in the case that negotiations fail, organizing strikes. Private sector unions are governed by the National Labor Relations Board (NLRB). The NLRB is an independent federal agency created in 1935 under the National Labor Relations Act (also known as the Wagner Act, amended in 1947 and 1956). It is the predominant institution for safeguarding workers' rights. Nearly all official revenues for employee empowerment and retribution run through the NLRB. Along with union elections and collective bargaining agreements, it handles petitions for unfair labor practices and petitions for union representation. The NLRB has jurisdiction only over private sector employees.

The NLRB election process is illustrated in Figure 4. The NLRB attempts to make the election process timely and fair by requiring that employers notify employees of the election and allow them to vote freely. The two events that we focus on throughout our analysis are the date the petition is filed with the NLRB ("filing" date) and the date that the NLRB certifies the results ("closing" date).[3] The filing date is the date that the NLRB receives a petition with signatures from at least 30% of the bargaining unit as defined by the employee that submits the petition. Upon, receiving the petition, the NLRB determines if the bargaining unit as defined by the petitioner is legitimate DiNardo and Lee (2004). Upon approval of the petition, the NLRB notifies the petitioner and the employer, and an election takes place. The NLRB works with both parties to determine the details of the election, setting the date, time, and place for balloting, the ballot language(s), the appropriate unit, and a method to determine who is eligible to vote. Furthermore, the NLRB publicly releases the case information on their website. We view the filing date as the date when the election is announced. The closing date is when the results are certified and the outcome is announced. In between these two dates, the election is conducted and any disputes or complaints are

[3]Throughout the paper, we refer to firms 'filing a win' or 'filing a loss.' Naturally, the election outcome is unknown at the time of filing. By 'filed a win,' we mean the firm filed for an election that ultimately resulted in a win.

adjudicated by the NLRB.

There is an average period of 6 weeks between filing a petition and tallying the votes, and 2 weeks between tallying the votes and closing the election. A union receiving a simple majority of the votes among a bargaining unit is certified as the bargaining representative and is entitled to be recognized by the employer as the exclusive bargaining agent for the employees in the unit. Failure to bargain with the union at this point is an unfair labor practice.[4] There are two ways that an election may be questioned by the parties involved. The first is during the actual election process prior to the tally, and the second is by challenging individual ballots during the casting of the votes.

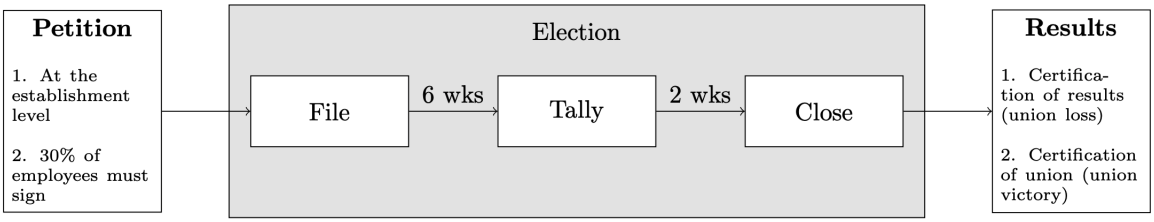


Figure 4: NLRB Union Representation Election Process

Critically, the certification of a union representative (the result of a successful election) does not ensure the new union will reach a collective bargaining agreement (CBA) with the employer (Naidu, 2022) - in about half of all successful elections an agreement is never reached. If a union and firm do form an agreement within a year of election closure the union may be decertified via a subsequent petition and election resulting in a loss.[5] As such, going forward our (and other measures) in the literature measures the effects of election victories, which does not per se capture the effects of any collective bargaining outcome.

3.2 Data Sources

The NLRB data is created from two sources. Data for elections after 2000 was obtained through a Freedom of Information Act (FOIA) request filed with the NLRB. For elections prior to 2000, we rely on data created by J.P. Ferguson and Thomas Holmes.[6] While the data covers elections occurring between 1961 through 2023, the exact dates (rather than year and month only) are only reliably populated beginning in 1994. Furthermore, the CRSP subsidiary data only begins in 1994. For these reasons, our primary analysis focuses

[4] https://www.nlrb.gov/about-nlrb/what-we-do/conduct-elections

[5] Although this may seem like an obvious goal for firms once a union is formed, unions are rarely decertified (Campello et al., 2018).

[6] Ferguson, 2016 and Holmes, 2024 respectively.

on the period between 1994 and 2023. However, we use the entirety of the union data for attributing total election counts to firms and for some of the long-run analysis.

We use stock data from the Center for Research in Security Prices (CRSP) and firm characteristics from the CRSP/Compustat (CCM) merged dataset. These data contain stock data for companies listed on all major US stock returns. We augment these data with the 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 constructing our panel of firms' establishment-level union elections and stock returns. A full description is relegated to Appendix A. The task is to match NLRB election data, which contain firm name, to CRSP stock data, which contain firm name 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, our task is further complicated by name changes (e.g. "Facebook" becoming "Meta"), mergers and acquisitions, delistings, and subsidiary companies. Thus, while names are 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 clean all company names by removing capitalization and punctuation. We also remove common words – those often found in firms' official titles that do not materially affect their name. These words are: "company", "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 remove all spaces from the company name. Doing this reduced the number of unique NLRB company names from 173,458 to 147,399.

After doing an exact match on clean company name, we take the remaining names and use a fuzzy merge. To do so, we used the R package Fuzzyjoin. 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 companies. Based on this exercise, we chose a threshold of 0.06 (the maximum, or most lenient value is 1 and 0 only selects exact matches). We only kept

matches that were 1-to-1, that is, names that matched to exactly one other name in either dataset. We were left with 5,042 fuzzy matches.

Merging: To start, we create a complete list of firm name and PERMCO combinations in the CRSP. To do so, we use three datasets in the CRSP. We use the "Subsidiary Data" and "CRSP/Compustat merged" datasets to get PERMCOs for all subsidiary companies. In the CRSP and subsidiary datasets there are 375,137 unique clean company names. The subsidiary data lists parent companies and their subsidiaries for companies 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. Overall, there are about 27,000 PERMCO across all the names; There is a median of 8 subsidiaries per parent company (average of 36). If we match any of the subsidiary names to an election company, we associate the election with the parent company. Subsidiaries often change parents and may have more than one parent. With this in mind, our matching procedure follows three steps

1. **CRSP Panel:** Create a panel of every date between first and last dates for PERMCO/name combinations. With the subsidiary data, we take the first time a company is listed as a subsidiary on an SEC filing up until a filing that does not list the company as a subsidiary – this assumes that the company was a subsidiary from the moment it is first listed as one until the date it is no longer listed. For every day that a company has a PERMCO, either directly or from a parent, there is an observation. Likewise, for companies in the Stock data, fill in all dates between 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:** First, we fill in all dates between first election filed and last election closed for each company name. Next, we create cumulative sums of elections and wins filed and closed and total counts for each company 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 pertaining to the name upon entering the CRSP. We make do not assign PERMCOs to companies outside of the periods when they are explicitly tied to a PERMCO in the CRSP. One we have the crosswalk, it is simple to merge in election and stock data.

Table 1 lists the number of names and elections matched for 5-year periods. The percentage of names and elections matched seems relatively stable over time, with the match rate being between 5% and 15%. The increased match percentage post-2000 is due to the introduction of the subsidiary dataset. Of course, even if we were able to match perfectly, the match rate would not be 100% because not all elections occurred at publicly traded firms. These matches are for elections which occur while the establishment is wholly owned or a subsidiary of a firm that is publicly-traded at that time. If we focus on name alone the match rates almost double (see Appendix A).

Table 1: Matched elections between 1961 and 2023

Years	Distinct names	Elections	Matched elections	Names matched (%)	Elections matched (%)
1961-1965	17,133	21,220	1,981	3%	9%
1965-1970	25,014	34,662	4,717	4%	14%
1970-1975	27,143	37,846	5,485	5%	14%
1975-1980	26,834	35,666	4,089	5%	11%
1980-1985	18,329	22,663	1,746	4%	8%
1985-1990	14,238	16,848	849	3%	5%
1990-1995	12,295	15,118	745	3%	5%
1995-2000	11,704	15,110	1,855	8%	12%
2000-2005	9,584	12,428	1,697	9%	14%
2005-2010	5,912	8,131	991	9%	12%
2010-2015	4,968	7,016	945	11%	13%
2015-2020	4,270	6,623	1,131	13%	17%
2020-2023	3,155	5,012	687	5%	14%

Note: Number of firm names and elections in the NLRB data along with percents matched by 5-year period.

3.4 Summary statistics

From 1961 through 2023, roughly 238,000 elections for union representation were held across 147,000 firms – 25,622 of these elections were held while the firm was publicly listed. Unions won just over half of the elections (55%) , with an average vote share of 57%. Elections were generally small, with the average size of a bargaining being 67. The NLRB does not have data on firm size. However, for those elections that we matched and had records of

number of employees, roughly 4% of firms' employees were included in the bargaining unit.

Table 2 reports summary statistics for elections in the NLRB data. The first two columns of Table 2 separate elections into those that took place while a company is listed on a major US stock exchange ("public"), i.e. elections we matched to the CRSP, and those that were not ("private"). The overwhelming majority of elections, 212,629, take place at private companies compared to 25,621 at public companies. Elections at public companies are substantially larger, which is to be expected given that public companies are typically larger than private ones. The average number of employees in a bargaining unit at public companies is 109 compared to 62 at private companies. The average percent of workers voting in favor of the union and election lengths are the same in both groups at 57% and 92 days. Columns 4 and 5 separate elections at public companies into winning and losing elections. We match nearly 14,000 wins and 12,000 losses, across 2,500 and 2,600 firms. Note that firms often experience 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 this is no coincidence, long elections are often due to employer complaints and may even be a symptom of unfair labor practices. While losing elections have larger bargaining units on average, they occur at firms with fewer employees. Losing elections happened at firms with roughly 52,000 employees whereas winning election firms had about 62,000 employees, on average.

Table 2: Summary statistics - NLRB data

Average	Non-matched elections	Matched elections	Matched elections: Wins	Matched elections: Losses
Avg. % vote for union	57%	58%	80%	32%
	[28]	[28]	[17]	[14]
Avg. num. eligible	62	109	84	137
	[28]	[28]	[17]	[14]
Avg. pct. eligibile	NA	4%	4%	4%
		[128]	[166]	[61]
Avg. election length	92	92	81	105
	[141]	[156]	[125]	[185]
Avg. year	1982	1982	1982	1983
	[15]	[17]	[18]	[15]
Avg. firm size	NA	56,562	60,475	52,133
		[103437]	[103852]	[102791]
Total number	211,425	26,918	14,449	12,469

Note: Average values and standard deviations in brackets. Columns 2 and 3 are computed for elections not matched to the CRSP non-matched) and elections matched to the CRSP (matched). Columns 4 and 5 split matched elections by outcome.

Table **??** reports the average number of employees, return, and benchmark return along with standard deviations in brackets. Election firms have far more employees than non-election firms; firms with wins are about three times as large as firms without wins. The average return is similar across all firms, ranging from 0.010 for non-election firms to 0.014 for election firms with no wins. 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 experience acquisitions and liquidations.

	Firms with		Election firms with	
Average	No elections	With elections	No wins	Wins
Num. Employees	2,645	19,693	12,389	32,892
	[17,653]	[66,809]	[32,834]	[101,536]
Return	8.34	6.68	7.02	6.06
	[570.28]	[362.23]	[383.04]	[320.61]
Benchmark ret.	4.07	4.74	4.8	4.64
	[126.33]	[122.86]	[122.22]	[124.03]
Delisted	0.28	0.12	0.1147	0.13573
Merged	0.45	0.21	0.19766	0.26647
Acquired	0	0.01	0.00863	0.01996
Liquidated	0.02	0	0.00056	0.002
Firms	12,055	4,594	3,592	1,002
Observations	21,374,208	9,771,561	6,322,258	3,449,303

Note:
Average values and standard deviations in brackets. Columns 2 and 3 are computed for CRSP firms that are not matched to the CRSP ('No elections') and elections matched to the CRSP ('With elections'). Columns 4 and 5 split matched elections by outcome.

4 Identification

Our strategy for recovering the causal effects of successful election filings and closures on stock returns relies on the quasi-random timing of election petition filings among firms which eventually have at least one establishment-level election victory. This differs from previous approaches which have often used regression discontinuity (RD) designs (c.f. DiNardo and Lee, 2004; Kim et al., 2021; Campello et al., 2018; Sojourner et al., 2015), which rely on the assumption that close unionization elections outcomes are as good as randomly determined. Threats to this identification strategy for the setting of union elections at this point are well-documented; Knepper (2020) and Frandsen (2021) show substantial evidence for nonrandom selection ("manipulation") around the 50 percent vote share cutoff which can lead to biased estimates from RD designs (McCrary, 2008). This is distinct from the issues surrounding measurement bias that may be induced due to workers' selection into unions (c.f. Freeman, 1984; Card, 1996).[7] Frandsen (2021) suggests instead using a "difference-in-discontinuities" approach which requires assuming that the traits on which selection into unionization occur are time invariant. However, when multiple establishments of a given firm unionize in a short

[7]See Appendix C.1 for formal evidence that the identifying assumptions required for an RD desing are unlikely to hold in our setting .

period, it is difficult to employ either form of the RD approach as there is not a definitive binary treatment at the firm level to assign to either side of the would-be running variable.

The literature examining the effects of unionization on equity prices has previously employed the two papers most similar to ours use event study design employed by Ruback and Zimmerman (1984) and later by Lee and Mas (2012). The authors use event studies to examine how the cumulative abnormal returns of firms change before and after union victories take place. This compares the performance of a given firm relative to its "expected level", proxied as a basket of similar firms within the industry (Lee and Mas, 2012). However, the event study approach may suffer from identification issues due to non-random selection into unionization (Dinlersoz et al., 2017) as well as attenuation effects when treated firms go out of business entirely (Frandsen, 2021; Wang and Young, 2022).

Our approach is modified given the threats to both identification and external validity stemming from selection into unionization. We adopt a difference-in-differences framework and restrict our main estimating sample to firms that experience at least one establishment-level union victory. Our strategy is closest to that of Wang and Young (2022), who use a modified version of the "difference-in-discontinuities" design that allows for plausible inference of the effects of unionization for firms not exactly at (or around) the 50% vote share cutoff. Their sample is instead the set of establishments where elections take place, which under a parallel trends assumption uses establishments where elections fail as controls to compare against those establishments where they succeed.

4.1 Identifying Assumptions and Notation

Let $i \in \{1, \ldots, I\}$ and $t \in \{1, \ldots, T\}$ index our panel of daily stock prices across the I firms in the CRSP for which we observe at least one successful establishment-level unionization election in the trading days T covering 1994 through 2023. Let $\{v_{j,f,c}\}$ denote the set of all establishment-level union election petitions that result in a certification of a representative (union victory). Each victory $v_{j,f,c}$ is associated with given firm j as well as two points in time as described in section 3.1: a filing date f and a closing date c. To study the effects of establishment-level union victories, we define two treatments. Let

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

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

be the daily counts for the number successful election petitions filed or closed at establish-

ments of firm i on trading day t. W_{it}^{f} and W_{it}^{c} denote the total number of establishment-level elections resulting in victory that were respectively filed and closed at firm i on trading day t. Finally, let $\mathbf{W}_{it}$ be a vector stacking establishment-level election closings and filings at firm i in trading day t and let $\mathcal{W}$ be the set of values $\mathbf{W}_{it}$ can take.

We adopt a potential outcome framework and define the potential return $R_{it}(\mathbf{w})$ as the change in the stock price of firm i and 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, and 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'haultfœuille (2023) make the following assumptions in Section 5:

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

$$\mathbb{E}\Big[R_{it}(\mathbf{0}) - R_{it-1}(\mathbf{0}) \mid \mathbf{W}_{i,1}, \dots \mathbf{W}_{it},\Big] = \mathbb{E}\Big[R_{it}(\mathbf{0}) - R_{it-1}(\mathbf{0})\Big] \tag{3}$$

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 firms' returns absent treatment. This require 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 unrelated to daily stock price variation that would occur absent any elections. We believe that quasi-random timing of filing dates is plausible as it's unlikely that union organizers choose specific dates on which to file petitions based on day-to-day fluctuations in company stock returns. Randomness in closure dates is likely as there is no reason that the NLRB chooses the time it takes to tally votes, resolve any challenges, and formally certify elections based on the stock returns of the company whose election is closed.

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

$$\mathbb{E}\Big[R_{it}(\mathbf{0}) - R_{it-1}(\mathbf{0})\Big] \tag{4}$$

does not vary across firms.

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 $t > 1$. Absent treatment, the

expected evolution of untreated returns between periods should be the same across all (it) pairs. Assumption 2 is a standard parallel trend assumption on firms' untreated outcomes evolving together over time. This assumption is testable in that one can check whether it holds in the data we observe multiple firms go untreated over two periods. We report the results of these falsification tests along with our event study estimates in Section 5.2.

4.2 Sample Restriction

To lend credibility to Assumptions 1 and 2, in our setting, we restrict the estimating sample to firms where at least one establishment has unionized since 1961. Restricting our sample to the 3,553 firms that experience establishment-level victories avoids using firms which never unionize as a control group. The controls in our setting are instead the firms that do not experience a successful filing or closure that particular trading day. Narrowing the estimating sample provides additional credibility to the parallel trends assumption, as it allows the time fixed effects to capture aggregate shocks that affect only firms which have characteristics that lead to selection into unionization.

Suppose, for example, the United Auto Workers (UAW) holds a nationally rally of automotive workers demanding a doubling of wages at unionized plants in period t. Consider three firms: Ford, Mercedes, and Tesla. Suppose Mercedes and Tesla have election votes in period t. Suppose the Mercedes establishment in question is the one covering Mercedes' American plants in the cities of Vance and Woodstock, both in the U.S. state of Alabama. The state of Alabama is notably hostile to unions; it recently passed legislation withdrawing all state and local support for firms that voluntarily recognize unions. Meanwhile, suppose the Tesla factory in question is in California, a state more historically friendly to labor organizing. Parallel trends requires that change in returns in each firm absent treatment would be the same across these two period

$$\mathbb{E}\Big[R_{it}(0) - R_{it-1}(0)\Big]$$

is equal across firms. For Ford and Tesla this may be plausible – Tesla operates in California, a state with both institutions and a workforce that hold more-favorable views of unions. There's no reason to expect this announcement to affect Mercedes in the same way – they're insulated by being located in the South. This event may have negative effects on the stock returns for Ford and Tesla (as they are exposed to the UAW's demands) but no effects on the third firm despite an election taking place there. Our specification avoids using this third firm as a control, helping ensure we estimate the average treatment effect on the treated (ATT) in our setting. Focusing on this smaller set of firms also avoids attenuation bias, as the majority

of the firms we focus on are present through the end of the sample period. Appendix E shows that our main results are robust to using a less-restrictive criteria for building our sample which includes either all firms where unions have filed petitions (regardless of outcomes) or the entirety of firms listed in the CRSP.

5 Results

In this section, we examine how daily stock prices are affected on the days that establishment-level elections are filed and closed. We estimate variations of the following model:

$$AR_{it} = \beta_f W_{it}^f + \beta_c W_{it}^c + \alpha_i' X_t + \gamma_t + \delta_i + \varepsilon_{it} \tag{5}$$

where W_{it}^f and W_{it}^c are count variables for the number of establishment-level elections filed and closed for firm i on day t described in Section 4. The outcome variables, AR_{it}, are abnormal returns for firm i on day t defined as the difference between individual firms' stock returns R_{it} and the return on a matched benchmark, $Ret_{it}^{Benchmark}$:

$$AR_{it} = R_{it} - Ret_{it}^{Benchmark}$$

Following Lee and Mas (2012) we use a value-weighted portfolio of firms in the same size decile as a benchmark. Alternatively, we use firm-specific Fama-French (Fama and French, 1993) factor loadings as a benchmark and arrive at similar results. Our estimating sample consists of matched firms that have had an election victory since 1961; There are 3,553 such firms. Of these firms, 2,551 have not had any elections since 1994 — these firms are our "never treated" group. In the appendix, we conduct the same analysis but use all firms, not just those that have had a successful election. The results are in Tables F.1 and F.2.

Table 3 displays the results from estimating equation (5) on our estimating sample of winning firms. Our preferred specification that includes two-way fixed effects but does not allow for firm-level factor loadings is in column 2. 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 points) on firms' stock returns the day an election is filed. These effects are counteracted by a positive effect of 6.9 basis points the day an election is closed. These effects are small: 7 basis points is about 1.1 times average daily returns among the firms in our estimating sample. Both effect sizes are less than five percent of a standard deviation of firm-level returns returns.

In the bottom row of Table 3, we show p-values for the test statistics associated with

the null hypothesis that the coefficients on filings and closures sum to zero (the statistical test for the null hypothesis that elections have no net effects). We fail to reject a test of both coefficients adding to zero in all three specifications; at the daily frequency, the net impact of establishment level unionization efforts on contemporaneous returns is effectively zero. The 95% confidence intervals for the bound on net effects corresponds to -1.6 days' worth of average returns or 3% of the standard deviation of returns.

Table 3: The Effect of Establishment-Level Union Victories on Stock Returns

Dep. Var: AR_{it}	(1)	(2)	(3)
Wins Filed	-7.654**	-7.436*	-6.734*
	(3.711)	(3.832)	(3.732)
Wins Closed	7.115**	6.908**	6.382*
	(3.342)	(3.403)	(3.404)
Time Fixed Effects	No	Yes	Yes
Firm Fixed Effects	No	Yes	Yes
Firm-Level FF3	No	No	Yes
N	8,042,828	8,042,704	8,003,588
$p(H_0 : \beta_c + \beta_f = 0)$	0.91	0.92	0.95

Note: All estimates are reported in basis points (one-hundredth of a percentage point or 0.0001). Estimates from β_f and β_c in Equation 5. Parentheses show multiway 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.

We next consider how the effects of filing and closing winning elections may differ from the effects of filings and closures regardless of outcome. We re-estimate a version equation of (5) where the variables file$_{it}$ and close$_i t$ are count variables of either filed (closed) wins, losses, or elections regardless of outcome:

$$AR_{it} = \beta_f \text{ file}_{it} + \beta_c \text{ close}_{it} + \alpha_i' X_t + \gamma_t + \delta_i + \varepsilon_{it} \tag{6}$$

Table 4 reports estimated values of the β_f and β_c coefficients. For readability, explanatory variables have been scaled by 1/10000 or 0.01% so that integers represent basis points. The first column examines the general effect of establishment-level union filings regardless of

outcome. The count variables here comprise all filings (closures), both wins and losses. The second and third column separate elections by outcome, with column 2 reporting wins and column 3 reporting losses.

Table 4: The Effect of Establishment-Level Union Elections on Stock Returns by Election Outcome

Dep. Var: AR_{it}	(1)	(2)	(3)
Filed	5.626		
	(3.011)		
Closed	5.813*		
	(2.793)		
Wins Filed		6.163	
		(3.707)	
Wins Closed		8.793*	
		(4.023)	
Loss Filed			4.884
			(4.673)
Loss Closed			1.920
			(4.168)
Fixed-Effects:			
Date	Yes	Yes	Yes
Firm	Yes	Yes	Yes
Observations	8,042,535	8,042,535	8,042,535
R2	0.00762	0.00762	0.00762
$p(H_0 : \beta_f + \beta_c = 0)$	0.96	0.62	0.64

Note: All estimates are reported in basis points (one-hundredth of a percentage point or 0.0001). Multi-way standard errors are clustered at the firm and trading day level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Interestingly, the effects in table 4 appear more pronounced for winning elections than losing elections. Effects are statistically significant for closing all elections and closing a winning election – estimates for losing elections are relatively smaller and statistically insignificant.

5.1 Joint significance

The results in Table 4 either pool winning and losing elections or examine them in isolation. To formally test whether the effects of filing and closing a winning election differ from filing and closing a losing election, we regress abnormal returns on filings and closures on four count variables that allow for differentiated effects of winning versus losing elections:

$$AR_{it} = \beta_{wf}\, W^f_{it} + \beta_{lf}\, L^f_{it} + \beta_{wc}\, W^c_{it} + \beta_{lc}\, L^c_{it} + \gamma_t + \delta_i + \varepsilon_{it} \tag{7}$$

where the values L^f_{it} and L^c_{it} represent count variables for losses filed and closed analogous to our definition of wins. This in effect modifies equation (5) such that there are two sets of βs, one for winning elections and one for losing elections. Estimates and standard errors from this modified regression specification are shown in Table 5.

Table 5: Elections filed and closed, wins and losses

Dep Var: AR_{it}	(1)
Wins Filed	-6.168
	(3.709)
Loss Filed	-4.877
	(4.673)
Wins Closed	8.789*
	(4.022)
Loss Closed	1.927
	(4.168)
Fixed-Effects:	
Date	Yes
Firm	Yes
Observations	8,042,535
R2	0.00762

Note: Estimates from β_{wf}, β_{lf}, β_{wc}, and β_{lc} in Equation 7. All estimates are reported in basis points or 0.01%. Multi-way standard errors are clustered at the firm and trading day level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

Even when jointly estimated, we find similar effects of both filings and closures on stock

returns regardless of outcome. While the individual coefficients are not precisely estimated, the point estimates for filings are negative for both wins and losses, while the coefficients on closures are both positive. Table 6 provides p-values from statistical tests of the joint significance for various combinations of the estimated coefficients from Equation (7). From rows (i) and (ii) we see that we cannot reject equality of filing *or* closing a winning versus a losing election (p-values of 0.82 and 0.26 respectively). Row (iii) tests whether the effects of filing *and* closing a winning election differ between winning and losing elections (p-value of 0.50). Row (iv) tests combined significance of all four coefficients with a p-value of 0.97). Taken together, these tests indicate that wins and losses are not statistically distinguishable from each other and the magnitudes of their coefficients, considered separately or together, are not statistically different from zero.

Table 6: Wald statistics from tests of joint significance

Test	p-value
(i) wins filed - losses filed = 0	0.823
(ii) wins closed - losses closed = 0	0.260
(iii) wins filed - wins closed = 0	0.008
(iv) losses filed - losses closed = 0	0.265
(v) wins filed + wins closed = losses filed + losses closed	0.506
(vi) wins filed + losses filed + wins closed + losses closed = 0	0.968

Note: Multi-way standard errors are clustered at the firm and trading day level.

5.2 Dynamic effects

To test for any anticipatory or lagged effects of union elections, we estimate a dynamic difference-in-differences equation. Specifically, we estimate the following regression equation:

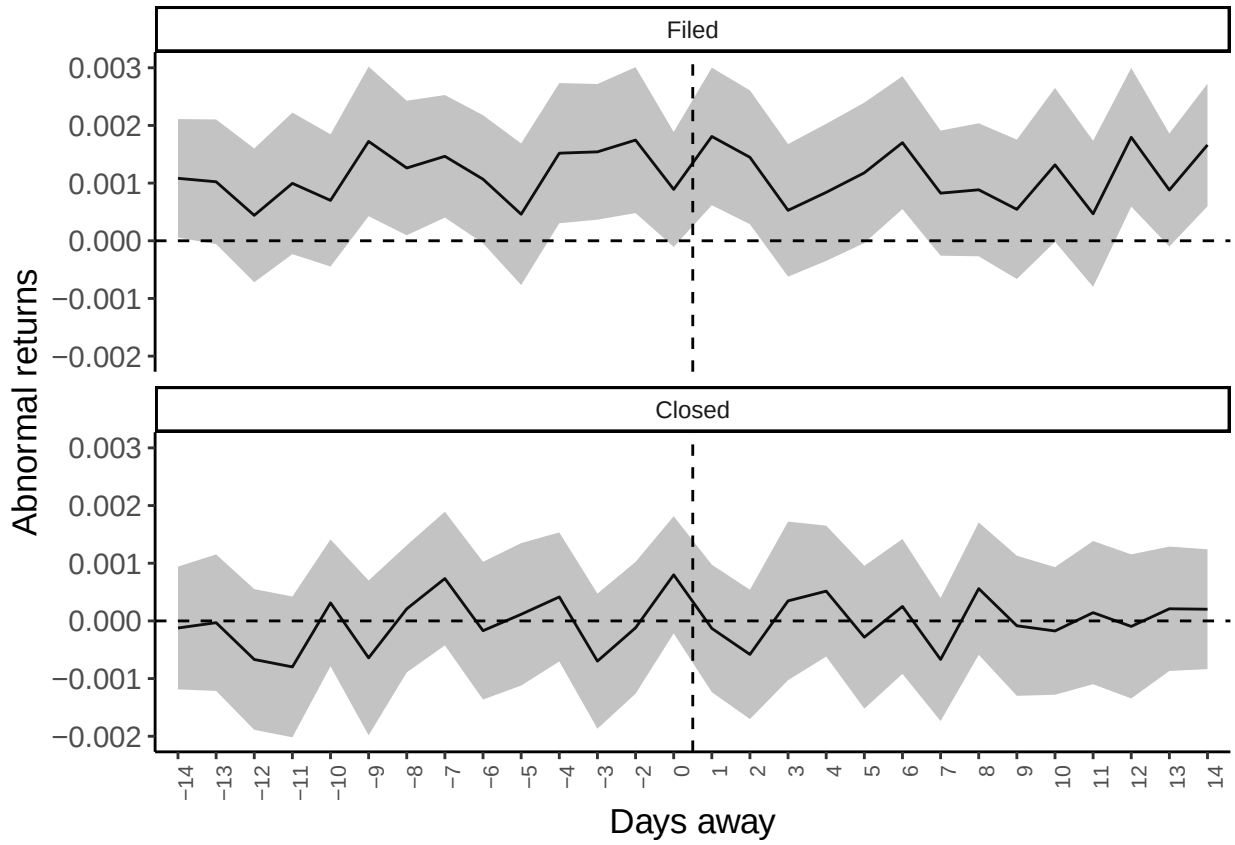
$$AR_{it} = \sum_{T_0:k\neq -1}^{T_1} \beta_k \times \mathbb{I}\{t - \text{event}_{it} = k\} + \phi_i + \gamma_t + \varepsilon_{it} \tag{8}$$

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. The variable event_{it} marks the calendar time t on which the election is filed or closed at firm i. Values of $k < 0$, are "pre-event" coefficients representing the periods prior to the election event. These capture any anticipatory effects. Likewise, "post-event" coefficients are those with $k > 0$. These capture any delayed effects that may materialize in the days following an event. The terms ϕ_i and γ_t are firm and calendar day

fixed effects. The coefficient for the period just before elections, $k = -1$, is left out of the regression, so that all β_k coefficients are interpreted as average differences (between treated and control firms) relative to the period before the election event.

Estimating equation 8 can help assess the validity of Assumption 2: insignificant coefficients on the pre-event terms indicate that in the data we observe, we cannot reject a null hypothesis of no "anticipatory" effects or pre-treatment differences between the evolution of untreated returns at control and treated firms. This is not a direct test of Assumption 2, which concerns a counterfactual scenario unobservable to the econometrician; it does however lend Assumption 2 a degree of plausibility. Figure 5, plots the point estimates and confidence intervals for the β_ks for all elections. (We consider wins and losses separately in the appendix). The top and bottom panels of the figure show 14 days before and after filing and closing an election. There do no appear to be any trends before or after each event. Motivated by the work of Rambachan and Roth (2023), we also perform a more-restrictive joint test of significance rather than relying solely on the the 95% confidence bands show in the figure. The p-value for joint significance of the pre-filing and pre-closing coefficients are 0.11 and 0.40, respectively. Similarly, the p-values for post-filing and post-closing are 0.05 and 0.46.

Figure 5: Dynamic Difference-in-Difference, 14 days before and after filing (closing) an election



Note: Point estimates and 95% confidence intervals on the β_k coefficients from Equation 8. Multi-way standard errors are clustered at the firm and trading day level.

5.3 Heterogeneity

The effects of establishment-level union efforts may also differ based on characteristics of the elections. To check for heterogeneity across types of elections we create dummy variables for elections that are have more than 250 people in the bargaining unit ("big"), are the first election at a firm ("first"), span longer than 90 days between filing and closing ("long"), or where the union gets between 45% and 55% of the vote share ("close"). Lee and Mas (2012) find that large elections lead to larger declines in profitability compared to smaller ones. Wang and Young, 2022 find that that the negative effects of unionization as they relate to establishment survival are greater when managers or owners are more opposed to the union. First elections carry more employer opposition as they attempt to

dissuade unionization efforts from spreading to other establishments (Wang and Young, 2022). Likewise, more-contentious and longer elections are often the result of employer meddling in the election process (Knepper, 2020; Frandsen, 2021; Wang and Young, 2022). Thus, these elections are likely to be especially salient in investor risk calculations, even if they may not directly translate into productivity or wage distortions (Wang and Young, 2022).

Table 7: Elections filed and closed dummies

Dep Var: AR_{it}	(1)	(2)	(3)	(4)
Filed Dummy	-4.632	-6.022*	-5.131	-4.800
	(3.589)	(3.436)	(3.608)	(3.738)
Closed Dummy	4.247	2.366	3.266	2.745
	(3.063)	(2.856)	(3.229)	(3.132)
$\mathbb{I}\{\text{Big}\}\times$ Filed Dummy	-11.06			
	(12.54)			
$\mathbb{I}\{\text{Big}\}\times$ Closed Dummy	7.013			
	(11.35)			
$\mathbb{I}\{\text{First}\}\times$ Filed Dummy		15.58		
		(18.56)		
$\mathbb{I}\{\text{First}\}\times$ Closed Dummy		45.27**		
		(20.87)		
$\mathbb{I}\{\text{Long}\}\times$ Filed Dummy			-0.9793	
			(8.785)	
$\mathbb{I}\{\text{Long}\}\times$ Closed Dummy			8.868	
			(8.493)	
$\mathbb{I}\{\text{Close}\}\times$ Filed Dummy				-3.685
				(10.32)
$\mathbb{I}\{\text{Close}\}\times$ Closed Dummy				16.08
				(9.997)
Date (7,552)	Yes	Yes	Yes	Yes
Firm (3,162)	Yes	Yes	Yes	Yes
Observations	8,042,828	8,042,828	8,042,828	8,042,828
R^2	0.00762	0.00762	0.00762	0.00762

Note: Multi-way standard errors are clustered at the firm and trading day level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

5.4 Long-run effects

A final shortcoming of the daily analysis above is it may overlook effects of unionization on stock returns that accumulate slowly over a longer time horizon. Previous findings using longer-run event studies find that the negative effects of unionization on firm-level stock return emerge over monthly or annual rather than daily time scales (Ruback and Zimmerman, 1984; Lee and Mas, 2012). Others find delayed effects of union elections on establishment-level outcomes such as total wage bills, employment, and survival rate (Knepper, 2020; Frandsen, 2021; Wang and Young, 2022).

To test for long-run effects, we examine how *cumulative* returns differ between firms that close a winning election and returns for paired benchmark indices. We aggregate firm-level returns to the monthly level and examine a window comprising the two years before and after each election is closed vis-à-vis benchmark cumulative returns over the same time period. For the relative months around an election at firm i that occurs at calendar time τ, we compute the cumulative return around this election from 24 months prior to time τ through relative period T:

$$CR_{iT}(-24, T) = \sum_{t=-24}^{T} Ret_{i,\tau+t} \tag{9}$$

The outcome $CR_{it}(-24, T)$ measures the cumulative return of firm i from two years prior to the election through to each relative month T. We repeat the exercise for each T in the 4 year window surrounding an election closure. Similarly, then compute cumulative returns for the benchmark by summing benchmark returns over the same window:

$$CR_{iT}^{Benchmark}(-24, T) = \sum_{t=-24}^{T} Ret_{i,\tau+t}^{Benchmark} \tag{10}$$

5.4.1 Forming Abnormal Returns

There are two important considerations for the long-run analysis. The first is how to treat firms that have elections within 2 years of one another. We use all elections between 1961 and 2023. There are 24,705 elections that occur while the firm is publicly listed. But, to avoid "double counting" relative months, we only take elections that are the first election at a firm or are at least 24 months away from the last election. For example, if a company has an election 12 months after the previous one, then there will be 12 months that are "double counted" as both pre-election and post-election periods: The first 12 months after the first election will serve as $T = 0$ to $T = 12$ for the first election and $T = -12$ to $T = 0$ for the second one. There are 6,493 elections that fit these criteria, of which 3,412 are first

elections.

The second consideration is what to use to benchmark returns. Lee and Mas (2012) – (LM)in this section – use a value-weighted portfolio of returns from firms in the same size decile as the election firm at the time of the election for their analysis of the 1961-1999 period. For the analysis below, we adopt the the same procedure. Each figure below compares average returns at firms where establishments unionize relative corresponding benchmark returns, $CR_{iT}^{benchmark}$, matched on size decile to a value-weighted portfolio from the CRSP database.

We compute the average cumulative returns, ACR, for each relative period by dividing the sum of cumulative returns by the number of elections, N:

$$ACR_{iT} = \frac{1}{N}\sum_{i=1}^{N} CR_{iT} \qquad \text{for } T \in \{-24, \ldots, 24\} \tag{11}$$

for average returns at firms that have elections and

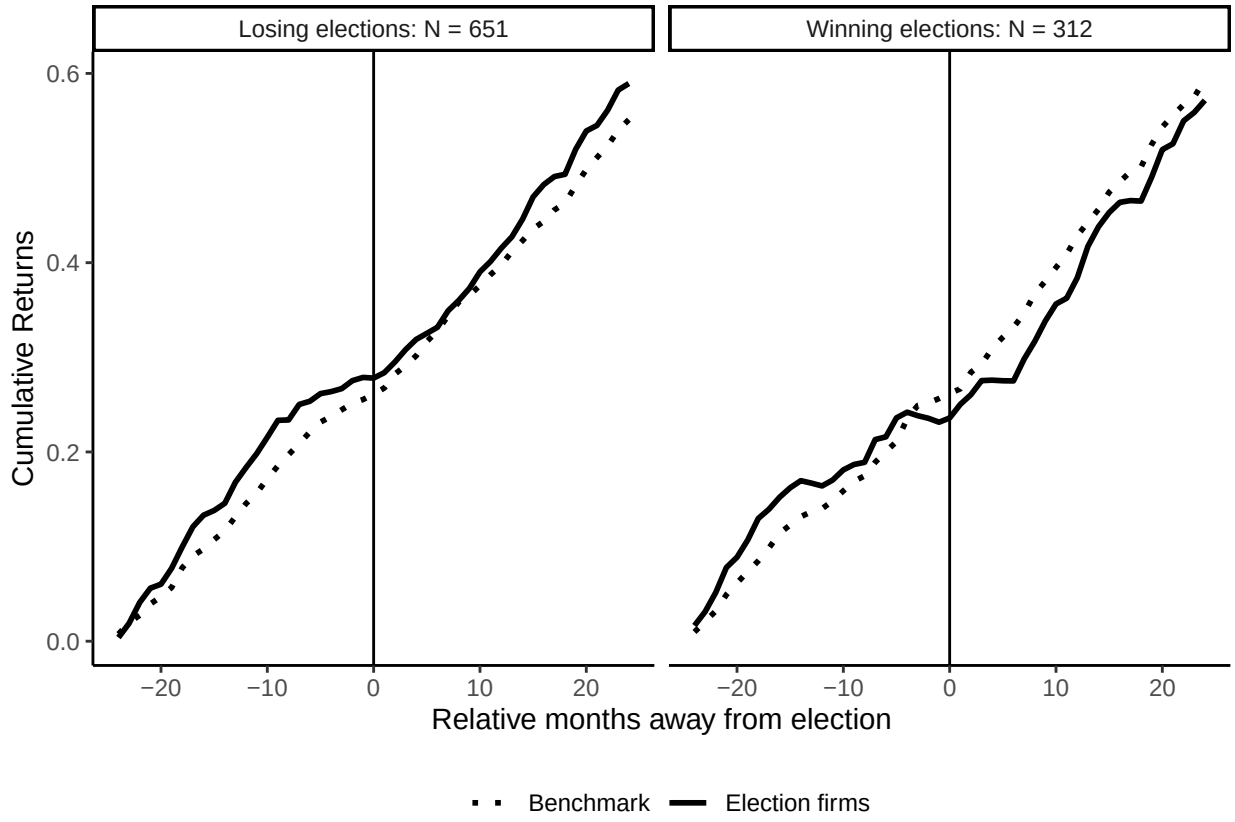
$$ACR_{iT}^{Benchmark} = \frac{1}{N}\sum_{i=1}^{N} CR_{iT}^{Benchmark} \qquad \text{for } T \in \{-24, \ldots, 24\} \tag{12}$$

for the associated benchmarks. If our panel were balanced, N would just be all elections. However, because our panel is unbalanced, there are elections that do not have a full two year window on either side of the election. Instead, N is equal to the number of firms that have return data in each relative month T.

Before viewing these results for our sample period of interest – 1961-2023 – we test whether our procedure produces qualitative results similar to those in the literature. As an initial vetting procedure for our data construction, we repeat the data construction process above to form abnormal returns for wins and losses at "big" firms for the period between 1961 and 1999. This, in theory, should generate a dataset similar to that of LM in which they find large, negative abnormal returns for election winners. The full results from our replication of the results from LM can be found in Appendix B. In short, using our matching criteria, we are able to qualitatively replicate their results. We match a similar number of winning and losing elections and find similar results in terms of cumulative returns for both winning and losing firms. In short, we view this as prima facie evidence that our matching procedure and dataset construction process can replicated one vetted in existing literature.

Figure 6 display our results for the long-run outcomes for firms with big elections between 1961-2023. This figure separates the results by winning and losing elections and compares average election outcomes with those for their paired benchmark.

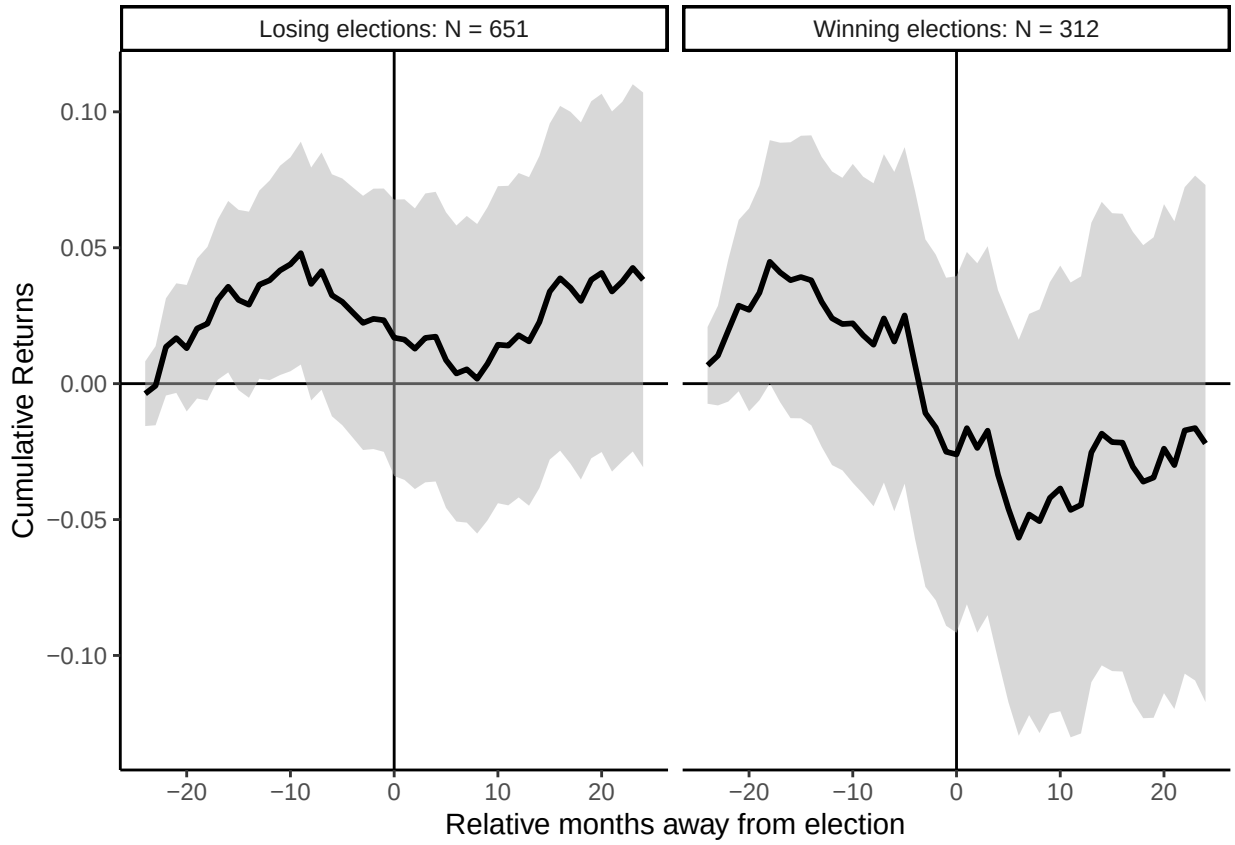
Figure 6: Cumulative returns - election companies and benchmark



Note: ACR_{iT} and $ACR_{iT}^{Benchmark}$ for $T \in [-24, 24]$

Figured 7 display our results for average cumulative abnormal returns separately for winning and losing elections.

Figure 7: Average cumulative abnormal returns



Note: Difference between ACR_{iT} and $ACR_{iT}^{Benchmark}$ for $T \in [-24, 24]$.

5.5 Robustness to Heterogeneous Treatment Effects

A recent strain of econometrics literature shows that the standard two-way fixed effects (TWFE) estimator we employ can lead to $\hat{\beta}_f$ and $\hat{\beta}_c$ being biased estimates of the treatment effects of filings and closures even when Assumptions 1 and 2 hold. When the effects of union victories filing or closing are heterogeneous across firms or change over time, even in isolation (absent including one of the two treatments) the estimators may produced biased estimates of the average treatment effects (ATEs) due to identifying an non-convex combination of firm-level treatment effects (De Chaisemartin and d'Haultfoeuille, 2023). Second, the TWFE specification can suffer from a contamination effect, where the estimators for each average treatment effect (ATE) in the specification of equation (5) $\beta.$ in isolation (that is, the effects of closing *or* filing) are comprised weighted averages of the underlying firm-level effects of both treatments (De Chaisemartin and D'haultfœuille, 2023).

To check to the extent to which contamination and negative weights may bias our estimates, we employ the test suggested in De Chaisemartin and d'Haultfoeuille (2020). The test shows that for the baseline specification in equation (5), only the latter source of bias may treated our identification. The estimated firm-level level effects of filings (closures) all enter the weighted summations positively in the TWFE estimator $\hat{\beta}_f$ ($\hat{\beta}_c$). The portion of the estimates $\hat{\beta}_f$ and $\hat{\beta}_c$ that comprise estimated ATTs of their respective subscripts all receive positive weights. However, there is some contamination in each estimator in our setting; the estimators for filings and closures both contain weighted summations of the other estimated treatment.[8] This contamination implies some bias in our TFWE estimates even when Assumptions 1 and 2 hold.

De Chaisemartin and D'haultfœuille (2023) suggest an alternative estimator, the DiD_M^f, that is robust to treatment effect heterogeneity and multiple treatments. Their estimator in the case of two non-binary treatment variables provides an unbiased estimate of the weighted average effect of unit increases in treatment level (increasing the victories filed on a given trading day by one) on stock returns while holding a second treatment (the number of victorious election closed at that firm) constant. This is in essence an unbiased estimator for the average treatment on the treated (ATT) in our setting. 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 $\mathbf{W}_{.,t-1}$ 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 8: Filings and Closures, DiD_M^f Estimator

Dep. Var: $R_{it} - R_{rf,t}$	Baseline
Filed Wins	2.987
	(-)
Closed Wins	6.480
	(-)

Note: bootstrapped standard errors clustered at the firm level shown in parentheses.

[8] 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.

Table 8 shows the results from re-estimating equation (5) specified with no X covariates with fixed effects at the firm and day level using the DiD_M^f estimator. The coefficients should be interpreted as a weighted average of the effect of an additional victorious establishment-level election being filed on firm returns that trading day. The estimates suggest an additional winning establishment-level election being filed (closed) is associated with returns rising by 6 basis points. The alternative estimates are qualitatively in line with our baseline findings in Section 5 given the point estimates are very close to zero. However, our estimated effects here are substantially less precise. Bootstrapped standard errors from the DiD_M^f estimator are large enough that while we cannot reject a null effect, we fail to reject a negative effect as large as (-XYZ) basis points.

6 Conclusion

This paper presents new evidence of how establishment-level union efforts affect the stock returns of publicly-listed firms. To analyze this, we construct a panel of the universe of union petitions filed with the NLRB between 1994 and 2023. We use a fuzzy matching procedure to identify among these elections the subset held at an establishment of a publicly-listed firm or one of its subsidiaries. Finally, we map the dates of each election into a longitudinal dataset of stock returns to form panel of establishment-level election filings and closures for firms listed in the CRSP database.

To identify the causal effect of union activity on stock returns, we restrict our matched sample to only firms that experienced at least one establishment-level unionization after 1961. Restricting our sample this way ensures that our control group in each period is still the set of firms that at some point have had establishments select into unionization. This avoids the threats to identification due to the failure of parallel trends holding across unionized firms and those which never unionize discussed in Dinlersoz et al. (2017) and Frandsen (2021). While our result is robust to a less restrictive sample selection procedure, we are to our knowledge the first to make an intuitive case for why this identification strategy may be more credible that other approaches motivated by the regression discontinuity approach which compares close winning and losing elections (c.f. DiNardo and Lee, 2004; Lee, 2008).

We find a precise null combined effect of establishment-level union petitions being filed and closed on the contemporary stock returns for the associated company. While filing of an establishment-level election causes the associated firm's stock returns to decline by roughly 7 basis points that day, subsequent election closures offset this effect entirely. We fail to reject that the cumulative effect of elections – the sum of the effects from both filings and closings – causes a net negative effect larger than 11 basis points in magnitude (less than

two days' of average returns) at the 95% level. This (null) hypothesis holds for a broad suit of alternative definitions of how "treatment" by unions is assigned, which firms we use as controls, the time period we consider, and when we allow for treatment effect heterogeneity across different types of elections.

Finally, we revisit results from Ruback and Zimmerman (1984), Lee and Mas (2012), and Hofmann and Schoonjans (2023) concerning the effects of unionization on stock returns over long time horizons as opposed to those which are contemporary to the timing of specific phases of election procedures. For the period of 1994-2023, we fail to reject long-run effects of unionization on long-run cumulative stock returns at all horizons up to 24 months following an initial closure date. While our long-run estimates are less precise the point estimates are smaller than a 2 percentage point drag on returns after two years.

Other papers examining how unionization affects establishment-level outcomes suggests that multi-establishment firms have the ability to adjust production in response to union activity. Firms have been found to reallocate production across plants (Wang and Young, 2022) or shed longer-tenured employees (Frandsen, 2021) in response to union activities. Our results suggest that these firm level responses may have important dampening effects on any long-run drags on stock returns unionization may otherwise have. Because we see the initial impact of filings offset by closures, we instead believe that the small negative effects we see are driven by uncertainty rather than investors' fears over concrete effects of unionization on future dividends.

Our study provides new evidence that may inform the long-running political discourse surrounding organized labor which has traditionally framed unions and firms as enemies.[9] The issue of trade unions has become especially salient due to the rise in income inequality being attributed to their decline (Dinlersoz and Greenwood, 2016; Farber et al., 2021 as well as their renewed presence at the forefront of the American political economy (Naidu, 2021).[10] Our evidence suggests that those who hold equity in publicly owned firms may in fact sacrifice little if they choose to side with labor versus management.

Future work examining other channels by which firms may mitigate any negative effects of unionization on profits or highlighting alternative channels by which unions may offset stock losses that might be expected due to increased bargaining power in the wage setting process is a promising future avenue for research. Until then, our findings suggest the perceived negative effect of unionization on stock as portrayed in popular narratives may be difficult to reconcile with realized outcomes.

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

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

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Appendix

Contents

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

Table 1 displays the number of firm names that we match to the CRSP *when elections occur while the company is publicly listed or is listed as a subsidiary.* To assure ourselves that our name-matching procedure is adequate, we also match based on name alone. For this exercise, we remove all elections that have fewer than 100 eligible voters. We also remove universities and hospitals/medical centers, as these are unlikely to be publicly traded. Table A.1 reports our match rates when using only name. We match typically match between 20 and 30 percent of names (second to last column from the right). These names constitute between roughly 20 and 30 percent of eligible voters (right-most column).

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

Years	Unique firm names	Matched firm names	Matched eligible voters	Tot. eligible voters	Pct. firm names matched	Pct. voters matched
1960-1965	2,337	398	152,992	649,256	17%	24%
1965-1970	3,317	704	236,919	891,428	21%	27%
1970-1975	2,927	699	247,049	778,577	24%	32%
1975-1980	2,634	605	180,420	624,457	23%	29%
1980-1985	1,816	380	109,742	445,928	21%	25%
1985-1990	1,524	285	79,899	338,475	19%	24%
1990-1995	1,424	306	78,832	327,472	21%	24%
1995-2000	1,667	390	108,270	395,221	23%	27%
2000-2005	1,184	263	68,314	270,230	22%	25%
2005-2010	604	120	30,131	150,372	20%	20%
2010-2015	662	175	47,306	154,874	26%	31%
2015-2020	539	128	29,002	128,628	24%	23%
2020-2025	291	56	16,199	84,259	19%	19%

Note: Names matched from the NLRB to the CRSP. Only includes elections with at least 100 workers eligible. Removes hospitals, medical centers, and schools/universities.

B Comparison of Matching and Long-Run Outcomes with Lee and Mas (2012)

Table B.1 tabulates cross-sectional statistics of matched elections for five-year periods between 1961 and 2023 which underpin the graphical presentations in Figures 1,2, and 3.

Table B.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.

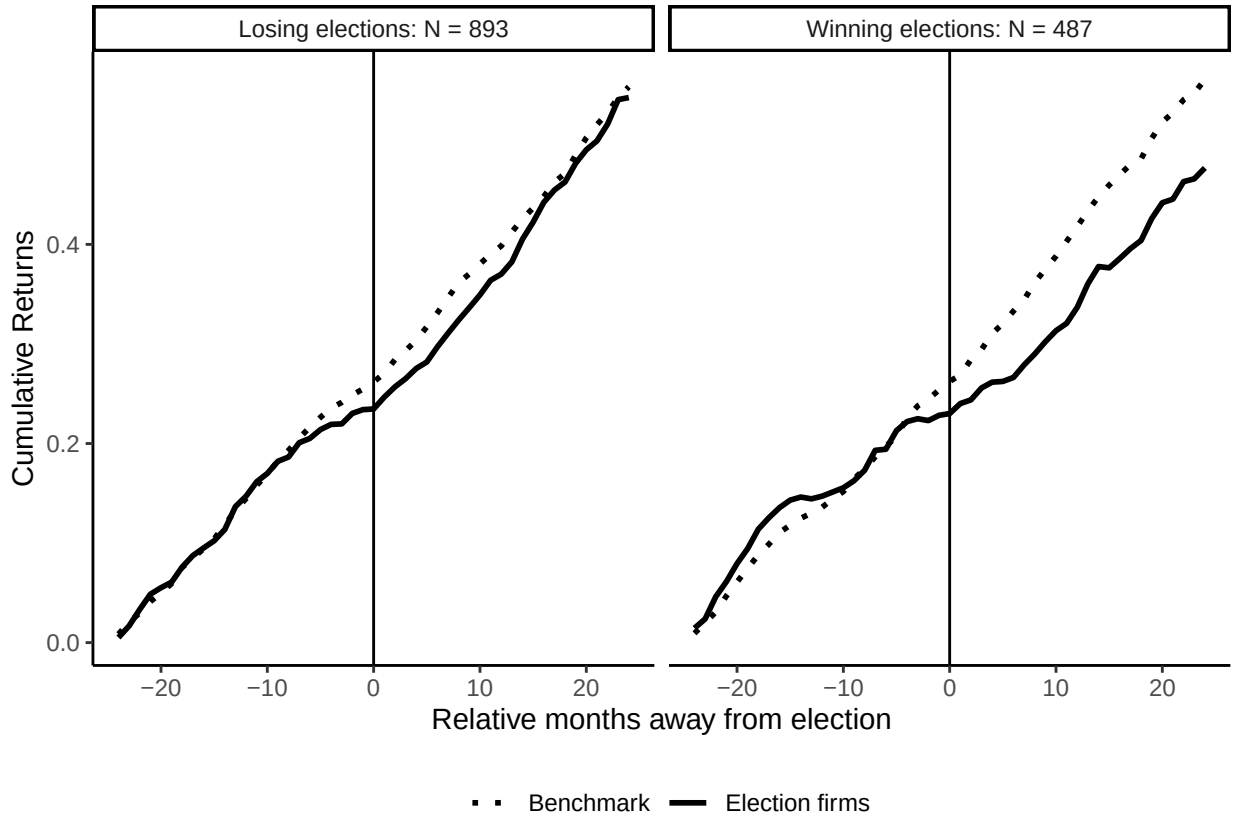
Table B.2 shows our fuzzy matching procedure does a good job in replicating the sample of Lee and Mas (2012) based on their selection criteria.

Table B.2: Lee and Mas (2012) Sample Replication, 1961-1999 "Big Elections"

	At least 5% of workforce voting				Less than 5% of workforce voting			
	Win	LM Win	Loss	LM loss	Win	LM Win	Loss	LM Loss
Number of elections	**473**	414	**867**	1022	**1278**	1163	**2293**	2682
	[-]	[-]	**[-]**	[-]	**[-]**	[-]	**[-]**	[-]
Percent vote for union	**75**	62	**35**	35	**75**	64	**34**	35
	[20]	[11]	**[10]**	[10]	**[19]**	[13]	**[10]**	[10]
Number of voters	**526**	449	**440**	454	**227**	277	**275**	298
	[916]	[535]	**[542]**	[559]	**[236]**	[263]	**[279]**	[302]
Number of eligible voters	**818**	496	**492**	494	**291**	286	**302**	318
	[1721]	[649]	**[642]**	[639]	**[310]**	[286]	**[318]**	[330]
Percent of eligible voters voting	**15**	21	**20**	23	**1**	0.01	**1**	0.01
	[15]	[21]	**[17]**	[21]	**[1]**	[1]	**[1]**	[1]
Year of election	**1974**	1975	**1977**	1977	**1973**	1975	**1977**	1977
	[9]	[9]	**[9]**	[9]	**[9]**	[9]	**[10]**	[9]
Number of employees	**6418**	3813	**3805**	3431	**52056**	68469	**69586**	75285
	[12733]	[5378]	**[6377]**	[5195]	**[97153]**	[134337]	**[113993]**	[123610]
Percent in manufacturing	**81**	78	**79**	75	**78**	79	**72**	81

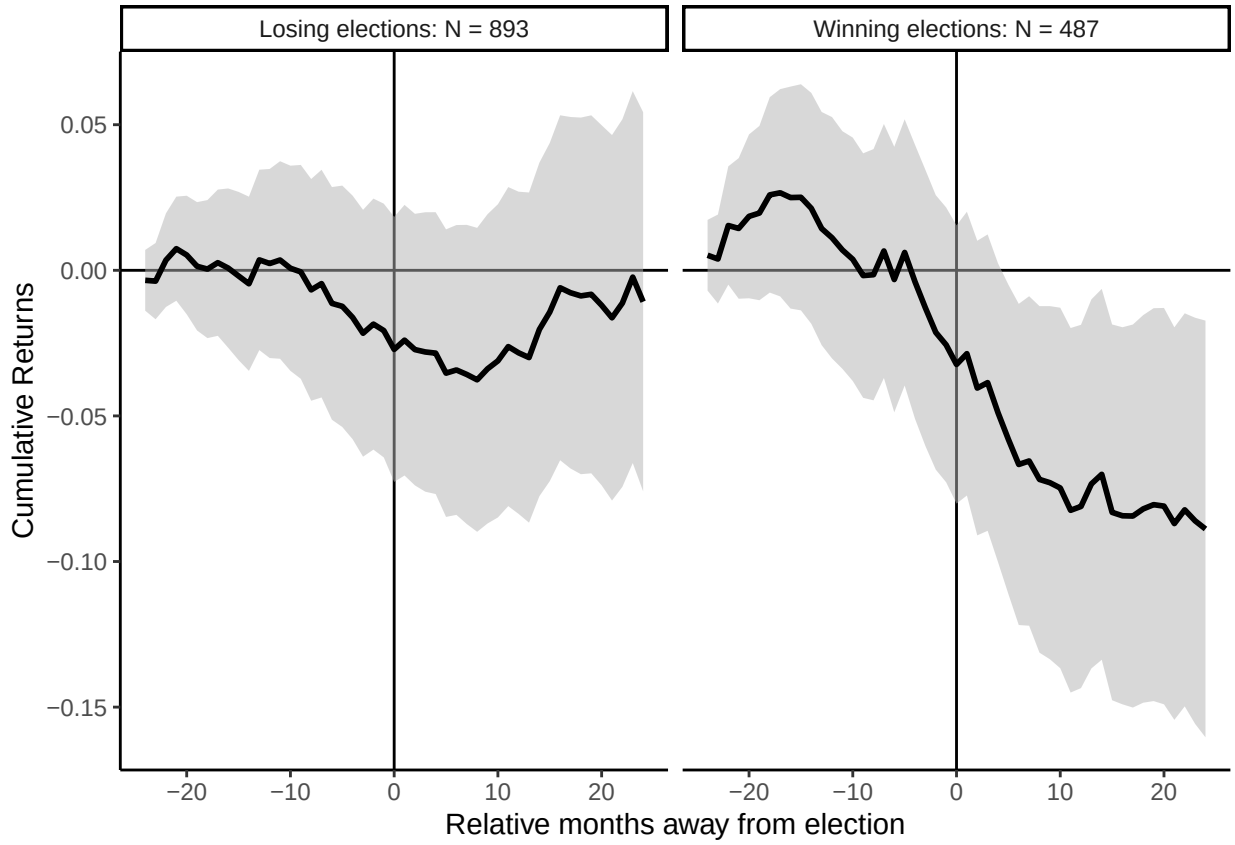
Note: Replication of the estimating sample in Lee and Mas (2012) using our matching procedure. Columns with "LM" denote values taken from Table 1 of Lee and Mas (2012). Standard deviations for each summary statistic are reported in brackets.

Figure B.1: Lee and Mas (2012) Long Run Cumulative Returns after Elections



Note: Replication of Figure 3 from Lee and Mas (2012)

Figure B.2: Lee and Mas (2012) Long Run Abnormal Returns after Elections

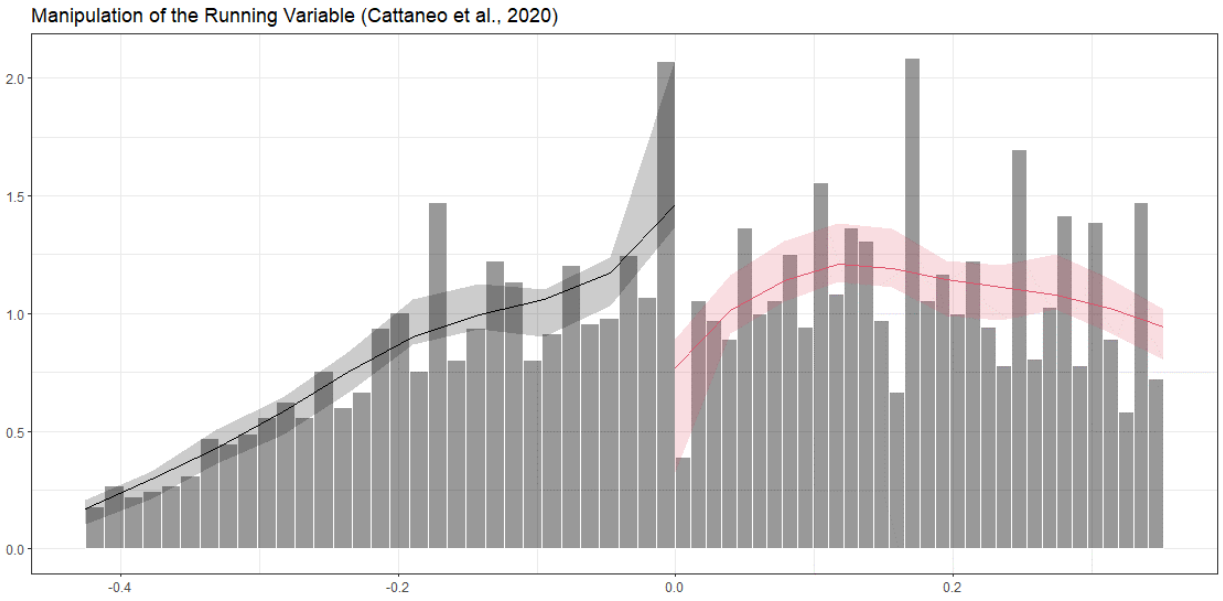


Note: Replication of Figure 3 from Lee and Mas (2012). Shaded regions denote 95% confidence intervals.

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

An RDD 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 the validity of the underlying assumptions in RDD designs. Figure C.1 gives graphical evidence that the share of elections just around the majority threshold is unlikely to be continuous. The fitted values local polynomial from the local polynomial estimator generated by the *rddensity* program (Cattaneo et al., 2018) are overlaid on the graph; the test proposed by (Cattaneo et al., 2018) rejects the null hypothesis of no manipulation of the running variable (vote shares) at the **XYZ** level.

Figure C.1: Vote Shares in Close Union Elections



Visualization of Continuity Test from Cattaneo et al. (2018)

D Robustness: Alternative Measure of Stock Returns

This appendix examines whether our findings in Section 5 in the main text are robust to alternative definitions of our outcome variable. Sections D.1-D.4 reveal a lack of substantive effects of union elections on returns as measured by this alternative outcome. This hold true across several different treatment specifications. Finally, section D.5 illustrates that there are no lagged or cumulative longer term effects from elections.

D.1 Filings and Closures

We redefine an alternative outcome variable in this appendix, where from here forward in Appendix D the left-hand side variable of interest is

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

firm-level daily returns in excess of the risk free rate that day. This alternative measure of returns follows from the capital asset pricing model literature (Fama and French, 2004). Table 3 shows results for various specifications of equation (D.1)

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

where W_{it}^f and W_{it}^c are count variables in variables measuring the number of winning elections filed or closed on that day. 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).

Table D.1: The Effect of Establishment-Level Union Victories on Stock Returns

Dep. Var: $R_{it} - R_{rf,t}$	(1)	(2)	(3)	(4)
Wins Filed	-0.00082	-0.00064	-0.00067	-0.00042
	(0.00038)*	(0.00037)	(0.00037)	(0.00056)
Wins Closed	0.00058	0.00076	0.00076	0.00060
	(0.00039)	(0.00035)*	(0.00033)	(0.00058)
$p(H_0 : \beta_1 + \beta_2 = 0)$	0.68	0.81	0.84	0.83
Time Fixed Effects	No	Yes	Yes	Yes
Firm Fixed Effects	No	Yes	Yes	Yes
Firm-Level FF3	No	No	Yes	Yes
Include Losses	No	No	No	Yes
N	8,036,956	8,036,832	8,004,661	8,004,661

Note: Parenthesis show standard errors clustered at the firm level. Firm-level FF3 allows for firm-level loadings on the Fama French (1993) factors.

Table D.1 illustrates using this alternative definition of firm-specific returns leads to minimal changes from our point estimates in the main text regardless of specification. Column (4) includes variables L_{it}^c and L_{it}^f that count the number of losing elections at the establishment level filed or closed at firm i each day and also shows little change. The same holds true for being unable to reject that the two effects are equal in magnitude under any of the specifications in Table 3, as shown in the row testing $H_0 : \beta_1 + \beta_2 = 0$.

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

It's possible our coefficients when estimating (D.1) are affected by days where many

elections take place which exhibit a large degree of leverage. Table D.2 shows our results when we replace our count variables $W_{it}^{\cdot}$ with indicators variables, $\mathbb{I}\{W_{it}^{\cdot} > 0\}$, that take on a value of one if any establishment-level election victories are filed (closed) that day.

Table D.2: Binary Treatments

Dep. Var: $R_{it} - R_{rf,t}$	(1)	(2)	(3)
$\mathbb{I}\{W_{it}^{f} > 0\}$	-0.00075	-0.00053	-0.00055
	(0.00051)	(0.00046)	(0.00046)
$\mathbb{I}\{W_{it}^{c} > 0\}$	0.00078	0.00091	0.00089
	(0.00049)	(0.00045)*	(0.00043)
$p(H_0 : \beta_1 + \beta_2 = 0)$	0.96	0.55	0.59
Time Fixed Effects	No	Yes	Yes
Firm Fixed Effects	No	Yes	Yes
Firm-Level FF3	No	No	Yes
N	8,036,956	8,036,832	8,004,661

Note: Parenthesis show standard errors clustered at the firm level. Firm-level FF3 allows for firm-level loadings on the Fama French (1993) factors.

Table D.2 shows the results when count variables are replaced by binary treatments. Results remain qualitatively unchanged across all specifications from those above in Section D.1 and in the main text.

D.3 Alternative Treatment: Weight by Employees Covered

It's possible our coefficients are attenuated by small elections. Table D.3 shows our results when we weight our count variables by the share of a firms' employees covered by the number of employees who are eligible to participate in each establishment-level election. Let $\%\text{covered}_{it}$ be the share of employees at firm i who are covered by the bargaining units of establishments that file (close) election petitions on day t. We construct

$$\tilde{W}_{it}^{\cdot} = \mathbb{I}\{W_{it}^{\cdot} > 0\} \times \%\text{covered}_{it}$$

this variable captures the share of employees at firm i that file or close a union on a given day. This is meant to give more weight to elections which cover a larger share of employees at a given firm.

Table D.3: Coverage-Weighted Treatments

Dep. Var: $R_{it} - R_{rf,t}$	(1)	(2)	(3)
$\tilde{W}_{it}^{f}$	-0.00655	-0.00670	-0.00668
	(0.00100)**	(0.00091)**	(0.00091)
$\tilde{W}_{it}^{c}$	-0.00053	-0.00032	-0.00027
	(0.00117)	(0.00117)	(0.00116)
$p(H_0 : \beta_1 + \beta_2 = 0)$	0.00	0.00	0.00
Time Fixed Effects	No	Yes	Yes
Firm Fixed Effects	No	Yes	Yes
Firm-Level FF3	No	No	Yes
N	7,848,732	7,848,671	7,820,693

Note: Parenthesis show standard errors clustered at the firm level. Firm-level FF3 allows for firm-level loadings on the Fama French (1993) factors.

Table D.3 shows results when count variables are replaced by the share of a firm-level employees who would be eligible for coverage by a collective bargaining agreement under a given establishment-level victory. We interpret these results as the effects of increased shares of firms' employees being covered by unions. However, as most establishment-level unionization efforts cover a small share of total firm-level employees (as discussed in Section 2) extrapolating this further than marginal changes may be inappropriate. At face value our estimates suggest unionization of the entirety of a firm's employees lowers returns by about 65 basis points on net. Importantly, the net effect is significantly different from zero in all specifications. The results in Table D.3 suggest that establishment-level efforts that comprise a large share of a *firm's* total employees have a larger, more precise negative impact on firm-level returns.

D.4 Alternative Treatment: Isolate Extensive and Intensive Effects of Establishment Victories

Finally, we retest the results from Section 5.3 to examine whether there is a significant different between the extensive and intensive margins of firm-level unionization.

Table D.4: Extensive and Intensive Margins of Union Victories

Dep. Var: $R_{it} - R_{rf,t}$	(1)	(2)	(3)
Win Filed	-0.00093	-0.00052	-0.00048
	(0.00058)	(0.00051)	(0.00051)
Win Closed	0.00058	0.00071	0.00061
	(0.00054)	(0.00049)	(0.00047)
Initial Win Filed	0.00074	-0.00003	-0.00026
	(0.00119)	(0.00113)	(0.00110)
Initial Win Closed	0.00080	0.00078	0.00108
	(0.00121)	(0.00113)	(0.00111)
Time Fixed Effects	No	Yes	Yes
Firm Fixed Effects	No	Yes	Yes
Firm-Level FF3	No	No	Yes
N	8,036,956	8,036,832	8,004,661

Note: Parenthesis show standard errors clustered at the firm level. Firm-level FF3 allows for firm-level loadings on the Fama French (1993) factors.

Table D.4 displays the effects of first-time versus subsequent firm-level elections.

D.5 Longer Time Horizons

A potential shortcoming of the TWFE specification that uses high-frequency identification and daily outcomes in Section 5 is it may overlook effects of unionization on stock returns that take place over a longer horizon. Lee and Mas (2012) show evidence that the negative effects of unionization on firm-level stock returns emerge at a two year horizon. Other results in the literature that look at establishment-level outcomes such as wage bill, employment, and survival find that negative effects emerge as much as five years after a successful unionization elections (Knepper, 2020; Frandsen, 2021; Wang and Young, 2022).

To test for long-run effects, we follow Lee and Mas, 2012 and examine how the cumulative return on stock prices equity prices initial successful elections affect at affected firms stocks for treated firms. Define the cumulative return for firm i over a given horizon t_1 to t_2 as the sum of monthly stock returns R_{it} over this period:

$$CR_i(t_1, t_2) = \sum_{t=t_1}^{t_2} R_{it}$$

For each initial victorious election closed on date t at firm i, define cumulative returns (CR) over horizon τ relative to the month the initial victorious election is closed normalized to $t = 0$ as

$$CR_{it}(-24, \tau) = \sum_{t=-24}^{\tau} R_{it} - R_t^f \qquad \text{(Cumulative Returns)}$$

This metric captures cumulative returns from 24 months prior to a victorious being election being closed up through month τ relative to election closure. Similarly, define average cumulative returns at this horizon as the average value of cumulative returns from 24 months prior to that normalized time $t = 0$ across firms $j \in \{1, \ldots, J\}$ as them across firms

$$ACR_t(-24, \tau) = \frac{1}{J} \sum_{j \in \mathcal{J}}^{J} CR_j(-24, \tau) \qquad \text{(Average Cumulative Returns)}$$

This outcome captures the average cumulative returns across a set of firms $\mathcal{J}$ over the τ-month horizon beginning 24 months before a given month t normalized to zero.

To examine how firms experiencing an initial union election victory perform in the long run relative to other firms, we construct this average cumulative return metric for all other firms which experienced an initial union victory prior to 1994 around the same date. The set $\mathcal{J}$ is then the set of all publicly-listed firms in our dataset for which at least one establishment successfully unionized before 1994. We calculate equation (Average Cumulative Returns) for the set of winners $\mathcal{J}$ around each initial election victory month after 1994 so as to create an average cumulative return among winners that serve as controls, analogous to our sample selection process described in Section 4.

For each initial victorious election after 1993, we construct the cumulative returns at the treated firm i as well as average cumulative returns for all other firms which have had previously successful union elections around the same month in which that election takes place. For each initial victory, we difference the firm-level cumulative returns $CR_{it}(-24, \tau)$ and the average returns at already-unionized firms $ACR_t(-24, \tau)$ at each horizon to construct a measure of cumulative *abnormal* returns CAR. Abnormal returns reflect how a given stock

performs relative to some expected return cumulative return $\mathbb{E}[CR_i(-24,\tau)]$. We assign this to be the basket of previously-unionized firms around the same time period:

$$CAR_{it}(-24,\tau) = CR_{it}(-24,\tau) - \underbrace{ACR_t(-24,\tau)}_{\mathbb{E}[CR_i|X_\tau]} \qquad \text{(Cumulative Abnormal Returns)}$$

Finally, we average this metric for cumulative abnormal returns across all initial union victories closed

$$\hat{\beta}^\tau \;:=\; ACAR(-24,\tau) \;= \frac{1}{N}\sum_{i=1}^{N} CAR_i(-24,\tau) \qquad \text{(ACAR)}$$

for each monthly horizon in a four-year window surrounding each election across the $N = 261$ unique monthly dates on which at least one firm experiences an initial successful election closure after 1993. This allows the $\hat{\beta}^\tau$ terms from equation (ACAR) to capture average cumulative abnormal returns (in that they exceed a benchmark value) over a given horizon τ.

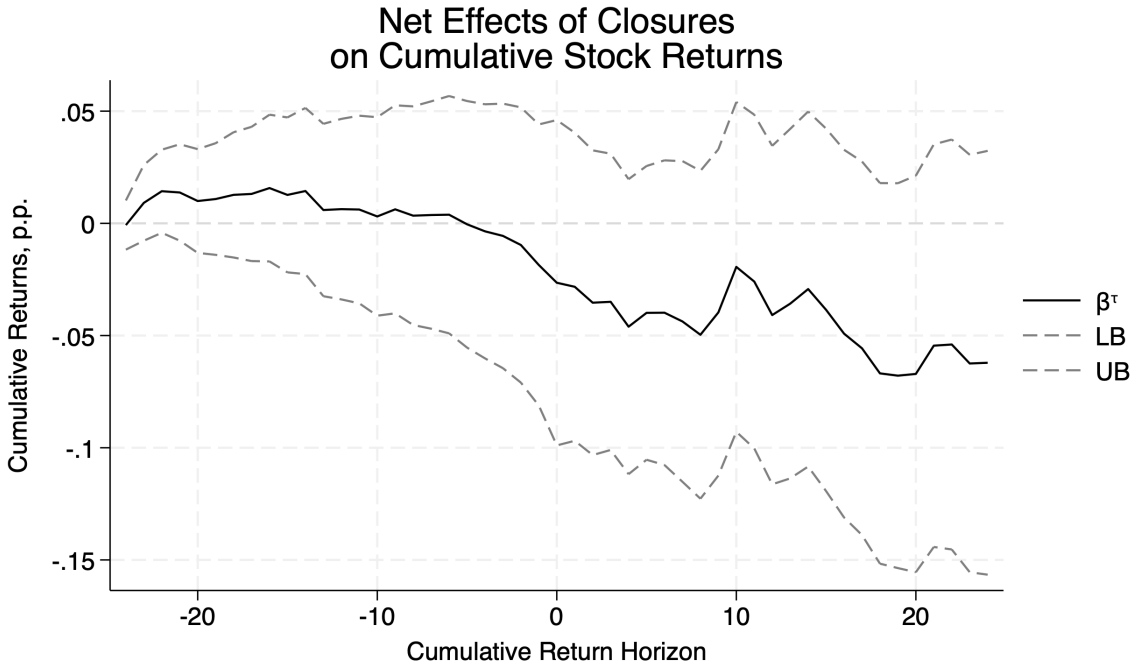


Figure D.1: Net Effects of Successful Closings on Cumulative Returns – Controls as Other Winners

Figure D.1 displays the effects of closures on cumulative returns at horizons ranging

from 24 months prior through to 24 months after an election is closed. Our point estimate is centered on zero throughout most of the 24 months prior to elections, indicating that prior to election victories the set of firms where unions have already won serves as a good control group. There is however some initial downward movement in relative cumulative returns within six months of the election. While our estimates become less precise at horizons longer than 24 months, we cannot reject a null effect of closures on returns at any time horizon.

We repeat this exercise with an alternative set of firms used to construct expected returns around first elections. For each first victory, we instead calculate average cumulative returns over the set of all firms where an election is filed after 1961 $\mathcal{J}'$:

$$ACR_t(-24, \tau) = \frac{1}{J'} \sum_{j \in \mathcal{J}'}^{J} CR_j(-24, \tau) \qquad \text{(Average Cumulative Returns')}$$

In this alternative specification, the control group $\mathcal{J}'$ is now a larger set of firms including all firms with union activity after 1961 that do not have an initial victory post-1993. Note that by definition this alternative control group contains the more-restrictive subset of winners, such that $\mathcal{J} \subset \mathcal{J}'$. The results with this alternative control group to use to construct our measure of average cumulative abnormal return in equation (ACAR) are displayed in Figure D.2. Unlike with the results in Figure D.1 where only winning firms are used as controls, clear pre-trends emerge in the alternative specification when comparing new winners post-1993 with firms that are already affected by union activity. The newly-unionized firms' begin to display clearly lower cumulative returns nearly a year away from an initial election closure, making the assumption of parallel trends unlikely to hold in this setting. While our point estimate at the 24 month horizon is a relative difference of about 10 percentage points in cumulative returns, about half of this difference occurs prior to the actual election closure.

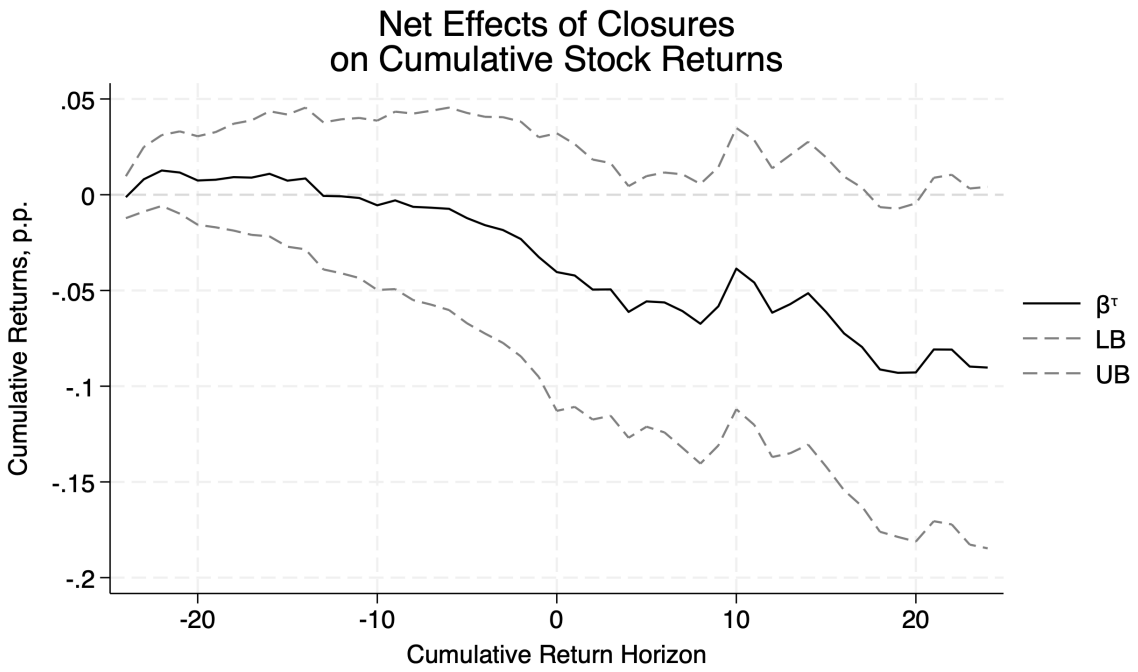


Figure D.2: Net Effects of Successful Closings on Cumulative Returns – Controls as All Elections

Finally, motivated by the results from Lee and Mas (2012) we compare cumulative returns between firms where the first election filed is a win versus those where it's a loss. The relative path of cumulative returns at firms where first elections win (lose) is displayed in Figure D.3. While our confidence intervals are large, they are universally centered around zero and our point estimate suggests virtually no difference between trajectories for winners and losers after an initial establishment-level election is closed.

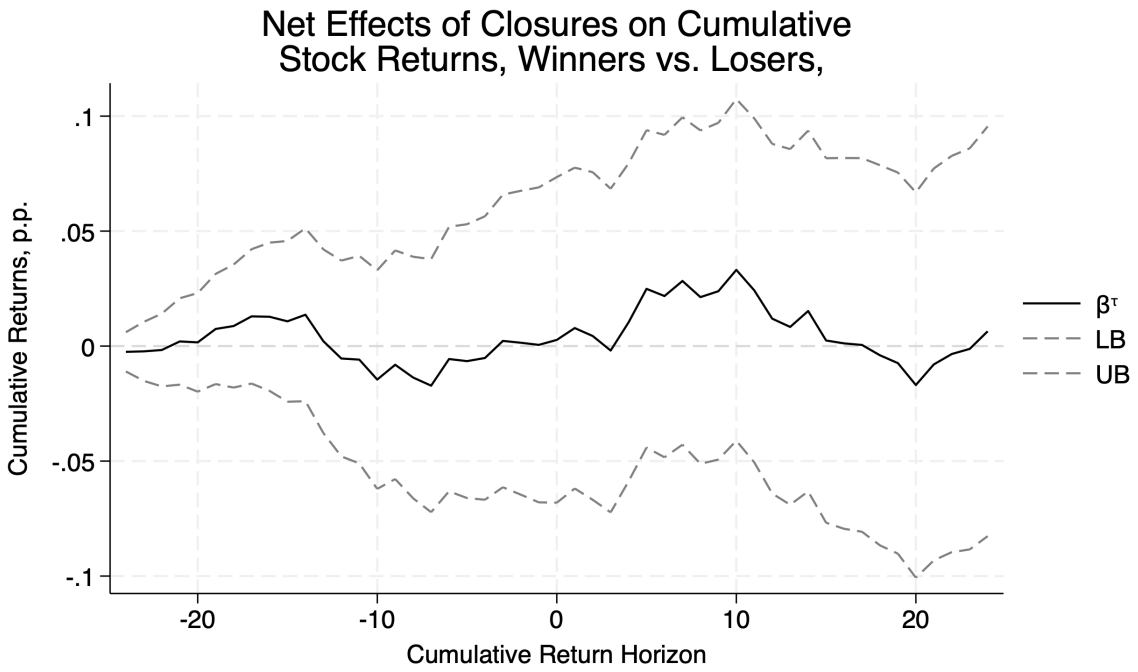


Figure D.3: Net Effects of Successful Closings on Cumulative Returns – Controls as Losing Firms

E Robustness: Alternative Specifications of the Main Estimating Equation

Table E.1: Elections filed and closed, dummies

	All elections	Wins	Losses
Filed Dummy	-4.827	-5.920	-3.496
	(3.316)	(4.355)	(4.777)
Closed Dummy	5.434	8.500	1.772
	(2.950)	(4.470)	(4.481)
Fixed Effects:			
Date	Yes	Yes	Yes
Firm	Yes	Yes	Yes
Observations	8,042,535	8,042,535	8,042,535
R^2	0.00762	0.00762	0.00762
$p(H_0 : \beta_f + \beta_c = 0)$	0.89	0.67	0.80

Note: Estimating 5 using dummy variables for any elections filed or closed instead of count variables

F Robustness: Alternative Estimating Samples

Table F.1: Wins and losses, filed and closed - All companies

	All elections	Wins	Losses
Filed	-6.214*	-5.213	-7.486
	(3.044)	(3.803)	(4.701)
Closed	6.679*	9.493*	3.298
	(2.822)	(3.976)	(4.332)
Fixed Effects:			
Date	Yes	Yes	Yes
Firm	Yes	Yes	Yes
Observations	31,120,869	31,120,869	31,120,869
R2	0.00263	0.00263	0.00263
$p(H_0 : \beta_f + \beta_c = 0)$	0.91	0.42	0.53

Note: Estimating equation 5 using all CRSP companies

Table F.2: Wins and losses, filed and closed - All companies

Wins filed	-5.220
	(3.804)
Loss filed	-7.476
	(4.701)
Wins closed	9.487*
	(3.975)
Loss closed	3.307
	(4.333)
Fixed Effects:	
Date	Yes
Firm	Yes
Observations	31,120,869
R2	0.00263

Note: Estimating equation 7 using all CRSP companies

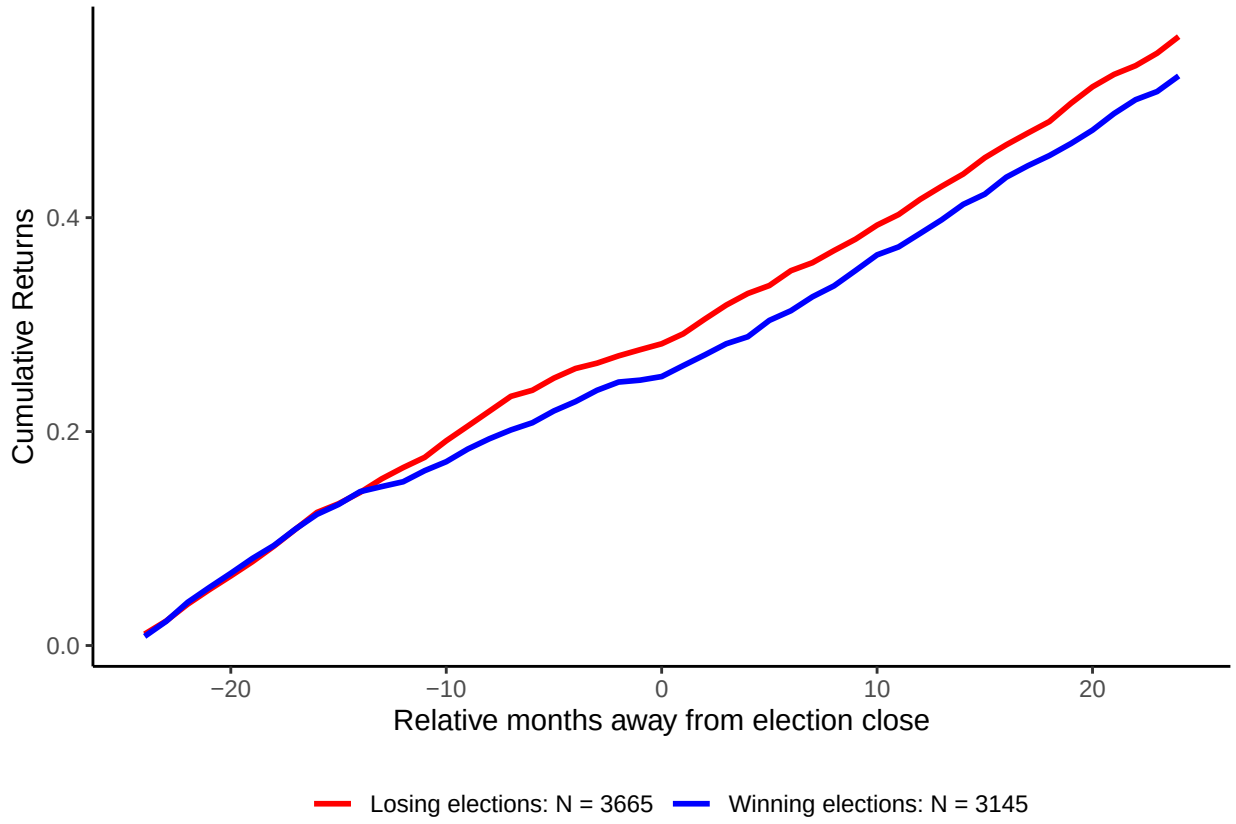


Figure F.1: Long run effects, winners vs losers, all elections

Note: This figure compares firms where the union won to firms where the union lost. It includes only elections that do not have another election 2 years prior.

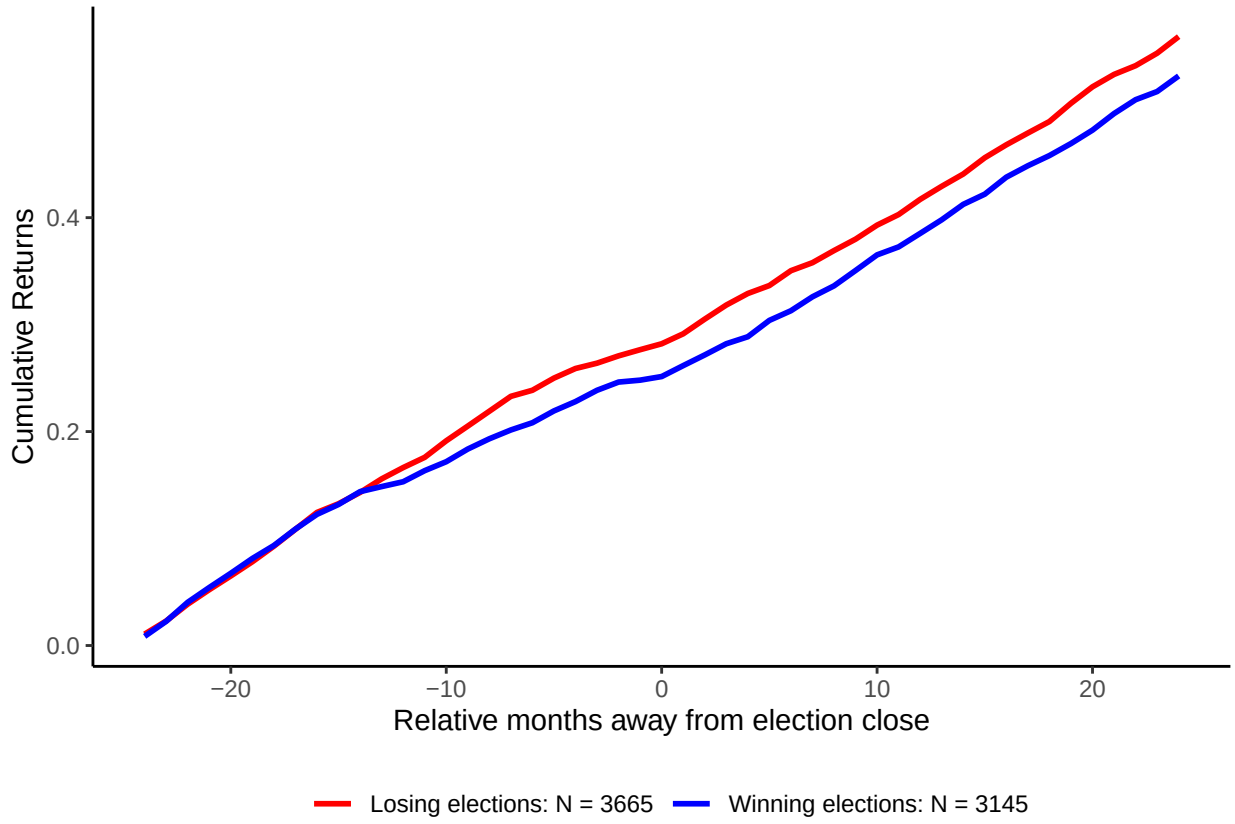


Figure F.2: Long run effects, winners vs losers, first elections

Note: This figure compares firms where the union won to firms where the union lost. It includes only first elections.

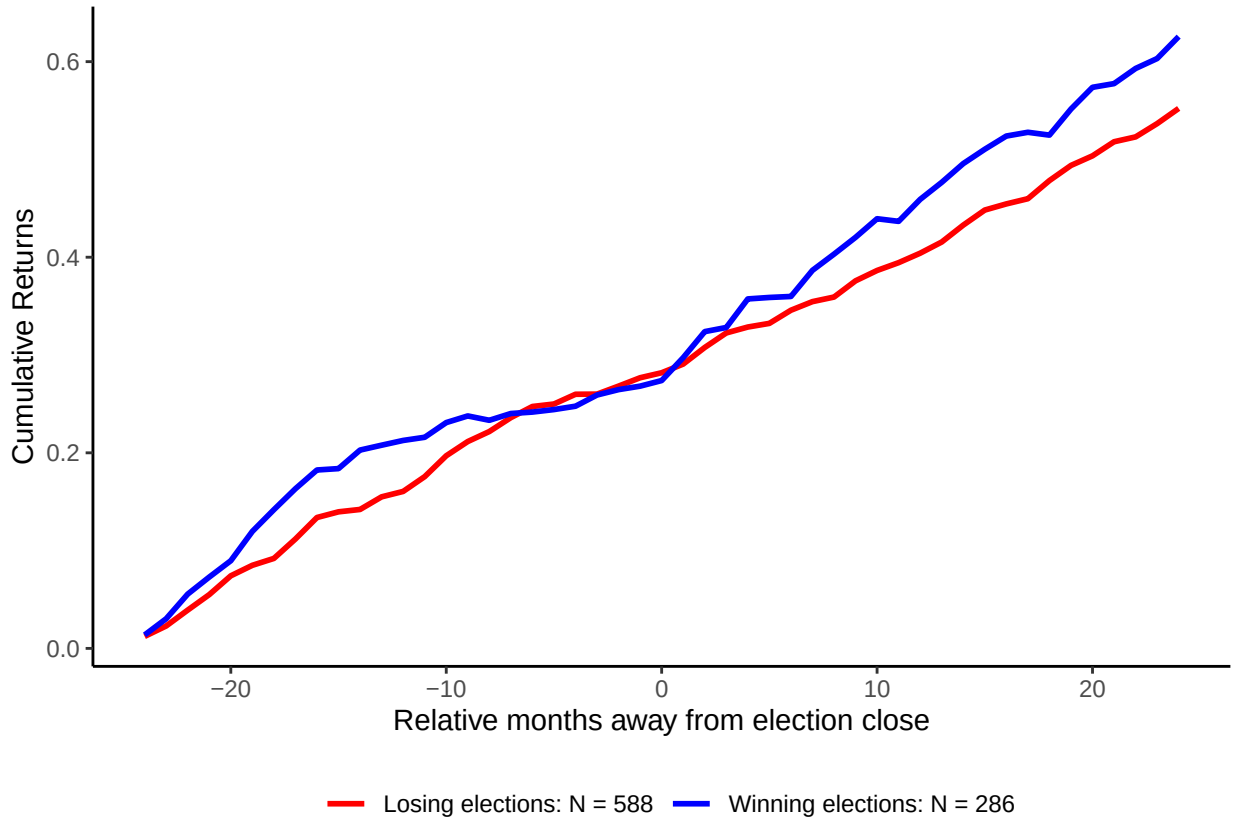


Figure F.3: Long run effects, winners vs losers, first elections

Note: This figure compares firms where the union won to firms where the union lost. It includes only elections where the union received between 45% and 55% of votes.