Long-Run Impacts of Unions on Firms:
New Evidence from Financial Markets, 1961-1999[1]

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Abstract

We estimate the effect of new unionization on the equity value of firms over the 1961-1999 period using a newly available sample of National Labor Relations Board (NLRB) representation elections matched to stock market data. As in Dinardo and Lee (2004), the point estimates from a regression-discontinuity design – where we compare the stock market impact of close union election wins to close losses – imply that unions pose small costs to firms. Event-study estimates using both close and large victories, however, show that new unionization is associated with at least -10% abnormal returns, equivalent to $40,500 per unionized worker. We find a negative relationship between the cumulative abnormal returns and the vote share in support of the union, thus allowing us to reconcile these two seemingly contradictory findings. The lack of a discontinuity in abnormal returns at the 50% union vote share threshold is a result of firms experiencing increasingly worse financial performance when workers are more supportive of unionization. When viewed through the lens of a "median voter" model of endogenous union determination, the patterns we find are consistent with firms having limited responsiveness to the threat of new unionization, and unions moderating their demands in order to gain electoral advantage.

"[L]aymen and economists alike tend, in my view, to exaggerate greatly the extent to which labor unions affect the structure and level of wage rates." – Milton Friedman, 1950[1]

"Everyone 'knows' that unions raise wages. The questions are how much, under what conditions, and with what effects on the overall performance of the economy." – Richard Freeman and James Medoff, 1984[2]

1 Introduction

It is undisputed that employers oppose unions, viewing them as a threat to profitability. An example that has received recent national attention is Wal-Mart's effort to resist unionization, from its strategic location of stores in areas less favorable to unions, to its hard-line stance against organization (Basker, 2007). According to a handbook that the retailer distributed to its managers, "Staying union free is a full-time commitment... The commitment to stay union free must exist at all levels of management – from the Chairperson of the "Board" down to the front-line manager...."[3] It is easy to find isolated cases that confirm the fears of employers like Wal-Mart. As an example, in a March 1999 National Labor Relations Board (NLRB) representation election, workers at National Linen Service (NLS) Corp., a large linen supplier, voted by a more than 2 to 1 margin to organize as a local chapter of Union of Needletrades, Industrial, and Textile Employees (UNITE). The stock market appears to have punished NLS in a severe, though perhaps not swift, fashion. Figure 1 shows the cumulative return of NLS' stock beginning two years prior to the election through two years after, as well as the cumulative return of a broad market index over the same period. Prior to the election, NLS and the market returns tracked each other quite closely. But immediately following the election, NLS began to lag. By March 2001, the price of NLS shares had fallen by 25%, while the broad market index had increased by 50%.

How general is this phenomenon? Is National Linen Service Corp. the exception or the rule? Despite an enormous literature that has documented numerous aspects of unions and their role in the labor market, the magnitude of a "average" effect of unions on firm performance throughout the

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[1] See Friedman (1950).
[2] See Freeman and Medoff (1984).
[3] Quoted in Featherstone (2004).

economy remains somewhat unclear, as we argue below.

Empirically, there are at least three reasons why measuring these effects has proven to be quite challenging. First, large-scale establishment or firm-level micro-data containing the relevant information on the extent of unionization are not readily available. Second, even when such data are available, omitted variables and the endogeneity of unionization at the level of the firm makes it difficult to separate causal effects from other unobserved confounding factors. A third reason is that it is difficult to find data that can also be plausibly representative of the population of unionized companies in the United States.

From a theoretical standpoint, it is not obvious to what degree unions should affect firms. One view, articulated by Milton Friedman, is that workers would reject substantially above-market wages, knowing full well that such wages could have adverse effects on job security. Unions, after taking these considerations into account, would tend to moderate wage demands.[4] Moreover, firms may respond to a unionization threat by conceding higher wages and better working conditions. These forces would tend to reduce the gap in compensation and working conditions between union and non-union workforces, at least in those situations where there is a threat of unionization. These ideas may help explain the results of DiNardo and Lee (2004), who find generally small differences in wages, employment, and output between unionized and otherwise comparable non-unionized workplaces.

In this paper, we first assess the extent to which the pattern in Figure 1 is a more general phenomenon, measuring an average overall effect of unionization among publicly-traded firms. To do so, we begin with a sample frame that is the universe of all firms in which NLRB union representation elections took place between 1961-1999. Since most unionized workplaces in the U.S. come into existence via a secret-ballot election on the question of representation, this population provides a reasonable representation of newly unionized workplaces, and to the extent they survive, the future stock of unions in the U.S.

We use event-study methodologies to analyze the stock market reaction to union victories. The most distinctive feature of our data – crucial for our research design – is the long panel of up to 48 months before and after the election, of high-frequency data on stock market returns for each firm. This feature allows us to use the pre-event data to test the adequacy of the benchmarks used to predict the counterfactual returns in the post-event period. The long panel also allows us to

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[4]It is this line of reasoning that led to Friedman's view that the impact of wages was exaggerated (Friedman, 1950).

examine returns several months beyond the event, so as to capture the long-run expected effects of new unions, without having to rely heavily on the assumption that the stock price immediately and instantaneously adjusts to capture the expected presence of the unions.

Our event-study analysis reveals substantial losses in market value following a union election victory, equivalent to \$40,500 per unionized worker. The evidence is compelling in the sense that we find that these firms' average returns are quite close to the predicted returns for every month leading up to the election. Precisely at the time of the election, the actual and counterfactual returns diverge. The results for these firms are robust to a number of different specifications. Notably, in the sample of firms where we know that the union is a small fraction of the workforce, we do not find a similar divergence.

We then employ a Regression Discontinuity (RD) design, implicitly comparing close union victories to close union losses. As in DiNardo and Lee (2004), we find little evidence of a significant discontinuous relationship between the vote share and market returns. The RD point estimates show, if anything, a 4 percent *positive* (though statistically insignificant) effect of union certification (vis-a-vis union defeat). The event-study estimates vary systematically by the observed vote share, with the largest negative abnormal returns for cases where the union won the election by a large margin.

We interpret the evidence through a simple model of endogenous unionization. This framework offers predictions for the various kinds of union impacts that might arise when firms can alter working conditions to prevent the union from successfully organizing, and when unions can elect to moderate their demands in order to attract more support. Specifically, we consider a two-party model of electoral competition, where the firm and the union are seeking to win the sympathies of the "median" voter in an NLRB election. As is standard in this class of models, the parties, although having opposing interests, may be forced to propose a level of compensation, accompanied by a risk of job loss, that is closer to that preferred by the median voter. The model implies that the regression-discontinuity design estimate of the unionization effect identifies the gap between the union and firm's proposal when the median voter is indifferent between the two. Depending on how aggressively firms and unions court voters, this gap could be close to zero, even if *on average* – including both small and large electoral victories – unions affect the profitability of firms on average.

Viewed through the lens of the theoretical framework, the pattern of effects is broadly consistent

with firms that do not (or cannot) compete for voters by changing working conditions. In a way that is reminiscent of Friedman's view, the evidence is also consistent with unions that modify their positions in order to gain electoral advantage. The estimates imply that voters have strong desire for higher wages in only a relatively small number of elections. But because the firm is mostly unresponsive to voters, these elections are associated with a large pro-union vote share, and a substantial reduction in equity value of the firm. The results imply that whatever salary increase is enacted following an NLRB election, it should rarely be larger than the amount desired by the median voter, regardless of who wins the election. Therefore, the model implies that new unionization should lead to relatively limited employment effects, even when the firm experiences a considerable reduction in value.

The paper is organized as follows. Section 2 briefly highlights what is known from the literature, and how our study relates to that literature. Our theoretical framework for precisely defining the different kinds of union effects is presented in Section 3. We then provide some institutional details in Section 4 that are relevant to our research design, which is described along with our data. We present and discuss the results in Section 5. Section 6 concludes.

2 Existing Literature and Background

In this section we provide a brief overview of the literature that is most related to our analysis. First, there is an enormous union wage premium literature, discussed and summarized in the landmark works of Freeman and Medoff (1984) and Lewis (1986), with more recent evidence discussed in Blanchflower and Bryson (2007). Most of these studies use household-level survey data to compute the wages for workers who are union members, comparing them to "otherwise comparable" non-union members. In some cases, these studies involve following workers in longitudinal data sets, as they switch from union to non-union status. In their analysis, Freeman and Kleiner (1990) note that these "[e]stimates based on longitudinal data... contrast workers who change union status by moving to or from already organized workplaces rather than contrasting workers in plants that are newly organized with those in plants that remain nonunion." Following this point, DiNardo and Lee (2004) make clear that the effect of *unionization* (changing a workplace from non-union to unionized) is distinct from the effect of moving a *worker* from a non-unionized workplace to a

unionized workplace. In particular, a "typical" unionized workplace may differ from a "typical" non-unionized workplace along a number of different dimensions (e.g. geography, firm size, industry) which themselves may independently influence wage levels. We therefore view this well-established literature as being fundamentally unable to account for the selection of unionism at the firm- or establishment-level, and therefore potentially estimating something quite distinct from the causal effect of unionization.

Next, there is a literature that does utilize firm- or establishment-level data containing information on union status. As pointed out in Hirsch (2007), a recent review of this evidence, there are a number of important reasons why caution is warranted in drawing inferences from the existing research. First, there can be important omitted variables, unobserved determinants of the long-run viability of the firm that could be correlated with the presence of the union. Related to this idea is a potential endogeneity problem, whereby unions may specifically target a highly profitable firm for organization. Alternatively, it may be poorly managed, resulting in low-performing firms, leading to the demand for worker representation. Examples of studies that implicitly rely on the assumption that union status is an exogenous variable include the in-depth analyses of Clark (1984), Hirsch (1991a), and Hirsch (1991b). A second limitation that Hirsch (2007) emphasizes is the limited generalizability of many of the studies. For example, the cement industry is examined in Clark (1980a) and Clark (1980b), hospital and nursing homes in Allen (1986a), the construction industry in Allen (1986b), the trucking industry in Rose (1987), and sawmills in Mitchell and Stone (1992). It is difficult to extrapolate from these studies on productivity to a broader, representative cross-section of firms in the U.S. Indeed, our analysis is largely motivated by the notion that it might be easy to find particular incidents and/or companies where unions have imposed large costs on firms. The question, however, is to what extent isolated examples such as that illustrated in Figure 1 generalizes to a broader population of interest.

Finally, there are three particular studies that we consider to be most related to our analysis – that of Lalonde et al. (1996), Ruback and Zimmerman (1984), and DiNardo and Lee (2004). We believe our analysis addresses some of the most important limitations of each of these studies.

The main difficulty faced in Lalonde et al. (1996) – which utilizes a "fixed effects" approach with establishment level panel data from the Longitudinal Research Database (LRD) to examine the impact of a successful union organizing campaign – is one of interpretation. The study shows some

differences in employment growth between the eventually successful and failed organizing attempts, *prior* to the election event. For example, one sample shows an expanding gap in employment, while another shows a contracting gap. Overall, the estimates and standard errors are consistent with pre-election employment growth differences ranging from -10 percent to 14 percent. As a result, Lalonde et al. (1996) are careful to note that their examination of pre-election growth rates for many of the outcome variables proved "inconclusive", and that their "subsequent findings on the effects of unionization may be too large." Essentially, the main problem is that the data they examine are not rich enough to rigorously test their "difference-in-difference" specification with the pre-event data, and as a result more caution is required in interpreting the post-event patterns.[5]

A similar issue arises in the well-known study of Ruback and Zimmerman (1984), which, like our analysis, examines the stock market reaction to NLRB union certification events.[6] There, the main estimates of a 3.8 percent drop in the stock market valuation is computed within a few months surrounding the unionization event.[7] Again, the difficulty in interpretation arises from the substantial negative abnormal returns that emerge *well before* the unionization event, a decline in market value of about 7 percent between the 12th and 7th months preceding unionization. While Ruback and Zimmerman (1984) have no explanation for this important decline, they argue that it is unlikely to indicate anticipation of the outcome of the election due to the its timing.[8] This pattern raises the question as to whether the post-election decline in the stock market valuation reflects unionization, or whatever factors that might have led to the pre-election trend in the first place.

In our analysis, we address these ambiguities by taking advantage of a very long panel of monthly

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[5]Another study in the spirit of a "before-after" design is that of Freeman and Kleiner (1990), in which 203 establishments were surveyed before and after NLRB elections took place, and were compared to 161 "control" firms, where elections did not take place. In the study, there was only one period before and one period after, so testing the over-identifying restrictions of the difference-in-difference design was not possible.

[6]There are a number of other studies that examine various aspects of unions as seen through stock market reactions. They typically do not aim to generate effects of unionization (versus the absence of unions), as they use samples of already unionized firms or industries. See Abowd (1989), Becker and Olson (1986), Neumann (1990), DiNardo and Hallock (2002), and Becker (1987). Olson and Becker (1990) is an exception in this regard, as it examines the impact of the passage of the National Labor Relations Act on 75 firms that were at risk of being unionized in the 1930s.

[7]Specifically, their main estimate of -3.84 is computed by taking the 1-month change associated with the petition date and adding it to the 1-month change associated with the date of the actual certification. This can be seen as the summation of the third and fifth rows, which equals the first row of the third column in their Table 2. Their main estimate can also be seen in their Figure 1(c) as the summation of the two downward notches around the petition and certification dates.

[8]Specifically, on p. 1145, they note that "[t]he abnormal return for these firms in the 6 months immediately preceding the petition is 0.16 percent. This timing suggests that the pre-petition abnormal returns are not due to unionization. Instead, the results suggest that firms in which unions are successful experienced declines in value prior to the union activity."

data on stock returns, using an arguably more disciplined approach to modeling the counterfactual "no union" state. Specifically, we use the data between 24 months prior to the event and just before the event to *test* our specification. If there are significant departures between our predicted returns and the observed returns over a long period of time before the event, we then consider any estimates we obtain from the post-event data to be invalid.[9] This approach is a direct application of conventional testing of over-identifying restrictions for "difference-in-difference" modeling in labor economics program evaluation.[10] Furthermore, we track abnormal returns over a period of at least 24 months after the unionization. Strictly speaking, perfectly efficient financial markets predict that any changes in valuation caused by the outcome of the election will be fully capitalized into the stock price by the time the outcome of the election is known. Nevertheless, our approach is to rely less on the assumption of instantaneous adjustment, by examining the patterns of returns for many months following unionization, and thus allowing time for the market to adjust.

The final study to which the present study relates is the regression discontinuity analysis of union elections, using the data from the LRD, as in DiNardo and Lee (2004). There, they exploit the "near-experiment" generated by secret ballot elections, comparing establishments where unions became recognized by a close margin of the vote with those workplaces where the union barely lost. The most precise estimates in that study are those on wages: wage increases of 2 percent could be statistically ruled out as far away as seven years after the election.[11] There are a number of important limitations to inferring the long-run costs of unions from this evidence. For one, it may take a much longer time – perhaps 10 or more years – for unions to establish support within the workplace to have the required bargaining power to negotiate for substantially higher wages. Second, unions impose other costs that are not measured by the LRD, such as the use of seniority rules, work rules, grievance procedures, and other working conditions specified in union contracts. In principal, our approach of examining the effect of stock market valuation can address both of these concerns: if the market correctly prices the firm, it should capture the sum of all costs imposed by the union, and effects that might occur many years into the future should be capitalized into the stock market

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[9]An alternative interpretation of pre-election divergence in the predicted and actual returns is the diffusion of anticipatory information regarding the election outcome. Recognizing this alternative, we allow for non-zero excess returns in a short window prior to the event, but conclude that any significant divergence over a long-period of time prior to the event is evidence of a mis-specified model.

[10]For example, see Ashenfelter and Card (1982) and Heckman and Hotz (1989).

[11]Interestingly, the magnitudes are also in line with what was found on wages in Lalonde et al. (1996). Freeman and Kleiner (1990) also find wage effects that are much smaller than those found in cross-sectional worker-level studies.

valuation of the firm in the short-run.

A final important limitation is that the RD analysis, by estimating a discontinuity in the relation between wages and the vote share at the 50 percent threshold, can only estimate a weighted average treatment effect, where the weights are proportional to the *ex ante* likelihood that an election was to be "close".[12] That is, among the observed close elections, a disproportionately small number would have had the fundamentals of strong union support, for example. The RD is fundamentally unable to provide a counterfactual for the set of elections in which the workers voted 90 percent in favor of unionization. By contrast, the present analysis seeks to estimate effects for precisely these "inframarginal cases". In the analysis we describe below, we present results from both an event-study as well as an RD approach, and provide a framework for interpreting both sets of results.

3 Conceptual Framework

Here we summarize a simple conceptual framework that we believe is a reasonable way to describe the interaction between the management, union, and the workers at a firm in the events leading up to potential unionization.[13] We use this framework to precisely define the different kinds of union effects and the various counterfactuals needed for our empirical analysis. Specifically, our goal is to develop a model that allows us to translate the observed relationships in the data into the parameters governing the behavior and composition of unions, firms, and worker during representation elections.

We consider a variant of "median voter model" of endogenous union determination. While the median voter-type model has previously been considered in the theoretical literature on unions (see Atherton (1973), Farber (1978) and Booth (1995)), we recognize there is likely to be a virtually unlimited number of distinct ways to model union elections, bargaining, and union threat. Our goal, therefore, is to describe a theoretical framework that has as its backbone a few elements that any sensible model of the process should include.

The basic idea of the model is that the firm and the union each propose an outcome, and the voters choose between the two in a secret ballot election. For purposes of exposition, suppose the key issue is the wage; it should be clear that the model applies to any contentious issue over which the workers stand to gain at the firm's expense. Both the union and firm try to persuade the

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[12]For a detailed discussion of this interpretation, see Lee (2008).

[13]Some of these ideas are discussed, but not fully or formally developed, in DiNardo and Lee (2002, 2004).

workers to vote for or against union representation by proposing different wage levels. The voters will select a proposal that maximize their utility, taking into account that too high a wage increases the likelihood of job loss as a result of the firm moving up the labor demand curve. We formalize the model below.

3.1 Setup

This model has three agents who are involved in a recognition election: workers, the union, and management.[14]

- **Workers.** Workers are considered to maximize their own individual utility, which could be, for simplicity, lifetime wealth. In this environment, each worker will face the decision to either vote for or against union recognition. In doing so, each forward-looking worker compares the anticipated outcome if the union wins to the expected outcome if the union loses. Reasons to vote for the union might include higher wages, benefits, better working conditions; reasons to vote against might include the potential responses of the firms (e.g. lower employment). We assume that each worker has an "ideal" bargaining unit-level wage increase, with the median worker's preferred level denoted μ. To simplify the problem, we assume that positive and negative deviations from this ideal wage enter symmetrically into worker loss functions. We assume that there is heterogeneity in μ across establishments, perhaps depending on the firm's elasticity of labor demand.

- **Union.** The unions choose a proposed pay raise, denoted y_U, which is the percent increase from the pre-election wage. As we only consider situations whereby the union proposes a larger salary increase than the firm, if the union raises its offer, it is a more extreme offer, and all those who already were in favor of a lower y_U will not change their vote, while some on the margin will be swayed from switching from the union to the management. Suppose the union's offer is a simple linear function $y_U = \alpha + \beta\mu$.[15,16] The parameters α and β summarize the

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[14]The setup is similar to Ashenfelter and Johnson (1969) who also consider management, workers, and unions as separate maximizing entities.

[15]More precisely, the union offers $y_U = \max(\alpha + \beta\mu, 0)$.

[16]We recognize that, strictly speaking, this model is deterministic in the sense that vote share and election outcomes are perfectly predictable given the parameters of the model. This raises the question of why unions and firms would choose a proposal that is known to produce a loss in an election. A natural extension to the model is to include an unpredictable component to the vote share. Many of the qualitative predictions of the model withstand this

"reduced-form" optimal response to the preferences of the workers' preferences, which is taken to be exogenous. For example, such a response function could arise if the union were to choose its proposal strategically, so as to make the best response given the firm's offer (which may also be chosen strategically), and to take into account the trade-off between the probability of winning the election and a higher wage, which presumably benefits the union.[17] The parameter α describes the extent to which the union moderates its offer when the electorate has a low tolerance for higher wages. The parameter β represents the responsiveness of the union to the preferences of the workforce. For example, if $\beta = 0$, the union always offers α to the bargaining unit. If $\beta = 1$, then the union offers a premium α, relative to the median voter's ideal wage.

- **Management.** The firm proposes a salary increase, denoted y_M (where $0 \leq y_M < y_U$). Raising its offer – and hence proposing something more moderate – will influence more workers to vote against the union. However, the firm's objective function penalizes higher wages. Symmetric to the union, we specify the firm's offer to be the linear function $y_M = \gamma + \delta\mu$.[18] The parameter δ affects the degree to which the firm responds to the union threat. If γ and δ both equal zero, the firm offers the prevailing wage, regardless of the preferences of the median voters. These parameter values are consistent with firms complying with the National Labor Relations Act (NLRA), which prohibits employers from increasing wages or benefits in response to a petition.[19]

3.2 Various Union Effects

The sequence of events is as follows: an election is announced, union and management propose pay raises, the election occurs and, depending on the true distribution of the worker preferences, either the firm or union wins and its offer is implemented. The outcome of the election results in a change in profits, denoted $\Delta\pi$, which itself is a decreasing function $\Delta\pi\left(\cdot\right)$ of the wage (with $\Delta\pi\left(0\right) = 0$, and $\Delta\pi\left(x\right) \leq 0 \ \forall x$) , so that if the firm prevails in the election we have $\Delta\pi = \Delta\pi\left(y_M\right)$ and if the union does $\Delta\pi = \Delta\pi\left(y_U\right)$. The party whose proposal is closer to the median voter receives

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modification, which is why we chose to present this more simple framework. It is also not necessary for the offer to be a linear function of the median voter preference, as any increasing function suffices. We present the linear model for expositional simplicity.

[17]Such a game is isomorphic to the model of final-offer arbitration in Farber (1980).

[18]More precisely, the firm offers $y_M = \min(\max(\gamma + \delta\mu, 0), y_U)$.

[19]See LRM Packaging, Inc., 308 NLRB 829, 834 (1992)

more than a 50 percent vote share, and therefore wins the election. For example, the union wins when $|y_U - \mu| < |y_M - \mu|$. This is true, when the median voter's preferred point μ is greater than the threshold $\overline{y} \equiv (y_U + y_M)/2$. This is true, after substituting the above response functions, when $\mu > \widetilde{\mu} \equiv \frac{\alpha + \gamma}{2 - (\beta + \delta)}$. Since $\tilde{\mu}$ is the point at which the median voter is indifferent, then as $\mu - \tilde{\mu}$ becomes more positive, the vote share for the union is larger.

We consider the thought experiment of repeated elections, with values of μ drawn from a distribution with pdf $h()$. We wish to explore how the parameters of this model translate into $E[\Delta\pi|v]$, a function that we will be estimating in this paper. Denoting the union vote share as v, we are interested in the following properties of $E[\Delta\pi|v]$:

$$B_1 = E[\Delta\pi|v \leq 0.5] = \int_{\mu \leq \widetilde{\mu}} \Delta\pi(\gamma + \delta\mu) \frac{h(\mu)}{\int_{\widehat{\mu} \leq \widetilde{\mu}} h(\mu)d\widehat{\mu}} d\mu,$$

$$B_2 = E[\Delta\pi|v > 0.5] = \int_{\mu > \widetilde{\mu}} \Delta\pi(\alpha + \beta\mu) \frac{h(\mu)}{\int_{\widehat{\mu} > \widetilde{\mu}} h(\mu)d\widehat{\mu}} d\mu,$$

$$B_3 = lim_{\Delta \to 0^+} E[\Delta\pi|v = 0.5 + \Delta] - lim_{\Delta \to 0^+} E[\Delta\pi|v = 0.5 - \Delta] = \Delta\pi(\alpha + \beta\widetilde{\mu}) - \Delta\pi(\gamma + \delta\widetilde{\mu}),$$

$$B_4 = lim_{\Delta \to 0^+} E[\Delta\pi|v = 0.5 - \Delta] = \Delta\pi(\gamma + \delta\widetilde{\mu}).$$

B_1 and B_2 are the average effect of a union loss and win, respectively. B_3 is the discontinuity in $E[\Delta\pi|v]$ at the 50% union vote share threshold. B_4 is the effect of a close union loss on profits.

In Figure 2 we graph the proposed union and firm offers, and their average, as functions of μ. The discontinuity–B_3–corresponds to the point $\widetilde{\mu}$, where the average of y_U and y_M intersects the 45 degree line. The size of the discontinuity can be inferred from the distance between the y_U and y_M curves at that point. Specifically, B_3 is more negative in the difference between y_U and y_M. The union wins when $\mu > \widetilde{\mu}$. Therefore, B_1 corresponds to the area under the y_M curve, to the left of $\widetilde{\mu}$. In particular, B_1 is the area under the y_M weighted by the conditional density of μ. Similarly, B_2 corresponds to the area under y_U, to the right of $\widetilde{\mu}$.

To illustrate how knowledge of B_1 through B_4 helps us infer the behavior governing unions and

firms, as well as the underlying preferences of workers, we explore the implications of the model in several cases. We first consider the behavior of firms. From the above formulation, it can be seen that $B_1 = B_4 = 0$ implies that the firm does not respond to the threat of an election ($\gamma = 0$), nor to the preferences of voters ($\delta = 0$). $B_1 = B_4 < 0$ implies that the firm responds to the election by offering a pay raise ($\gamma > 0$), but not to the preferences of the median voter ($\delta = 0$). On the other hand, $B_4 < B_1 < 0$ implies that the firm responds to the election and to the preference of the median voter. Therefore, by estimating B_1 and B_4 we can test if and how firms respond to the threat of unionization as a result of the election.

If the parameters α and β are such that $y_U > \mu$ over most values of $\mu > \widetilde{\mu}$, then the union tends to "overshoot" the preference of the median voter in the majority of elections. Such behavior is consistent with the union placing considerable weight in obtaining higher wages, at a cost of losing support of the workers. By contrast, if $y_U \leq \mu$ generally holds, the union is acting relatively conservatively, which would be consistent with the idea that the union reduces its demands in order to gain electoral support. What we infer about the union's behavior from realizations of B_2 and B_3 depends in part on whether the firm responds to threat. For example, if the firm abides by the NLRA, and does not increase wages in response to the election, then $B_2 = B_3$ implies that $\beta = 0$, in which case $B_2 = B_3 = \triangle \pi(\alpha)$. This scenario corresponds to the situation where the union offers a fixed wage increase that is independent of the preferences of voters. The degree to which the union is acting conservatively in such a scenario depends on α. $B_2 < B_3 < 0$ implies that unions are responsive to voter preferences, so $\beta > 0$.[20] However, in order to infer to what extent the union is moderating its offers, we need to know both α and β, or to what extent and over what range of values is y_U greater or less than μ.

We will revisit this model in the empirical section of the paper. After discussing our empirical analysis, where we estimate B_1 through B_4, we discuss how we would interpret the findings that result from our research design in light of this framework.

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[20]More generally, B_2 will be more negative than B_3 provided that $\partial \overline{y} / \partial \mu < 1$.

4 Institutional Background, Data and Research Design

The National Labor Relations Act provides the legal framework through which most workers in the United States become unionized. Workers that organize into unions through the procedures specified by the NLRA are guaranteed the right to bargain collectively. There are several ways that a group of workers may become unionized under the auspices of the NLRA, though it is believed that most new unionization occurs through representation elections (Farber and Western (2001)). There are several steps involved in this process, which are described in detail in DiNardo and Lee (2004). Briefly, when a group of workers decides to organize into a union, they first petition the NLRB to hold a representation election. To be legally granted an election by the NLRB, the petition must be signed by at least 30% of the workforce, typically over no longer than a six month period. Once the NLRB determines the appropriate bargaining unit, the NLRB holds an election at the work site. The union wins the election with a simple majority of support amongst the workers. Barring objections by the employer, a win means that the union is certified as the exclusive bargaining agent for the unit, and that the employer is legally required to bargain with the union in good faith.

The research design and subsequent data collection were motivated by our desire to estimate the average effect of union victories and losses in representation election on firm profitability, and to attempt to address some of the aforementioned puzzles and challenges we see in the literature. In collecting the data our goal is to obtain data on the profitability of firms over a long time span, with a panel structure that allows for an event-study design with a long event window. Our sample size needs to be large enough so that we can also estimate the cross-sectional relationship between post-event abnormal returns and the union vote share. For this reason, and because we are interested in how the union effect has evolved over time, we sought to collect information on elections over as many years as possible. Because data on profits of privately held firms are difficult to come by, we focus on publicly traded firms for which we can obtain stock market information and other performance measures available through mandatory disclosure.

4.1 Data set Assembly

This study primarily uses three sources of data: election results from the NLRB, data from the Center for Research on Security Prices (CRSP), and the CRSP/Compustat Industrial Quarterly

Merged Database.

The NLRB began publicly reporting representation election vote tallies in 1961. However, previous studies that used NLRB election data have typically used records that were already in electronic form (e.g. Farber and Western (2001), DiNardo and Lee (2004), and Holmes (2006)). We use those data for the 1977-1999 period, but augment those with data from 1961-1976 that we digitized for this study.[21] Data for the 1961-1976 period were hand-entered from hard copies of NLRB monthly election reports.[22] Among other things, the NLRB data set contains the number of voters that voted in favor of the union, the number of voters voting against the union, the number of eligible voters, the name of the company, a two digit industry code, the city and state of the election, and the month that the NLRB closed the election.[23] The CRSP and Compustat data were obtained from Wharton Research Data Services.

The primary objective of the data assembly process was to match companies in the NLRB election files to companies in the CRSP data file. This matching process is complex because while we know the company name where the election took place in the NLRB file, most other information is unknown.[24,25] Therefore, when matching we are looking for similarities in the name listed in NLRB election file to names that were *ever* present in the CRSP files. To this end, we created two data sets, one containing the company names in the NLRB election file, and the other containing every company name that has ever appeared in the CRSP database. This second data set will be hereafter referred to as the "master names file". The master names file contains every name of every company that has ever appeared in CRSP.[26] The master name file also contains a unique company id, the "PERMNO", which allows for further matching to the CRSP and Compustat databases.

There are 195,889 certification elections in the NLRB data set that could potentially be matched to companies in the master names file. Because the matching process is tedious, and must be largely

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[21]The 1977-1999 period data were obtained from Thomas Holmes' website (http://www.econ.umn.edu/~holmes/data/geo_spill/) and are used in Holmes (2006).

[22]The records were digitized by a data-entry service. We took a random sample of records and estimated that they were 99.9% accurate.

[23]For a limited number of years the NLRB data has information of the calendar date of the election and the calendar date the NLRB closed the case.

[24]Location of the election is not very useful for matching because the CRSP file only contains the location of company headquarters, which may differ from the location of any establishment undergoing a recognition election.

[25]The only additional information that could help us identify a match is the two digit SIC industry code of the establishment. However, the industry of an establishment may differ from the primary industry of the firm. This variable will be more useful as a check for the validity of the matches.

[26]Many companies have multiple names.

done manually, we excluded from consideration any election with less than 100 voters. This exclusion results in 24,709 firms in the certification election file to be potentially matched to firms in the master list of CRSP company names.[27],[28]. These elections are comprised of 61% of all workers eligible to vote in NLRB certification elections. Using this smaller subset of the elections, firms in the election file were compared to firms in the master CRSP file. The algorithm matches company names in the NLRB file to company names in the master names file based on a so-called "spelling distance." We used the same matching algorithm employed by DiNardo and Lee (2004), which makes use of the SAS SPEDIS function. This algorithm considers those comparisons that have a spelling distance above a pre-determined threshold as candidate matches.[29] The algorithm may match a company in the election file to more than one company name in the CRSP file. In these cases we selected the lowest spelling distance as the candidate match. If there was a tie in spelling distance between two candidate comparisons, we selected one match at random.

Because we matched firm on names only, manual inspection of the matches revealed that our automated procedure resulted in many matches that were obviously incorrect. Therefore, research assistants reviewed every match, and dropped those where they judged the two firm names as different companies.[30],[31] We then collected all of the unmatched companies in the election file, from the initial set of 24,709, and attempted to locate each one in Dun and Bradstreet's Million Dollar Directory and the Lexis/Nexis' Directory of Corporate Affiliations for the year of election. This step identified subsidiaries of publicly traded parent companies, and allowed us to spot companies that were dropped erroneously in the previous step.

We ultimately matched 7693 elections from the NLRB election file to companies in the CRSP master file. In 1579 cases, the firm in the CRSP file was not publicly traded at the time the election.

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[27]Because a firm can have multiple elections, this number includes multiple cases of the same firms. There are 18,344 unique firm spellings, though there are fewer unique firm names because of misspelled names and abbreviations.

[28]In future iterations of this paper we plan on taking a random sample of companies with less than 100 voters in order to construct a more representative sample.

[29]We refer the reader to DiNardo and Lee (2004) for further details on this algorithm. That study relied heavily on establishment street address, which is unavailable here. Therefore, the spelling distance threshold was quite specific to that application. As a first pass, we modified the program to match only on firm name, and discovered that in this application, that same threshold leads to "too many" matches. As we describe below, we therefore augmented the process with a manual review process.

[30]For example, the algorithm determined that any company in the election file that had the word "American" as part of its name was a sufficiently good match for the company "American Enterprises" in the CRSP file, if there did not exist a better match. Therefore, a disparate set of companies like "American Laundry", "American Envelope", and "Pan American Screws" were all matched to "American Enterprise". All of these matches were dropped by our research assistants.

[31]Because there was an element of judgment, these exclusions were recorded in a log file for replication purposes.

Therefore, our final sample contains 6114 elections. This sample consists of 20% of all workers eligible to vote in elections.

In order to determine whether the matches appear reasonable, we compare the reported two-digit SIC industry code and state of the establishment from the election file, to the corresponding variables in the CRSP and Compustat files, for industry and state respectively. Because companies are diversified, the main SIC code for a company in the CRSP database need not be the same as the SIC code for a particular establishment in the NLRB election file. Similarly, an establishment may not be located in the same state as the company's headquarters. However, the comparisons are reassuring; the two digit SIC codes in the two data sets are the same for 50% of the matches, while 40% of the matches show the same state. For reference, if we randomly pair companies from the final NLRB data set to companies in the master names file that were ever matched to the NLRB data through our procedure, the corresponding match rate is 5% for industry and 4% for state.

Previous event studies of representation elections use samples of elections with a very large number of eligible voters. Ruback and Zimmerman (1984) and Bronars and Deere (1990) limit their sample to elections with at least 750 eligible voters. Elections of this size are quite rare, resulting in small sample sizes (54 union victories in the main sample of Ruback and Zimmerman (1984)). We believe that the effects of these elections are easier to detect if the number of eligible voters is large *relative* to the size of the firm. However, limiting the sample to large elections is neither necessary nor sufficient to achieve this objective. Because many of these elections take place in very large firms, the ratio of voters to total firm employment is no larger here than for moderately sized elections. While we do not have the exact sample used by Ruback and Zimmerman (1984), we can attempt to replicate it based on their description of the sample selection scheme.[32,33] Using their sample selection scheme we find that in more than 10% of the elections, less than 1% of the firm's workforce voted. In our reproduction of their sample, the median percentage of the workforce voting in an election is 5%.[34] By contrast, our main analysis limits the sample to elections where

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[32]We contacted Professors Ruback and Zimmerman to request their dataset. As their paper was published more than 20 years ago they understandably could no longer provide it.

[33]Using the Ruback and Zimmerman procedure we ended up with almost twice as many elections as they had considered over the same time period. The only information that Ruback and Zimmerman had that we do not is the petition date. They excluded elections where the petition date was unavailable. We therefore infer that this exclusion restriction would have resulted in us dropping 50% of the elections in the sample.

[34]Huth and MacDonald (1990) conduct an event-study of decertification elections. Their sample selection scheme involves all decertification elections involving at least 250 workers between June 1977 and May 1987. They also do not condition on there being a sufficiently high fraction of a firm's workers involved in the election. Our (inexact)

at *least* 5% of the total workforce voted.[35] The median election in our sample consists of 13% of the company's workforce voting (mean = 22%).[36] Therefore, our sample selection scheme not only provides us with elections that are relatively salient for a given firm (or, at a minimum, excludes those elections which are clearly not salient), but also yields a substantially larger sample size as compared to what we would have obtained using the Ruback and Zimmerman (1984) criterion. Our baseline sample is almost eight times larger than the Ruback and Zimmerman (1984) sample.

We present summary statistics of firm characteristics in Table 1. Columns (1) and (2) correspond to elections where at least 5% of the workforce voted (hereafter the "≥ 5% sample") for UV ("Union Victory") and UL ("Union Loss") firms respectively. Columns (3) and (4) correspond to elections where less than 5% of the workforce voted (hereafter the "< 5% sample") for UV and UL firms respectively. We report the market value of the firm using both the CRSP and the Compustat databases. Because there is a large number of missing observations in the Compustat database, especially before 1970, these measures differ. Companies in the Compustat database have larger market value on average, implying that small firms are underrepresented in the Compustat dataset.

Looking at the first row of Table 1, there are about twice as many elections in the < 5% sample than in the ≥ 5% sample, and in both samples there are about twice as many firms where the union lost than where the union won. Not surprisingly, firms in the ≥ 5% sample tend to be substantially smaller than firms in the < 5% sample. This inference can be made by comparing a variety of measures, including employment (4530 vs.73,223 employees) and market value ($338 million vs. $5.9 billion in 1998 dollars, using the more broadly available CRSP measure). However, the ≥ 5% corresponds to bigger elections, with an average of 453 workers voting as compared to an average of 291 in the < 5% sample.

In addition to the mean and standard deviation, for variables derived from the Compustat database we report in braces the average percentile rank of that variable relative to all other firms in the Compustat database for the year and quarter of election. The average percentile rank is convenient for assessing how the firms in our sample compare to companies in the Compustat universe, and is advantageous as a statistic that is "robust" to outliers. From the percentile

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reproduction of their sample has a median fraction of the workplace voting of 2%, with approximately 30% of elections in the sample involving less than 1% of the company's workforce.

[35]Total employment in the year of the election is from the Compustat annual files.

[36]We do not use elections where employment information is missing.

rankings it can be seen that firms in the $< 5\%$ sample tend to be around the 75th percentile in the size distribution of all Compustat companies whereas firms in the $\geq 5\%$ sample are, on average, in the 35th percentile. In both samples, firms tend to be fairly representative with respect to profit margins, return on assets, Tobin's Q, and the dividend ratio. At the time of the election, UL and UV firms appear to be similar in most measures, including employment, market value, profit margin, profit per employee, Tobin's average Q, and industry composition.

Table 1 also shows the delisting rate for companies. We report the fraction of companies delisted in the two years before or after the election. UV firms are slightly more likely to delist than UL firms (10% versus 8% delisting rates respectively).[37] While this difference is not large, we will consider several approaches to address this issue, as well as the presence of missing returns more generally. These approaches involve imputing missing returns, estimating all models excluding periods with missing returns, or limiting the sample to firms that have no missing returns in the event window. Simply excluding missing values has the disadvantage that some of the changes in cumulative returns over time may reflect firms that are entering or dropping out of the sample. Using a balanced panel has the advantage that we can be sure that any differences over time are not caused by compositional differences. However, a balanced panel does involve discarding a large number of elections, and implies that inclusion into the sample may depend on the realization of the dependent variable. We will demonstrate that the results are not sensitive to the approach considered.

4.2 The event study method

Our objective is to assess the impact of union elections on the stock market value of firms. Ideally, we would like to compare the firm's stock returns to the returns that the firm would have experienced in the absence of a union organizing event. The event-study method provides a framework for estimating this counterfactual return.

As is standard in the financial economics literature, we define the abnormal return as the difference between a stock's actual return and the expected return given market conditions. For the company corresponding to union representation election i, in month t, the abnormal return is:

$$\text{AR}_{it} \equiv r_{it} - E[r_{it}|X_t],$$

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[37]We define delisting as any company with a non-missing delisting return in the CRSP dataset.

where r_{it} is the actual return and $E[r_{it}|X_t]$ is the predicted return. For this study, r_{it} is the CRSP monthly holding-period return including distributions, which is constructed using prices that are adjusted for splits and distributions.[38]

For convenience, we express time in terms of months relative to the event:

$$\text{AR}_{i\tau} \equiv r_{i\tau} - E[r_{i\tau}|X_\tau],$$

where $\text{AR}_{i\tau}$ is the abnormal return of the security corresponding to election i in the τ'th month relative to the event.

Because returns of companies with unionization events may vary systematically before the elections, perhaps due to anticipation of the event, and because the market may not react instantaneously, we are interested in the cumulative abnormal return (CAR) in a window surrounding the election. The CAR corresponding to event i between months T_1 and T_2 relative to the event is:

$$\text{CAR}(T_1, T_2)_i \equiv \sum_{\tau=T_1}^{T_2} \text{AR}_{i\tau}.$$

The statistic of interest is the average (across N firms in the sample) cumulative abnormal return:

$$\text{ACAR}(T_1, T_2) \equiv \frac{1}{N} \sum_{i=1}^{N} \text{CAR}(T_1, T_2)_i.$$

We will present the average cumulative abnormal return for the set of union victory (UV) and union loss (UL) firms beginning two years prior to the election. Our decision to use such a long event window is in part the consequence of having information on the month that the NLRB closed the case, rather than the exact calendar date. By considering a very long pre-event window we can verify that any difference in the cumulative return of the UL and UV firms and any counterfactual (or "benchmark") portfolio is not simply a continuation of differential pre-event trends. The long panel also allows us to examine returns in the months beyond the event, so as to capture the long-run expected costs to the firm without having to rely on the assumption that the stock price immediately

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[38]When stocks are delisted we use CRSP delisting returns. We replace missing returns with the predicted return ($E[r_{it}|X_t]$) in order to mitigate survivorship bias, though the results are not sensitive to how missing values are treated. Specifically, the results are not sensitive to simply ignoring missing values, nor to only selecting companies that have no missing returns in the entire event-period.

and instantaneously adjusts to the presence of the union.

A critical decision in event-studies is how to model $E[r_{it}|X_t]$. In traditional short-run event-studies the counterfactual is often estimated from a market-model fit to historical data (as described, for example, in Campbell et al. (1997)). In this approach, denoting R_{mt} as the return of a broad market index in month t, one uses historical data to estimate:

$$E[r_{it}|X_t] = a_i + b_i R_{mt} \qquad (1)$$

This approach is theoretically attractive because the Capital Asset Pricing Model (CAPM) predicts that market beta is sufficient to describe cross-sectional expected returns. While this choice of benchmark is theoretically justified, a voluminous literature has discredited this idea (see Fama (1998) for a review), leading to the use of additional explanatory factors for the expected return. For example, it is common practice to include the company's size and the book equity-to-market equity ratio (BE/ME) (Fama and French, 1993; Carhart, 1997) in these market models.

As pointed out in the literature, however, there are a number of difficulties with estimating counterfactual returns using out-of-sample data in long-run event-studies. The approach requires that the estimated parameters remain time-invariant, an assumption that is known to not hold (Harvey, 1989). Additionally, estimation of the market-model parameters in the pre-event period must be done over an interval that is free of unusual pre-event returns, perhaps owing to (or even causing) the event. A solution to this problem is to estimate the market-model parameters using data that a long time perhaps several years, prior to the event. But doing so exacerbates the first problem – that the market-model parameters may have changed – and confront a new one: stocks that were not listed during the estimation window will be excluded from the analysis. As a result of these, and other concerns, the traditional methodologies developed for short-run studies are rarely used for long-run studies.

A common approach for computing abnormal returns in long-run event-studies involves the use of reference or "benchmark" portfolios matched on a firm's characteristics (see Barber and John D. Lyons (1997), Lyon et al. (1999), and Brav (2000)). The advantage of this approach is that the benchmark can be constructed in-sample, and because it allows for shocks that occur by chance that affect firms with similar characteristics. We employ this approach, matching every firm in our

sample to a portfolio of firms in the same size-decile.[39] As a probe for robustness we have also used the CRSP equally-weighted NYSE/AMEX/NASDAQ index as a benchmark, as well as firms both in the same size decile and in the same one-digit SIC industry.[40]

A second commonly used approach in long-run event studies is the calendar time portfolio (CTP) approach developed by Jaffe (1974) and Mandelker (1974) and advocated by Fama (1998). For each calendar month we compute the return of an equally-weighted portfolio of companies that unionized in the last T months, where T is 18 or 24 in our study. The return of this "unionization portfolio" is denoted R_{ut}, where u indicates that the portfolio consists of companies where workers voted for unionization, and t denotes the calendar month. The unionization portfolio is rolling, because companies with new unionization events are added in any given month, while those firms that have not had a unionization event within the last T months are dropped. The Fama-French three factor model (Fama and French, 1993) is used to compute the abnormal return of this portfolio:

$$R_{ut} - R_{ft} = \alpha_u + b_u(R_{Mt} - R_{ft}) + s_u SMB_t + h_u HML_t + \varepsilon_{ut}, \tag{2}$$

where R_{ft} is the one-month treasury bill rate, R_{Mt} is the monthly return on a value-weight market portfolio of NYSE, AMEX, and Nasdaq stocks, SMB is the difference in the returns on portfolios of small and big stocks (below or above the NYSE median), and HML is the difference in the returns of portfolios of high- and low-BE/ME stocks.[41] In practice, equation 2 is estimated weighting by the number of equities in the R_{Mt} portfolio at time t, as suggested by Fama (1998). Assuming that the broad-market return and the Fama-French factors adequately describe average returns, the parameter of interest, α_u, can be interpreted as the average abnormal return associated with holding this simulated portfolio.

The CTP methodology has been used in many long-run event-studies, for example by Loughran and Ritter (1995), Brav and Gompers (1997), Mitchell and Stafford (2000), and Greenstone et al. (2006). This approach is thought by some, including Fama (1998) and Mitchell and Stafford (2000),

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[39]CRSP produces indices for such purposes. Specifically, every year CRSP allocates companies into one of ten size deciles, based on market-value. The value-weighted average return of securities in these deciles are then calculated on a monthly basis. CRSP also produces a cross-walk that allows one to link each security to the appropriate size decile.

[40]We cannot match on the book equity-to-market equity ratio, as many studies do, because this variable is unavailable for a large number of companies in our sample, especially in the earlier periods of the sample.

[41]The three factors, R_{Mt}, SMB, and HML, were taken from Kenneth French's web page (http://mba.tuck.dartmouth.edu/pages/faculty/ ken.french/data_library.html). The web page contains additional information on the construction of these series.

to have better statistical properties than leading alternatives. For example, firms clustered in event-time can lead to over-stated test statistics in the matched-portfolio approach described above.[42] Because in the CTP methodology we use a time-series of portfolio returns, cross-correlations of firm abnormal returns are incorporated in the portfolio variance. Additionally, this approach allows for classical statistical inference because the distribution of this estimator is well-approximated by the normal distribution (Mitchell and Stafford, 2000). A disadvantage to this approach is that the market-model parameters of the portfolio are assumed constant. But because the model is estimated over a long time-period (1961-1999), and because the firms in the portfolio are changing, that assumption is unrealistic.

A complication that arises in both methodologies is how one defines the "event". The appropriate event is the date on which most the information on the probability of future unionization is incorporated. For much of the sample (1961-1976) we only observe the month that the NLRB closed the case. While we have a well-defined event, it is not the only relevant event, and it may not be the most important one. Alternative events that are potentially important are the petition and election dates. Using post-1977 data, where both the election and case closure calendar dates are available, we find that the median time between the election and NLRB case closure is 10 days. In some case, typically when one of the parties issues a challenge, this gap can be considerably longer. In five percent of the elections it took at least six months for the NLRB to close the case. While we do not have data on when the petition was submitted to the employer, it is known from (Roomkin and Block, 1981) that elections usually occur very soon after the petition. In their sample, 42 percent of elections occurred within 1 month of petition, and 83 percent within 2 months (Ruback and Zimmerman, 1984). Therefore, we do not believe that using month that the NLRB closed the election will present serious problems for estimation if most of the new information is being revealed at or after the petition date. To assess whether gradual diffusion of news led to abnormal returns prior to the closing date it is useful to examine a long pre-event window. We believe, however, that it will be difficult to empirically distinguish the market's anticipation of unionization from an inadequate comparison portfolio.

The event-study method can inform us on how the equity value of firms responds to certifica-

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[42]Though, it should be noted, we will allow for such correlations in computing standard errors by clustering on election and calendar month, using the formula from Cameron et al. (2006).

tion elections. We can also estimate event-study models for elections with varying degrees of union support in order to explore heterogeneity in the effect size. A more complete investigation of heterogeneity in the impact of certification election on stock market performance involves estimating the post-event cumulative abnormal return for every election and relating these to the vote share in a flexible way. We also conduct this analysis in order to examine the heterogeneity in the stock market reaction to election outcomes, and to determine whether there is a discontinuous relationship between cumulative abnormal returns and the vote share at the 50% threshold.

5   Empirical Results

5.1   Event-study estimates

In Figure 3 we plot the average cumulative return of union victory firms against the average cumulative return of the size-matched reference portfolios over the same time period.[43] The figure reveals that both UV firms and the corresponding reference-portfolios have almost identical trends in returns prior to the union victory. However, close to the time of the election there is a pronounced downward break in the returns of UV firms relative to the benchmark, persisting for approximately a year and a half. The average cumulative abnormal return implied by this divergence is approximately -10%.

In order to assess magnitudes and statistical significance of the effect implied by Figure 3, in Figure 4, Panel A we plot $\text{ACAR}(-24, \tau)$, for $\tau = -24$ through $\tau = 24$, with 95% point-wise confidence intervals. In Panel B we plot $\text{ACAR}(0, \tau)$, for $\tau = 0$ through $\tau = 24$. This second panel is relevant for assessing the overall effect size and for determining statistical significance. The figures show that the downward shift in abnormal returns emerging soon after NLRB case closure is statistically significant. We can reject that the average abnormal returns are equal to zero five months after the event at a 5% level of significance. We interpret the figure as providing evidence that union election wins correspond to large negative abnormal returns.

Figure 5 contains the plot of the average cumulative return for union loss firms against average cumulative return of the size-matched reference portfolios. As with the UV firms, the reference portfolio closely tracks the progression of UL firms prior to the election, but unlike UV firms, the

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[43]For convenience, we will often refer to the event month as the "election month", though it should be understood that we actually only know when the NLRB closed the case.

returns of UL firms do not diverge from the benchmark after NLRB case closure. If anything, there is a moderate increase in the cumulative return of UL firms relative to the benchmark, though in Figure 6, which presents the difference in these series with confidence bands, we can see that this increase is not statistically significant at conventional levels.

We have conducted a variety of analyses to determine whether the patterns seen in Figure 3 and Figure 5 are robust. These analyses include not imputing missing returns (Appendix Figures 1 and 7), by using a balanced panel (Appendix Figures 2 and 8), excluding elections where following case closure cumulative abnormal returns are less than or equal to the 5th percentile or greater than or equal to the 95th percentile of all cumulative returns (Appendix Figures 3 and 9), using a four year pre-event window (Appendix Figures 4 and 10), using of an industry×size matched-reference portfolio (Appendix Figures 5 and 11), and using the CRSP equally-weighted market index as the reference portfolio (Appendix Figures 6 and 12). In all cases the overall pattern of cumulative returns look very similar to those seen in Figures 3 and 5.[44]

Our sample selection scheme was in part predicated on selecting elections for which a sizable fraction of the firm's workforce was voting; in practice we used a 5% cutoff. As an additional falsification check we examine elections where a small fraction of the firm's total workforce voted. In Panel A of Figure 7 we plot the average cumulative abnormal return of these small elections (relative to the size of the firm), both for UV and for UL firms. While both sets of firms appear to trend upward over the entire event-period, we do not observe a break in the trend in either series. Small-election UV firms appear to exhibit slightly larger positive abnormal returns relative to the small-election UL firms over the entire event-window, but this gap can be explained by the different time-periods in which the elections are occurring; when we weight the union victory sample so as to balance the distribution of the year of election in both samples, the two series line up almost perfectly (Panel B).[45]

Table 2, Panel A presents average cumulative abnormal returns following union victories. The first column corresponds to the use of the size-matched benchmark. Column (2) corresponds to

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[44]A possible exception is Appendix Figure 10, which shows that UL firms experienced a period of positive abnormal returns three years before the election.

[45]The weight for year j is $\omega_j = \frac{P_{jL}}{P_{jV}}$, where P_{jV} is the percent of union victory firms in year j, and P_{jL} is the percent of union loss firms in year j. To calculate the year-weighted average cumulative abnormal return ($WACAR$) in the UV sample, we use the formula: $WACAR(0, 24) \equiv \sum_{i=1}^{N_V} \omega_{ij} \left\{ CAR(0, 24)_i \right\} / \sum_{i=1}^{N_V} \omega_{ij}$, where N_V is the number of union victory firms, and ω_{ij} is the weight corresponding observation i's election year.

the industry ×size matched-benchmark. Column (3) corresponds to the CRSP equally-weighted NYSE/AMEX/NASDAQ index benchmark. In the first row of Panel A we report ACAR(0,24) for each of the three benchmarks. The estimated post-election average cumulative abnormal returns range from -9% to -10%, and are significant at the 1% level. To gauge magnitudes we calculate that a 10% negative return corresponds to approximately $20 million in lost market value (in 1998 dollars), or $40,522 per worker who is eligible to vote. This appears to be a plausible value. For example, if the average income of workers prior to certification is $35,000, then our magnitudes imply approximately a 9% union wage premium.

In the second row of Table 2 we report ACAR(-24,-4), that is, the average cumulative abnormal return prior to case closure, excluding the three months immediately preceding the event. ACAR(-24,-4) is statistically indistinguishable from 0 in all three specifications. The lack of significant abnormal returns prior to the election indicates that the market did not anticipate these events, on average, and also suggests that all three benchmarks do a reasonable job of predicting average returns of the portfolio of UV firms. In the third row we compute ACAR(0,24) after adjusting abnormal returns in the post-event period for the equity-specific trends in abnormal returns in the months before the election. Specifically, before computing ACAR(0,24) we subtract off the average abnormal return for the equity in months -24 through -7 relative to case closure from the post-event abnormal return. The point estimates are very close to the unadjusted version. But, not surprisingly, they are somewhat less precise. Table 2, Panel B reports the same set of estimates for union loss firms. Consistent with what we observe in Figure 4, the cumulative abnormal returns are close to zero and statistically insignificant.

In Table 3 we present the estimates from the calendar time event-study methodology described in Section 4. The portfolio of stocks consisting of all firms with a unionization win in the previous 24 months has a precisely estimated estimated alpha of -0.005 (t-ratio=-3.6). In the second row we consider a hypothetical portfolio of firms that are purchased two years prior to case closure, and are sold four months prior to case closure (-24 to -4 months relative to closure). This portfolio corresponds to a small and statistically insignificant alpha. Likewise, we do not observe an economically or statistically significant alpha for portfolios of firms recently experiencing union losses (Table 3, Panel B), nor for portfolios consisting of firms with small elections relative to the size of the company (Table 3, Panels C and D). These results give us confidence in our finding; negative

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alphas only appear when the union wins, and then only when the electorate is a large fraction of the total firm's workforce. Moreover, the results are robust to the use of two standard methodologies for long-run event studies.

5.2   Discussion of the Results and Additional Analyses

Speed of Adjustment

Perhaps a surprising feature of Figure 4 is that, while the efficient market hypothesis would predict that the entire unionization effect should be fully realized by the time of the election, we instead see an effect which grows over a longer period, with an abnormal return that begins around the time of election and persists for approximately 15 months. Ours is not the first study showing that markets under-react to seemingly important events. Systematic under-reactions have been reported in response to IPOs and SEOs (Loughran and Ritter, 1995), mergers (Asquith, 1983; Mitchell and Stafford, 2000), stock splits (Ikenberry et al., 1996), share repurchases (Mitchell and Stafford, 2000), exchange listings (Dharan and Ikenberry, 1995), dividend initiations (Michaely et al., 1995), spin-offs (Cusatis et al., 1993), earnings announcements (Ball and Brown, 1968), and predictable changes in demographics (Dellavigna and Pollet, forthcoming). While Fama (1998) questions the robustness of some of these findings, even he acknowledges that the short-term continuation of returns documented by Jegadeesh and Titman (1993) is "an open puzzle", and that the slow post-earnings announcement drift "has survived robustness checks, including extension to more recent data."[46]

The gradual-adjustment to an union election win is perhaps less surprising than some of these other events because the firms in our sample are typically small, and also because large unionization events are relatively rare for a given firm. For example, one explanation for what we observe is that news only gradually filtered to investors. According to I/B/E/S International analyst data, only 50% of these companies had analyst coverage at the time of the election, meaning that these elections may not have been widely publicized or followed.[47] To further explore this possibility, in Figure 8 we compare average cumulative abnormal returns for companies that did and did not have analyst coverage at the time of the election. Companies with analyst coverage appear to have experienced negative abnormal returns earlier than those without analyst coverage. But even these experience a

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[46]Quoted in Fama (1998).
[47]The 50% figure is derived from I/B/E/S International analyst data for years 1976-1999.

relatively slow-reaction to the event, on average, suggesting that the lack of analyst coverage is not the complete story.[48]

Another explanation for the pattern of results is that the unionization events were known, but investors or analysts may not have immediately understood how to revalue the company with this information. Large unionization events are relatively rare–affecting any given company only a small number of times (otherwise they would not be large elections)–and the terms of the first contract are uncertain. In fact, approximately 25% of successful NLRB elections do not lead to a successful first contract Cooke (1985). Consistent with this interpretation, in Section 5.3 we will see that union wins are associated with a trend break in the growth rate of these companies, as measured by assets, shareholder equity, and profits, an effect that that may not have been immediately obvious to investors, but which may have become apparent over time.

Evolution of the unionization effect over time

Next we turn to the evolution of the effect over time. The DiNardo and Lee (2004) sample includes elections beginning in 1984. It is possible that unions no longer affected firm performance in this latter period, while in earlier years the effects may have been more pronounced. In Figure 9 we compare the average cumulative abnormal return of UV firms for elections occurring in the 1961-1983 period to those occurring in the 1984-1999 period. The figure indicates that the average effect of a union certification win on firm performance has remained fairly stable over time. Therefore, we do not believe that the lack of an estimated unionization effect in DiNardo and Lee (2004) is due to their sample frame.

A comparison of states with and without right-to-work laws

Twenty-two states have what are known as "right-to-work" (RTW) laws. These laws typically mandate that payment of union dues cannot be made a condition of employment, thus weakening unions through possible free-riding. In Figure 10 we compare average cumulative abnormal returns in states with and without right-to-work laws. Interestingly, conditional on a union winning its

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[48]We are cognizant that companies not showing up in I/B/E/S may still have analyst coverage. This kind of misclassification will tend to reduce the measured difference in excess returns between these two groups of firms, if in fact there are real differences. This measurement problem will likely not affect the relatively slow speed of adjustment for companies covered by analysts, as these are presumably measured correctly, meaning that our basic conclusion–that analyst-covered companies exhibit a relatively slow speed of adjustment–still holds.

election, the stock-market effects of unionization tend to be more pronounced in states with right-to-work laws than those without. This finding does not mean that states with right-to-work laws are more favorable to unions, as these firms differ in other dimensions, and also because these laws (or the business climate more generally) may affect the likelihood that a union organizes, as well as the likelihood that a union wins an election. But the result does call into question whether right-to-work laws fundamentally weaken unions because of a potential free-riding problem.[49] This finding lends qualified support to conclusion of Farber (1984) and Moore and Newman (1985) conclusion that RTW laws are primarily symbolic, reflecting a taste against union representation rather than having any real effect.

5.3 Compustat analysis

The results presented up to this point suggest that union victories are associated with negative abnormal returns. We complement this analysis with an additional investigation using variables from the balance sheets and income statements of these firms. Using quarterly data from Compustat, we examine whether shareholder equity, assets, total liabilities/total assets (a measure of leverage), plant, property and equipment, sales, the dividend ratio, Tobin's average Q, profit margin, and return on assets are affected by the outcome of representation elections. We compute the average value of these variables (logged when appropriate) over the 12 quarters before and after the event date, comparing UV and UL firms.[50] As before, we assess whether or not these series were trending differentially prior to the event, and whether their trend breaks around the time of the event.

Unfortunately, the early part of the sample period is unusable in the Compustat analysis because many of these variables were not reported until the late 1960's, and not universally until the early 1970's. Moreover, the fraction of observations that are missing is substantially higher in the Compustat dataset than in the CRSP dataset. As a result, for this analysis we will only consider elections over the 1973-1999 period. In order to mitigate composition bias to due to unbalanced panels we demean the variables, but we do not drop elections with missing values.

In the nine panels of Figure 11 we plot averages of these de-meaned variables over event-time, in

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[49]It is possible that unions that organize in right-to-work states are those with substantially more solidarity amongst the workers than those without these laws, through selection. Therefore, conditional on organizing a campaign and winning, these unions tend to be more effective.

[50]All variables in dollar units are adjusted to reflect dollar values in 1998.

each case comparing elections where the union won to those where the union lost. Figure 12 plots the difference in these series with 95% pointwise confidence intervals.[51]. The figures show that the time pattern of variables that proxy for "size" are consistent with the pattern in equity value. UV firms display a downward break in trend in total assets (Panel A), shareholder equity (Panel B), and sales (Panel C) near or just before certification. The reduction in asset growth is coming about in large part to reduced growth of plant, property, and equipment (Panel D).[52] Because of small sample sizes, these series are not as well-behaved as those for equity values, though they have a similar pattern. We see little effect of union wins on a measure of leverage, defined as long-term debt divided by total assets (Panel E). This last finding can be viewed as circumstantial evidence that companies are not using leverage strategically to influence bargaining negotiations, at least in this sample.[53]

The marked reduction in the growth rate of assets is notable because if unionization is increasing the price of labor, there should be substitution from labor to capital (though, as seen in Panel F, Tobin's average Q appears stable). The fact that assets are actually declining implies that the "scale" effect from reduced reinvestment dominates this possible substitution effect. The time pattern of these variables also sheds light on the seemingly slow reaction of investors to unionization events that we see in Figure 3. The pattern of abnormal returns mirrors the time-pattern we observe in shareholder equity, assets, sales and pre-tax income. The evidence is consistent with stock market pricing in the effect of unionization only after changes in these variables become known.

While the reduced relative size of the UV firms is associated with lower pretax income (Panel G), variables that proxy for operating performance, for example return on assets and profit margins, appear stable.[54] At first blush, the finding that companies that undergo unionization experience lower growth rates but stable return on assets and profit margins may seem puzzling. But if firms only select projects that are sufficiently profitable, and unionization reduces the number of these high net present value (NPV) projects, then it is possible for the company's growth rate to decline

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[51]Standard errors are clustered on election.

[52]We have also examined the corresponding figures using a balanced panel. The overall patterns are the same as when using the unbalanced sample, but because we lose so many elections the confidence intervals are substantially wider.

[53]Bronars and Deere (1991) shows that there is a positive association between financial leverage and unionization in the cross-section. Matsa (2006) provides evidence that firm measures of leverage were affected by state-level changes in right-to-work laws.

[54]The profit margin in UV firms appears to decline a bit relative to UL firms, but not until about 7 quarters after the election (Panel I).

in spite of experiencing no change in its operating performance.

In Table 4 we present difference-in-difference estimates for the effect of a union victory relative to a union loss on each of the six aforementioned variables. The sample consists of election × event-time observations. We regress each of the (non-demeaned) variables on election fixed-effects, an indicator for whether the NLRB closed the election on or after the given quarter ("post"), and the interaction of "post" with an indicator for whether the union won the election ("post × union win."). The point estimates suggest that assets, shareholder equity, and sales fall by approximately 10% in UV firms after the election, relative to UL firms. Pre-tax profits of UV firms are approximately 17% lower in the post-election period relative to the pre-election period, relative to UL firms. These statistically significant estimates are consistent with the 10-14% negative abnormal returns we observe in equities.

5.4   Heterogeneous impacts of unionization

In view of the findings summarized in the preceding discussion, a natural question comes to mind: how can these large effects be consistent with the substantially smaller ones found in DiNardo and Lee (2004)? This sections aims at providing a partial answer to this question.

Whereas DiNardo and Lee (2004) identify the "unionization effect" by focusing an implicit comparison of winning and losing establishments among close elections, the discussion in Section 3 suggests that we can learn about how unions affect firms by examining the heterogeneity in the effects of unionization at all points in the vote share distribution. This analysis is possible because of the long-panel structure that we have at our disposal.

We begin by relating the security-level cumulative abnormal return in the two years following the election, to the union vote share. Specifically, we are interested in the shape of $E[\text{CAR}(0, 24)_i | v_i]$, where v_i denotes the union vote share in election i. We graphically plot this functions by: (1) averaging $\text{CAR}(0, 24)_i$ over 20 equally-spaced vote share bins,[55] and (2) plotting the predicted values from the model $E[\text{CAR}(0, 24)_i | v_i] = p(v_i) + \beta 1(v_i > 0.5)$, where $p(\cdot)$ denotes a sixth-order polynomial, and $1(v_i > 0.5)$ is an indicator function for whether the union vote share in a given election exceeded 50%. Figure 14 presents estimates of $E[\text{CAR}(0, 24)_i | v_i]$ using both of these approaches. (For reference, Figure 13 shows the histogram of the union vote share variable.)

Figure 14 shows clear evidence that the effect of a certification election is heterogeneous, and

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[55]See DiNardo and Lee (2004) for a description of construction of these 20 equally spaced bins.

that it depends on the union vote share. As in the Dinardo and Lee study, there is no discernible discontinuity in the $E[\mathrm{CAR}(0, 24)_i | v_i]$ at the 50% union vote share threshold. In fact, the estimated discontinuity is somewhat perverse: firms with close union wins experience elevated post-election cumulative returns vis-a-vis firms with close union losses. On the other had, union victories with higher union vote share correspond to negative excess returns, and the negative impact of a union election win appears to become markedly more pronounced when the union has a higher vote share. Greater than 60% union vote share is associated with 20-30 percent negative cumulative abnormal returns.

Firms with union losses also exhibit a downward sloping relationship between abnormal returns and vote share. Much of the decline appears to occur at the largest vote shares, but there is also more variability in the predicted cumulative abnormal returns due to small sample sizes. Close union losses are associated with marginally-significant negative abnormal returns, though as we will show these declines can be explained by a small amount of pre-election trending in the abnormal returns.

We now turn to several robustness checks. In Figure 15 we overlay the predicted CAR in months 0 through 24 (shown in Figure 14) with the predicted CAR computed over event-months -24 through -4. The figure shows that the gradient in CAR by vote share, seen for months 0 to 24, is not present for months -24 through -4. This plot reassures us that the negative CAR observed for higher union vote shares is not a continuation of a pre-event trend.

In order to address the issue of pre-event trends more completely, we consider an additional analysis where we adjust abnormal returns in the post-event period for possible pre-event trends. Specifically, we calculate the cumulative abnormal return in the post-event period deviated from the average abnormal return in the pre-event period (from months -24 to -7 relative to case closure):

$$\text{adjusted-AR}_{i\tau} \equiv AR_{i\tau} - \frac{1}{18} \sum_{\tau=-24}^{-7} AR_{i\tau}$$

We then calculate:

$$\text{adjusted-CAR}(0, 24)_i \equiv \sum_{\tau=0}^{24} \text{adjusted-AR}_{i\tau}.$$

Figure 16 plots the predicted adjusted-CAR with 95% pointwise confidence intervals.[56] The figure

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[56]As before, the abnormal returns from the pre-event period are calculated using an estimation window that ends 29 months prior to the closing month. The abnormal returns from the post-event period are calculated using an

shows virtually the same pattern of heterogeneity seen in the earlier figures, though with a wider confidence intervals. The main difference between the pattern in this figure and Figure 14 is that there is weaker evidence here of a negative CAR amongst firms with close union losses.

In Table 5 we conduct formal statistical inference. Using the same sample of 1436 elections used to construct Figures 14-15, in column (1) we regress CAR(0,24) on a dummy for whether the union won the election. Consistent with earlier analyses, we find that union wins are associated with cumulative abnormal returns that are 12.1 percentage points lower than firms with union losses (t-ratio = -3.5). In column (2) we add the union vote share as a covariate.[57] The introduction of this variable alone is enough to change the sign on the coefficient on the union win dummy, resulting in a union effect of 0.048 (t-ratio = 0.89). Adding higher-order polynomial terms in the vote share (column 3) only makes the estimated union win coefficient more positive; the "regression discontinuity" estimate of a union win is 8 percentage points, but is statistically indistinguishable from 0. In column (4) we examine whether the negative gradient between CAR and the vote share differs among elections where the union won and lost. Specifically, we regress CAR(0,24) on a union win indicator, the vote share, and the vote share interacted with the win indicator. The interaction term is statistically insignificant in all specifications.

In columns (5)-(8) we estimate the same set of models using CAR(-24,-4) as the dependent variable. None of the patterns observed when using CAR(0,24) as the dependent variable are evident here. In columns (9)-(12) we re-estimate these models using adjusted-CAR(0,24) as the dependent variable. The point estimates in this set of specifications are very close to the ones obtained using CAR(0,24), but are estimated less precisely, with standard errors approximately 50% larger than those in columns (1)-(4).

5.5 Discussion

In order to facilitate interpretation of these results, we revisit the theoretical framework outlined above. Using the notation from the model, the point estimates imply that B_1 and B_3 are small, while B_2 is relatively large. As discussed in Section 3, $B_1 \approx 0$ implies that the firm is largely unresponsive to the election, or to the preferences of the median voter. This result is consistent with

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estimation window that ends 5 months prior to the closing month.

[57]Vote share is grouped into one of 20 equally spaced bins, ranging from 0 to 1. We transform this variable in order to avoid the "integer" problem described in DiNardo and Lee (2004).

firms complying with NLRA rules that prohibit management from changing working conditions in order to gain votes in an NLRB election.

The limited responsiveness on the part of the firm helps us pin down the parameters governing the behavior of the union. Figure 17 graphically shows the circumstances under which there could be a large average effect of unionization, while at the same time there be a limited difference in the effects of close union victories relative to close union losses. Because the firm does not adjust its offer in relation to worker preferences, it is only possible to have a close election when workers have low tolerance for high wages, and when the union's proposal is small. Specifically, as can be seen, a small discontinuity ($B_3 \approx 0$) implies a small α. Therefore, when workers have limited tolerance for higher wages, unions tend to make very modest proposals. Also evident from Figure 17 is that we must select a small β so as to make the discontinuity small. Provided that there are some elections with sufficiently large μ (corresponding to large y_U), we can preserve a large average unionization effect.

A small β implies that unions only partially adjust their proposals to accommodate the underlying preferences of the workers. In particular, unions will make larger, but increasingly relatively conservative offers as the median worker's preferred wage increases. Such behavior suggests that as μ becomes larger, the enacted wage is below the preferred wage for an increasingly larger share of workers. As Figure 17 demonstrates, the pattern of results is consistent with the idea that unions generally offer proposals that are lower than the preference of the median voter over most values of μ. Such behavior is consistent with the idea that unions place a high premium on winning elections, even at a cost of lower wages.

The results also have implications for the relationship between unionization and outcomes that we have not considered in this paper. For example, it is possible to view the preference of the median voter as related to the establishment's elasticity of labor demand, because preferences for higher compensation could correspond to establishments with relatively inelastic labor demand. If this is the case, then our results suggest that new unionization should lead to relatively limited employment effects, even when the firm experiences considerable equity loss from new unionization. This is because unionization leads to large profit impacts only when labor demand is inelastic. We should not expect to see plant closure following unionization, as the median voter would necessarily be adversely impacted by that outcome.

In light of the model, the histogram of the union vote share presented in Figure 13 provides additional information about the underlying distribution of preferences. The histogram shows that there are more union losses than wins. Moreover, the vote share distribution is bell shaped, indicating that large union wins or defeats are relatively infrequent. If we think about the union vote share distribution as an endogenous outcome of the model described in Section 3, it must be the case that most elections correspond to small μ–below $\widetilde{\mu}$ in Figure 17. Furthermore, since there are not many elections where the union wins overwhelmingly, there can only be a relatively small number of elections with very large μ. If we again relate preferences to the elasticity of labor demand, the model therefore suggests that most establishments undergoing elections have relatively elastic labor demand. As already suggested, it is the relatively few cases where demand is inelastic that lead to the large average effects of new unionization on equity value.

Of course, there are important caveats to be made with these inferences. First, while the estimated RD estimate is close to zero (and positive), it is measured with some imprecision. The RD estimates do not allow us to rule out cumulative abnormal returns between -4% and +21%. It is true, however, that the negative effects of unionization are largely driven by elections with more extreme vote shares. A second caveat is that the model outlined above is not the only one that can explain the pattern in the results. For example, it may be the case that unions need to have widespread support among the workers in order to be effective, perhaps because it is the only way that a strike threat would be credible.

6 Conclusion

The economic effects of unions on the labor market and the economy has been a longstanding area of interest for economists. The literature has considered the impact of unions on wages, their potential role as monopolies, their role in work stoppages, their effect on the aggregate economy, as well as the question of why they can even exist and survive in a competitive labor market. In order to even partially address many of these questions, we must first understand how unions affect firms.

We began by asking whether the case of National Linen Services was the rule or the exception. In one respect, it is the rule. We have shown that new unionization is associated with a reduction in the firm's market value totaling approximately $40,500 per worker eligible to vote. This finding

is robust to the use of a variety of specifications, and to the use of several different methodologies. The negative effects of unionization on the equity value of firms appears fairly stable over time, showing no major differences before or after 1984. An examination of the balance sheets and income statements of both sets of firms reveals that union wins are associated with relatively lower growth, though there is little evidence to suggest that these firms experienced lower return on assets or profit margins as compared to firms with union losses. The evidence is therefore consistent with the claim that unionization reduces the number of sufficiently positive NPV projects available to a firm.

But when viewed in isolation, the NLS case misses what turns out to be important heterogeneity in the stock market reaction to recognition elections. Using a different sample from DiNardo and Lee (2004), we also find RD estimates that imply that unionization is largely ineffective, at least to the extent that they do not affect a firm's equity value. This finding can be reconciled with the findings from the event-study analysis through the negative gradient in abnormal returns in relation to the union vote share. There is smoothness in the predicted abnormal returns about the 50% vote share threshold because more overwhelming union victories are associated with worse financial performance relative to more contested union wins. These results are consistent with firms that do not respond to the threat of an election, the existence of a limited number of elections where workers demand substantially higher wages, and unions that place a high premium on winning elections.

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Figure 1: Cumulative stock market returns surrounding National Linen Service's 1999 representation election
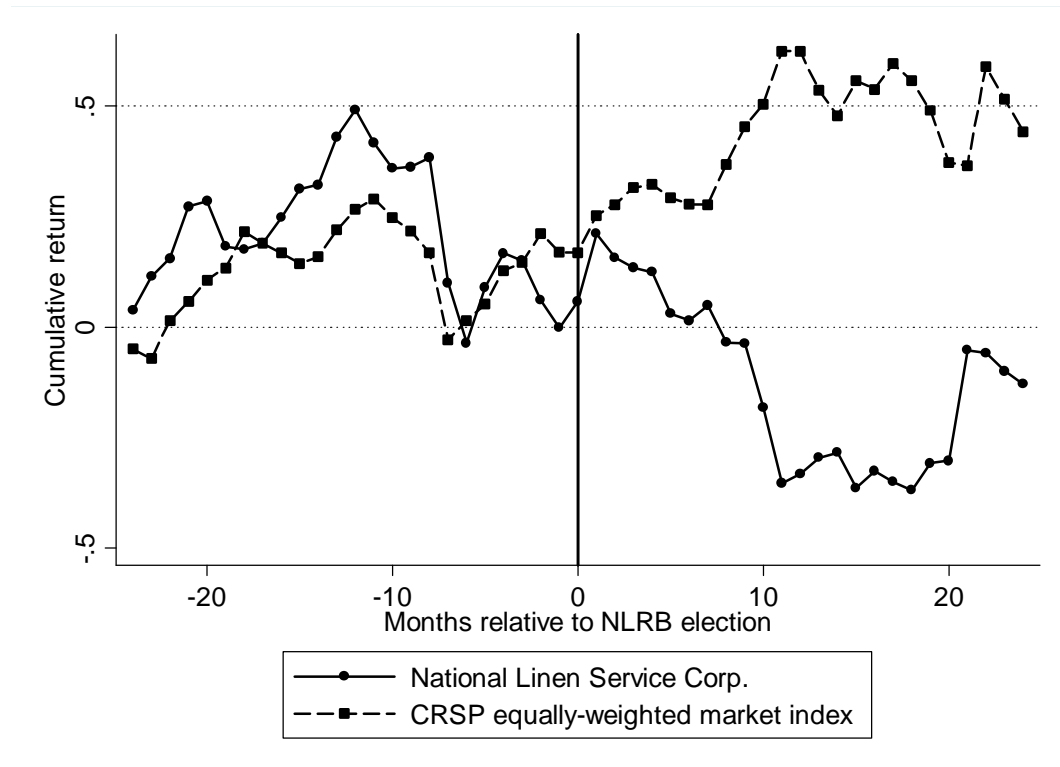
Figure 2: Union and firm offers relative to median voter preferences
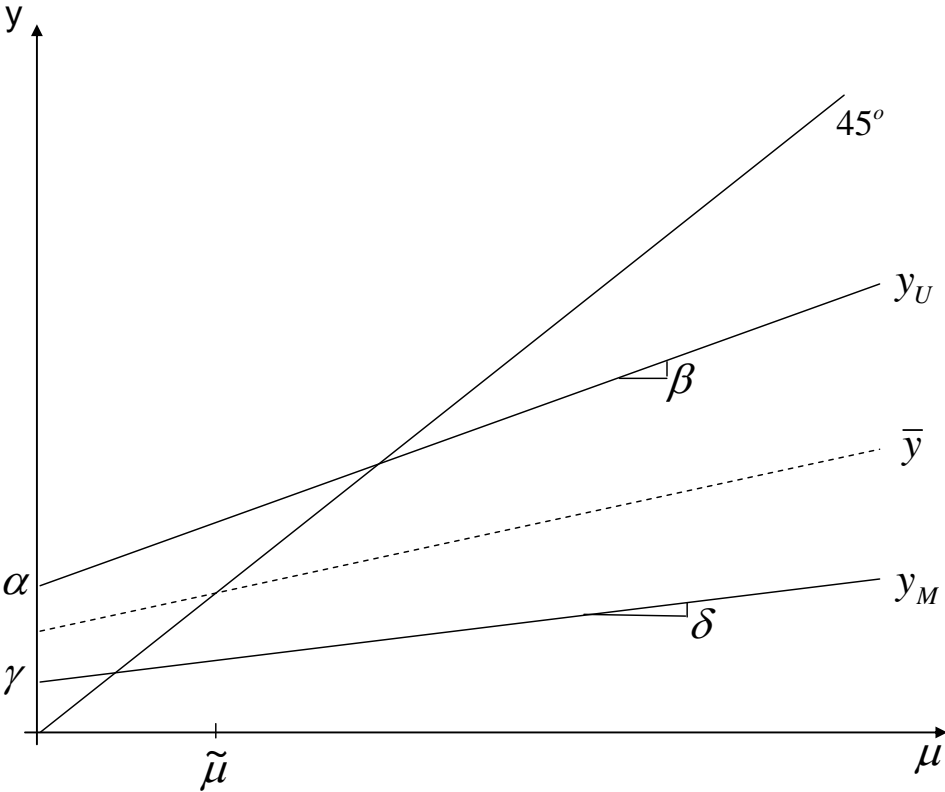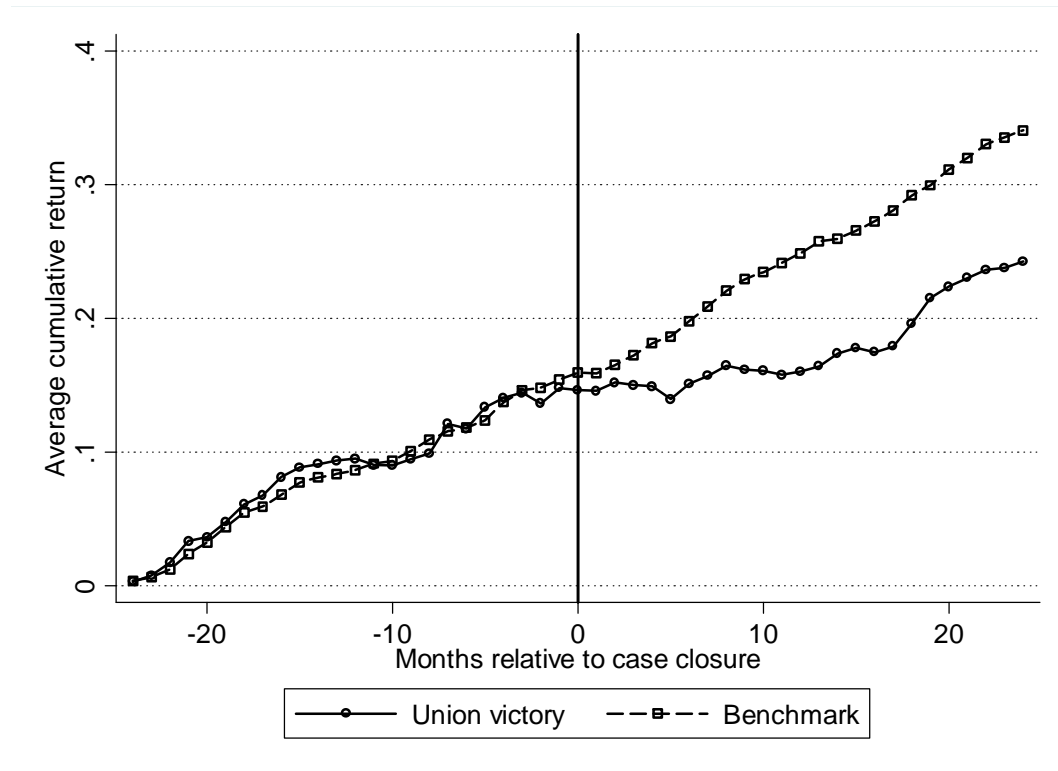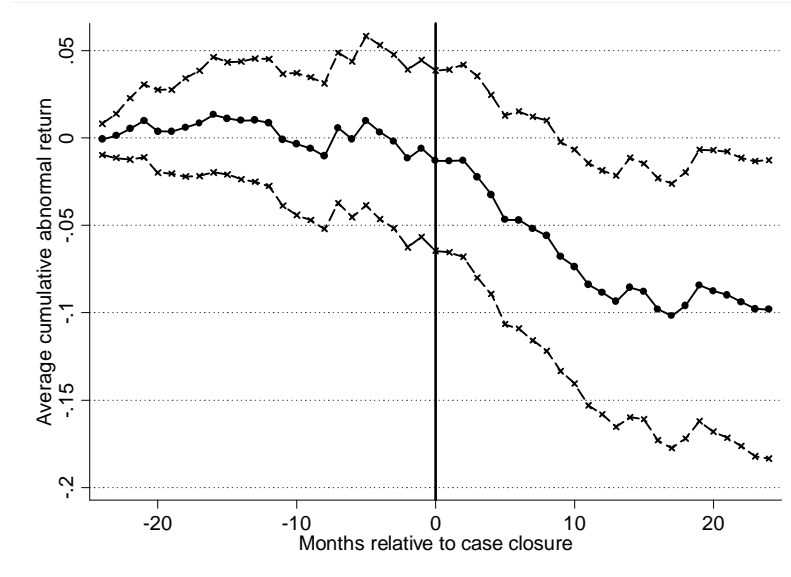
Figure 3: Average cumulative returns of union victory firms and of the size-matched reference portfolio, by month relative to NLRB case closure


Note: Union victory firms consist of publicly traded companies holding representation elections where at least 5% of the company's workforce voted, and where the union won. Each point is the average cumulative return up to the month relative to case closure, beginning 24 months prior to case closure. Each firm in the sample is associated with a benchmark portfolio matched on size. The benchmark series corresponds to the average cumulative return of these size-matched reference portfolios. Returns are expressed net of the risk-free rate.

Figure 4: Average cumulative abnormal return of union victory firms, by month relative to NLRB case closure

Panel A: Beginning 24 months prior to case closure

**Panel B: Beginning month of case closure**
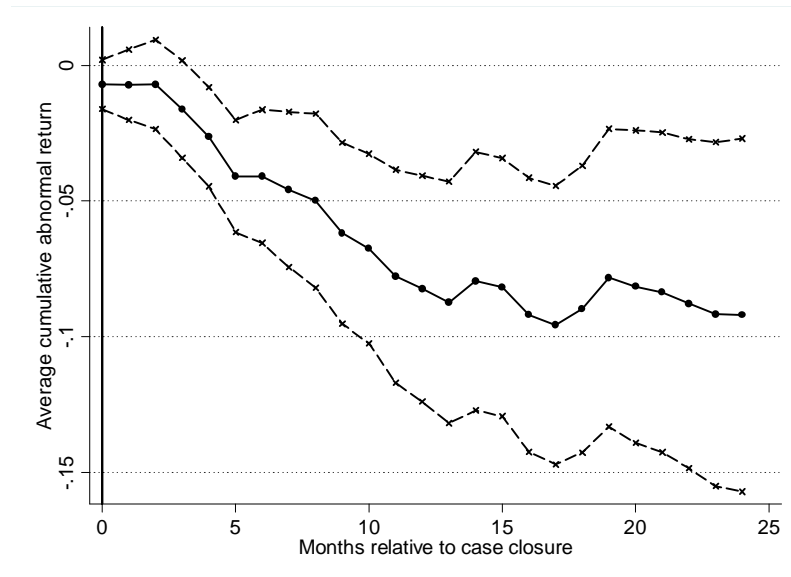
Notes: Both panels show the difference in the average cumulative return of union victory firms and the size-matched reference portfolio, as shown in Figure 3. Panel A corresponds to the average cumulative abnormal return computed beginning 24 months prior to case closure. Panel B corresponds to the average cumulative abnormal return computed beginning in the month of case closure. The dashed lines represent the 95% confidence intervals, which are computed using standard errors clustered on elections and calendar months. We use the formula in Cameron, Gelbach, and Miller (2006) to compute standard errors with multi-way clustering.

Figure 5: Average cumulative returns of union loss firms and of the size-matched reference portfolio, by month relative to NLRB case closure
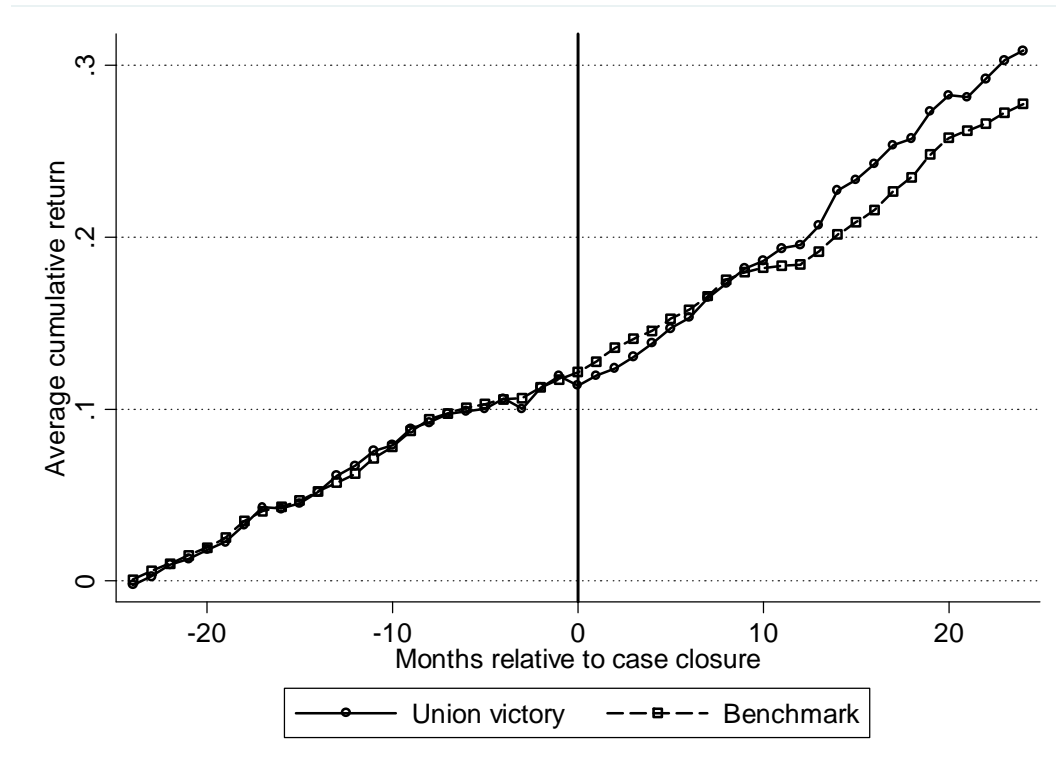
Note: Union loss firms consist of publicly traded companies holding representation elections where at least 5% of the company's workforce voted, and where the union lost. Each point is the average cumulative return up to the month relative to case closure, beginning 24 months prior to case closure. Each firm in the sample is associated with a benchmark portfolio matched on size. The benchmark series corresponds to the average cumulative return of these size-matched reference portfolios. Returns are expressed net of the risk-free rate.

Figure 6: Average cumulative abnormal returns of union loss firms, by month relative to case closure
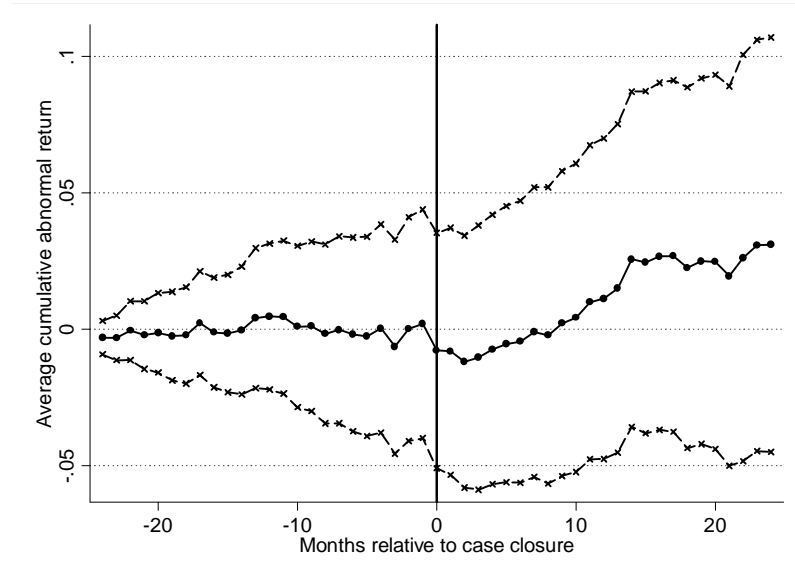
Panel A: Beginning 24 months prior to case closure

Panel B: Beginning month of case closure
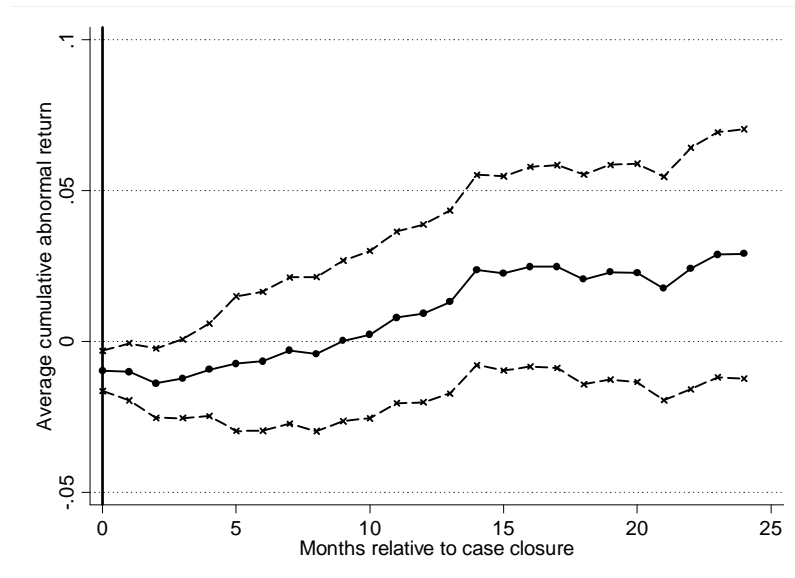
Notes: Both panels show the difference in the average cumulative return of the union loss portfolio and the size-matched reference portfolio, shown in Figure 5. Panel A corresponds to the average cumulative abnormal return computed beginning 24 months prior to case closure. Panel B corresponds to the average cumulative abnormal return computed beginning at the month of case closure. The dashed lines represent the 95% confidence intervals, which are computed using standard errors clustered on elections and calendar months. We use the formula in Cameron, Gelbach, and Miller (2006) to compute standard errors with multi-way clustering.

Figure 7: Average cumulative abnormal returns of union victory and union loss firms where less than 5% of the workforce voted, by month relative to case closure

Panel A: Unweighted
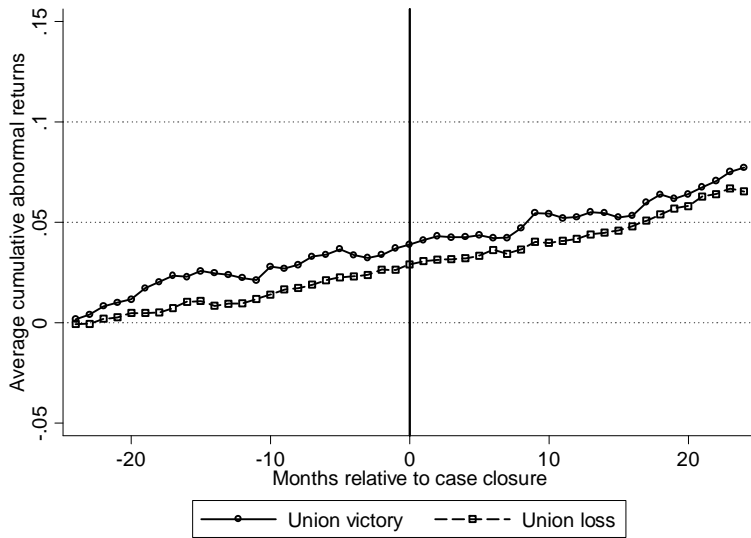
Panel B: Weighted
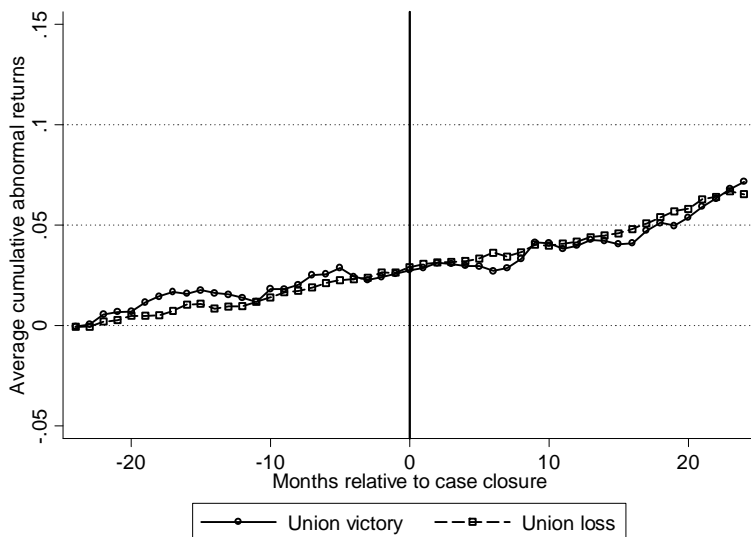
Note: The sample consists of publicly traded companies where less than 5% of the workforce voted. Returns are net of the risk-free rate. In Panel B the union victory firms are weighted so that the year of election amongst these firms mirrors the year of election of union loss firms. See Section 5.1 for additional details.

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Figure 8: Average cumulative abnormal return, by analyst coverage
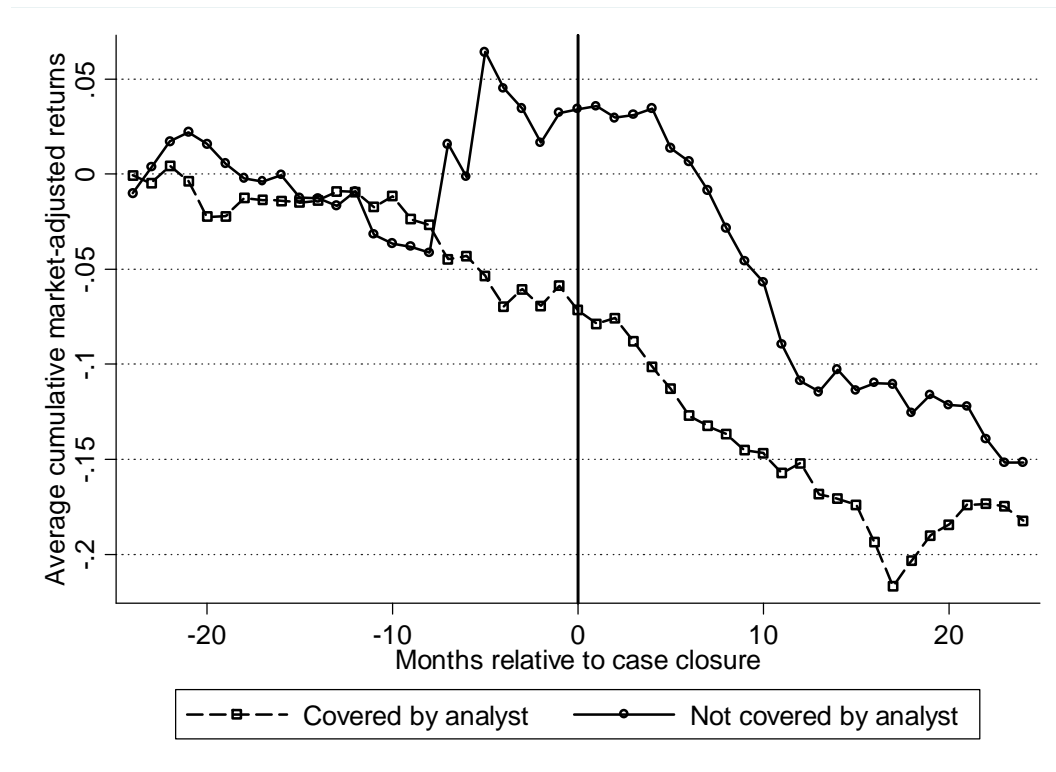
Note: A company is considered to have analyst coverage if it appears in the I/B/E/S dataset in the year of the election. The sample is limited to elections occurring in years where I/B/E/S data were available, between 1976 and 1999

Figure 9: Average cumulative abnormal return, by time period of election

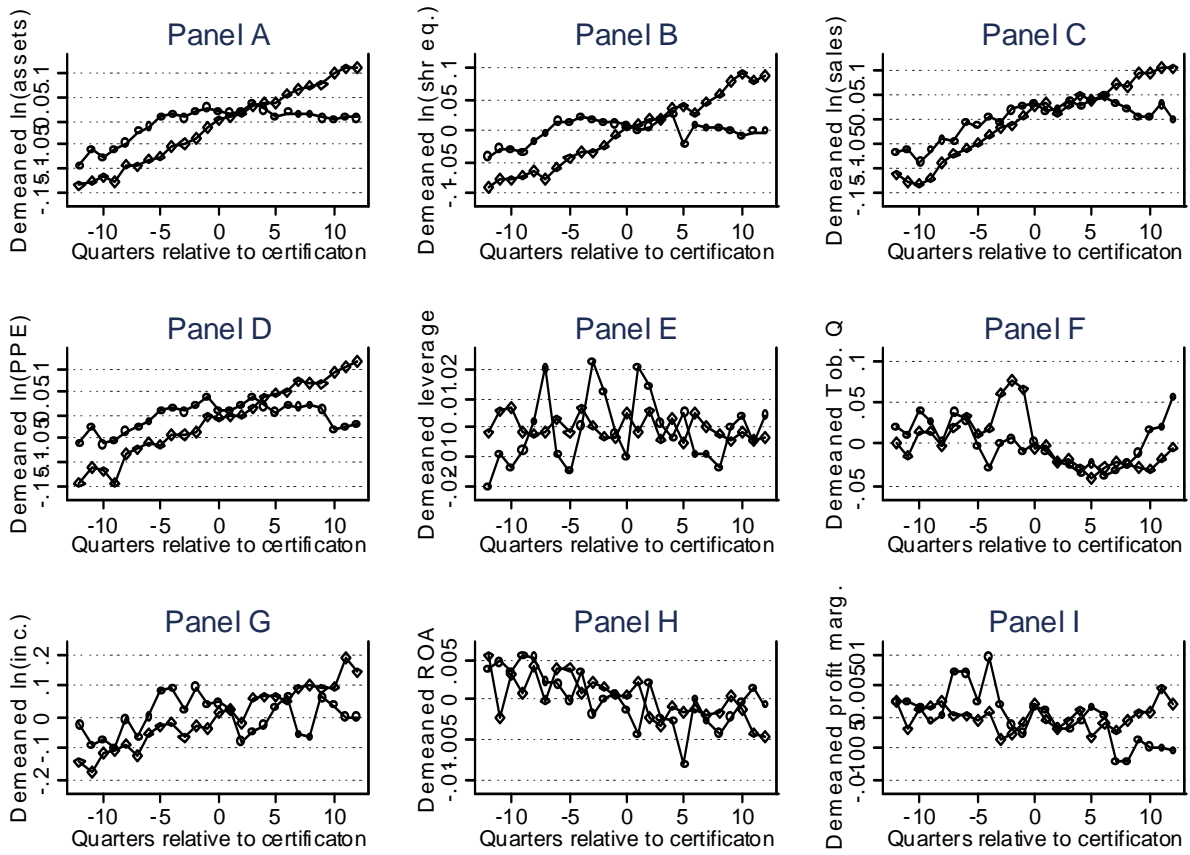**Figure 10: Average cumulative abnormal return, by right-to-work status**
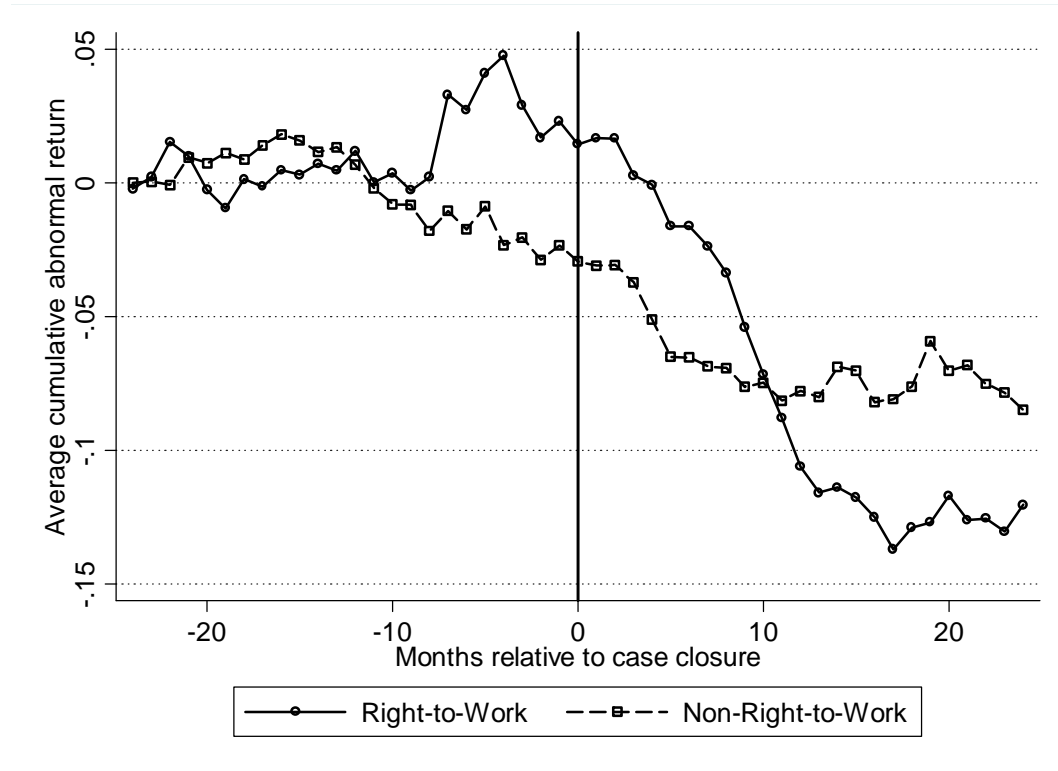
Figure 11: Compustat variables; Union victory/loss comparisons

Notes: The sample consists of publicly traded companies with elections taking place between 1973-1999 where at least 5% of the workforce voted. Lines with circles correspond to union victory companies. Lines with diamonds correspond to union loss companies. All variables are drawn from the Compustat quarterly database. Each variable is demeaned, where the mean is taken within each election panel.

Figure 12: Compustat variables; Union victory/loss differences
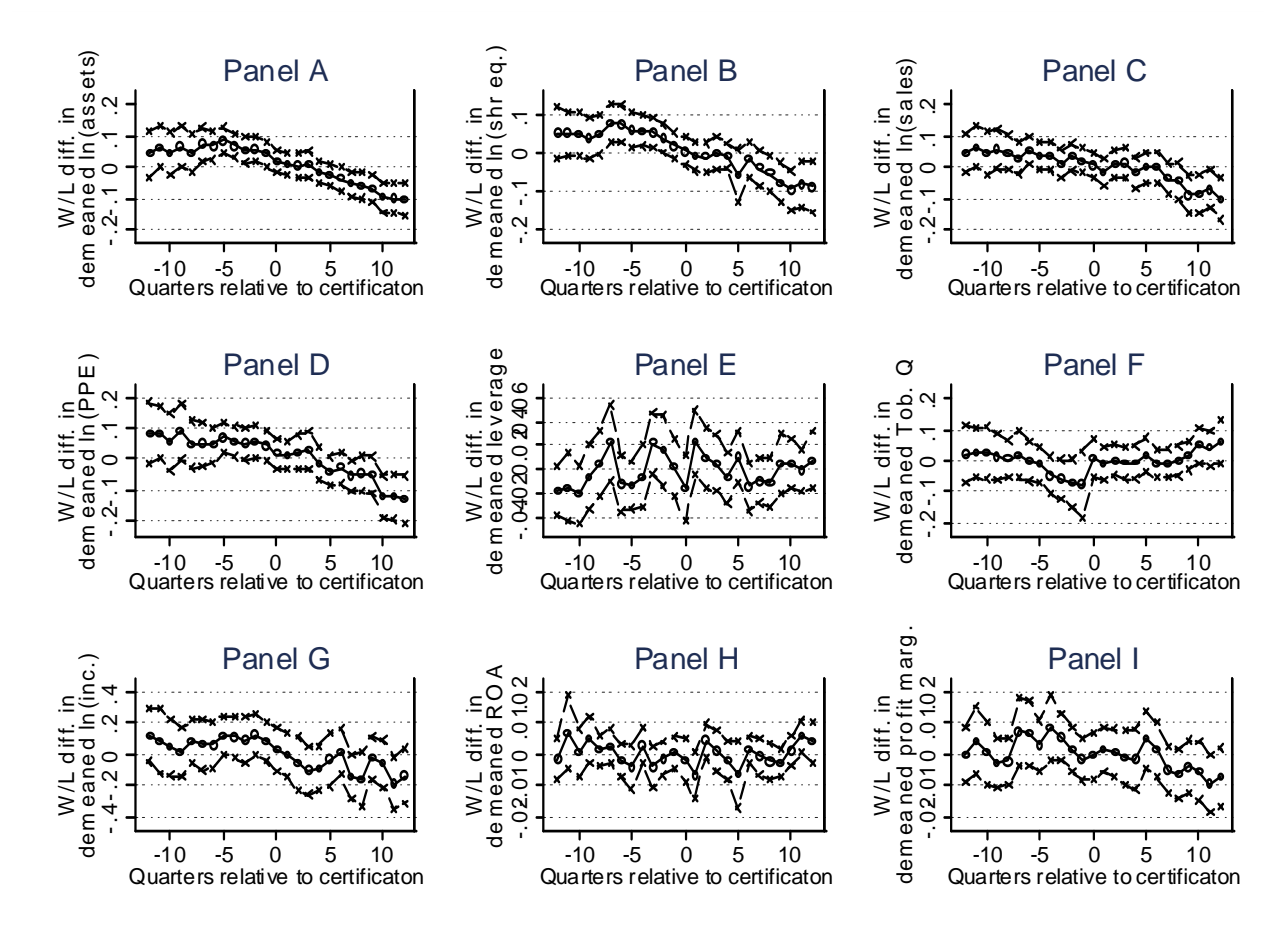
Note: See notes to Figure 11 for description of the sample and of the variables. Dashed lines represent the 95% confidence bands, computed using OLS standard errors clustered on election.

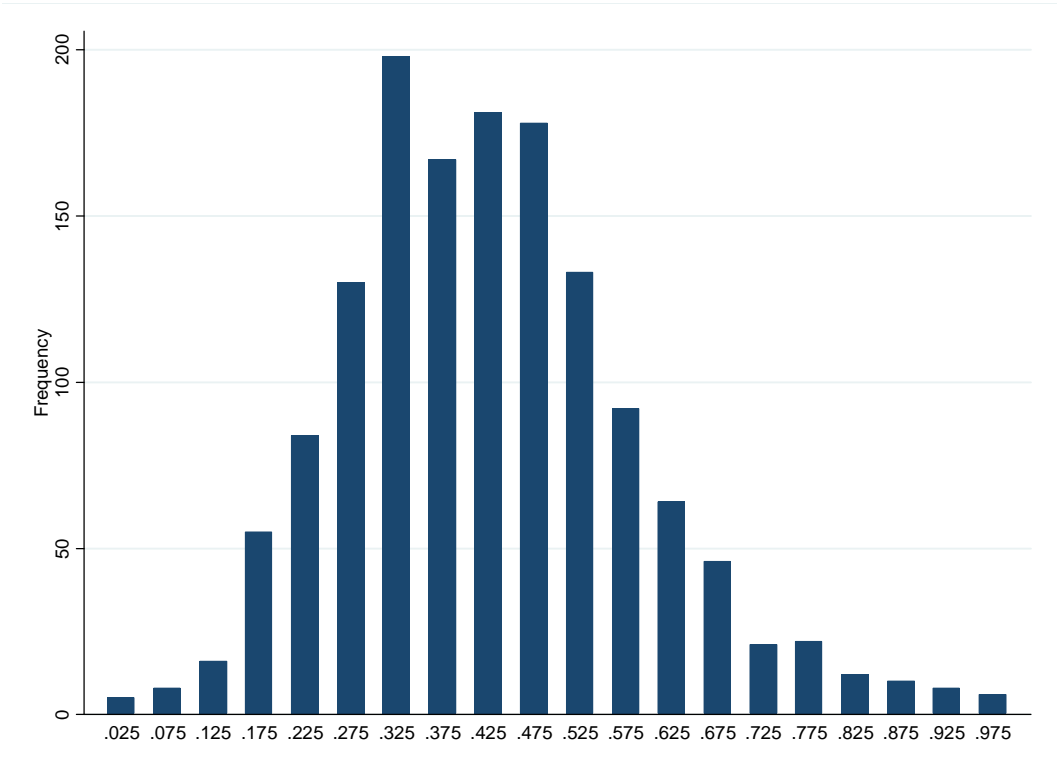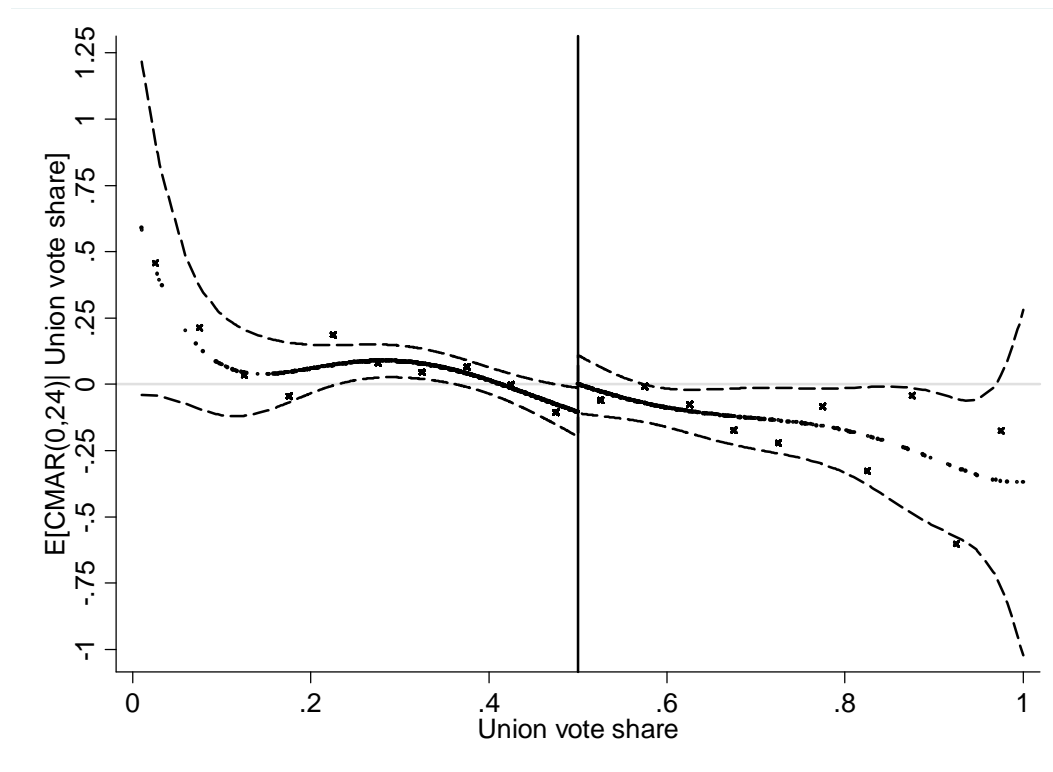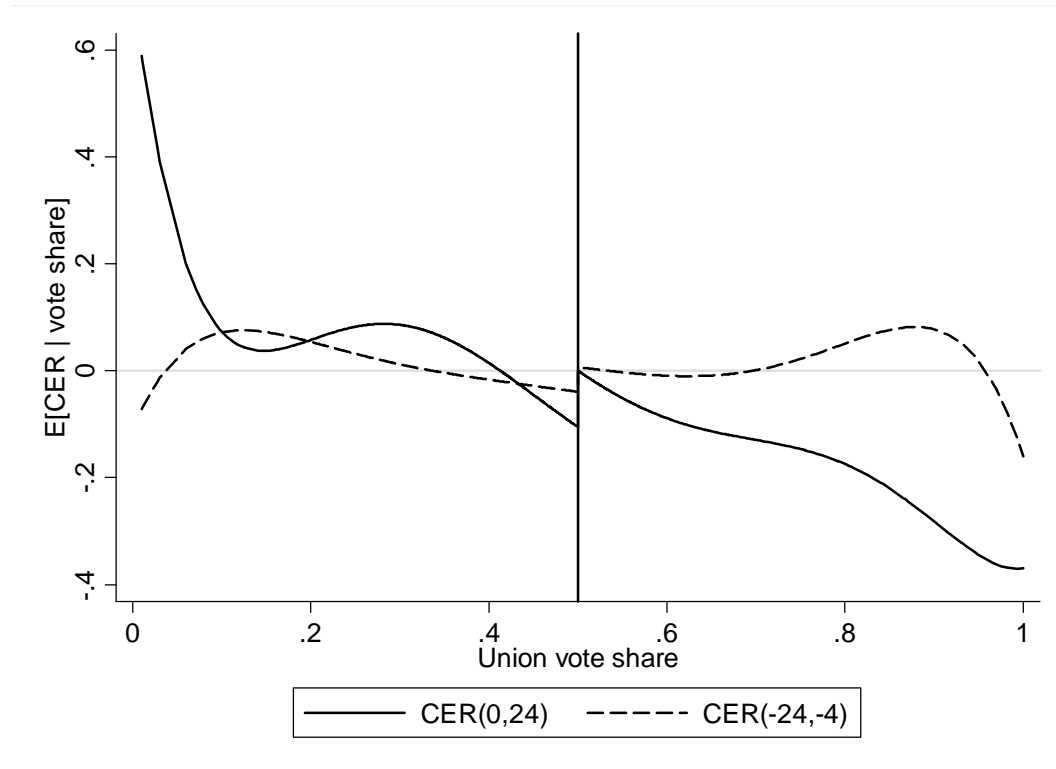**Figure 13: Histogram of the union vote share**

Figure 14: Cumulative abnormal returns in the two years after NLRB closes election, by relation to vote share


Note: Abnormal returns are the simple difference in the security's return and the size-matched benchmark portfolio in the same month. Cumulative abnormal returns are the sum of the abnormal returns over a two year period beginning in the month of case closure. Predicted values are calculated using a sixth-order polynomial, and an indicator for whether the union won. Dashed lines are the 95% confidence interval. Dots are the average cumulative excess return in 20 equally spaced bins. See Section 5.4 for further details on the construction of this figure.

Figure 15: Cumulative abnormal returns in the pre- and post-event periods, by relation to vote share

Notes: Predicted values are calculated using a sixth-order polynomial and an indicator for whether the union won. The solid line corresponds to the predicted cumulative excess return in the two years following case closure, conditional on union vote share. The dashed line corresponds to the predicted cumulative abnormal return calculated starting 24 months prior to the election through four months prior to case closure, conditional on union vote share. See Section 5.4 for further details on the construction of this figure.

Figure 16: Cumulative adjusted abnormal returns in the two years after NLRB closes election, by relation to vote share
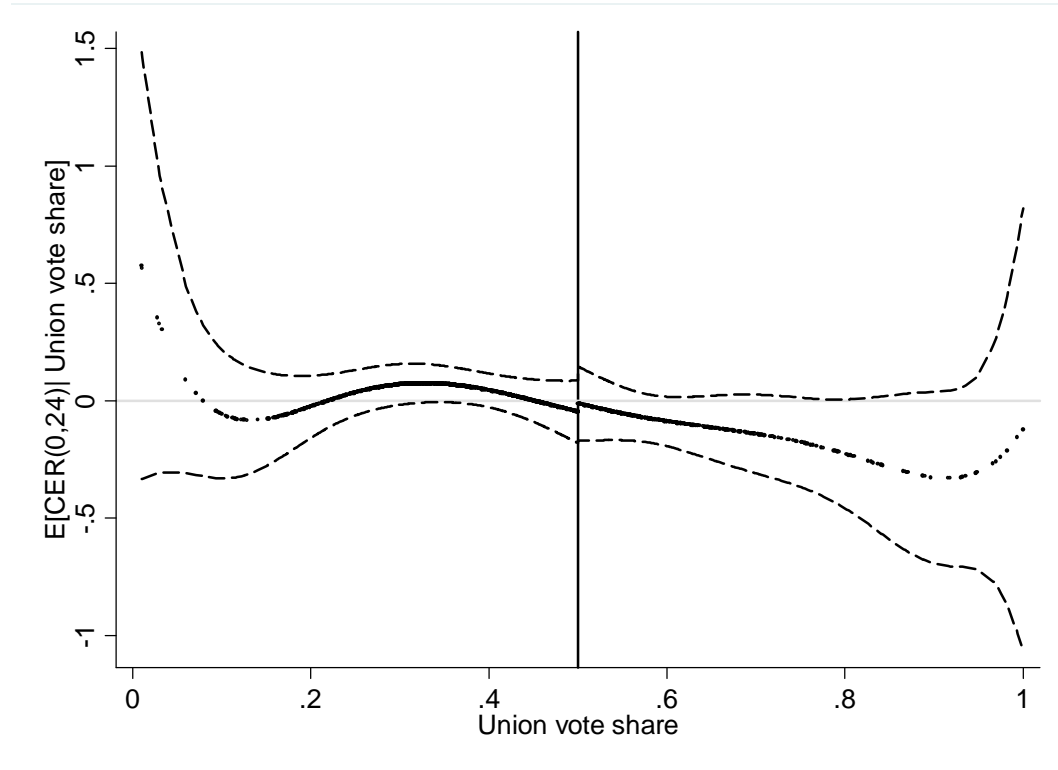
Notes: Adjusted-cumulative abnormal returns are cumulative abnormal returns that have been adjusted for security-specific pre-election trend in abnormal returns. See Section 5.4 for details on the construction of this variable. Predicted values are calculated using a sixth-order polynomial and an indicator for whether the union won. Dashed lines are the 95% confidence intervals.

Figure 17: Reconciling the model with estimates from the research design
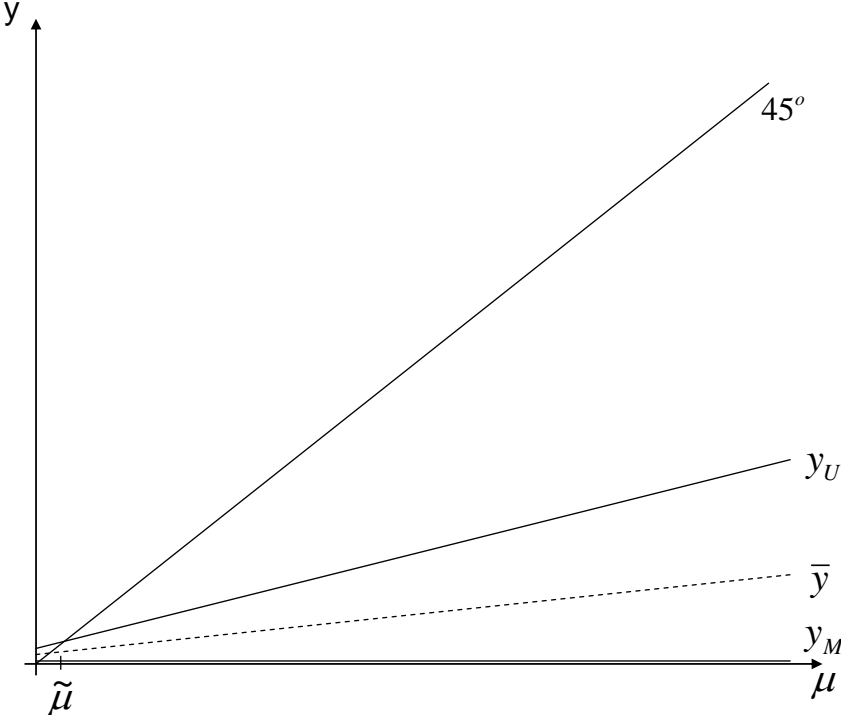
Table 1: Summary Statistics

	At least 5% of workforce voting		Less than 5% of workforce voting	
	Union wins	Union loses	Union wins	Union loses
Number of elections	414	1022	1163	2682
Vote share for union	0.62	0.35	0.64	0.35
	[0.11]	[0.10]	[0.13]	[0.10]
Number of voters	449.1	454.2	276.5	297.6
	[534.9]	[558.5]	[263.4]	[301.6]
Number eligible	496.0	494.0	286.4	317.9
	[649.3]	[638.9]	[286.1]	[330.4]
Fraction of employees voting	0.21	0.23	0.01	0.01
	[0.21]	[0.21]	[0.01]	[0.01]
Year of election	1975.2	1976.9	1974.9	1976.6
	[9.17]	[9.11]	[9.24]	[9.42]
Fraction in Manufacturing	0.78	0.75	0.79	0.81
Number of employees	3813.3	3430.8	68468.6	75284.6
	[5377.5]	[5195.4]	[134336.5]	[123610]
Market Value (CRSP)	353.8	330.9	4734.1	6350
	[880.3]	[783.8]	[10,547]	[13,660]
Market Value (Compustat)	308.7	329.80	6334.1	7580.9
	[614.9]	[799.0]	[13372.0]	[16,343.1]
	{0.34}	{0.33}	{0.76}	{0.78}
Shareholder equity	242.6	233.2	4991.7	4479.8
	[433.0]	[497.7]	[13859.3]	[9432.4]
	{0.34}	{0.31}	{0.77}	{0.77}
Total Assets	588.4	683.8	13974.4	14164.9
	[1243.3]	[1876.5]	[36396.5]	[33308.0]
	{0.37}	{0.31}	{0.78}	{0.79}
Total Liabilities/Total Assets	0.060	0.068	0.062	0.071
	[0.118]	[0.162]	[0.112]	[0.183]
	{0.58}	{0.44}	{0.55}	{0.60}
Pretax income	15.11	9.76	249.3	276.3
	[46.97]	[41.9]	[731.7]	[731.1]
	{0.35}	{0.36}	{0.74}	{0.74}

Table 1 (cont.)

	At least 5% of workforce voting		Less than 5% of workforce voting	
	Union victory (UV firms)	Union loss (UL firms)	Union victory (UV firms)	Union loss (UL firms)
Sales	160.7	144.2	2693.5	3041.2
	[238.7]	[225.1]	[5306.3]	[5534.1]
	{0.33}	{0.31}	{0.80)	{0.80}
Tobin's Q	1.17	1.30	1.29	1.31
	[0.658]	[0.694]	[0.642]	[0.625]
	{0.44}	{0.50}	{0.48}	{0.56}
Profit margin	0.069	0.060	0.084	0.084
	[0.119]	[0.167]	[0.073]	[0.074]
	{0.44}	{0.50}	{0.46}	{0.52}
Income/Employees	0.004	0.003	0.004	0.004
	[0.023]	[0.008]	[0.006]	[0.007]
	{0.41}	{0.49}	{0.48}	{0.51}
Return on Assets	0.013	0.022	0.026	0.027
	[0.051]	[0.037]	[0.023]	[0.027]
	{0.48}	{0.53}	{0.47}	{0.25}
Dividend Ratio	0.633	0.259	1.15	0.941
	[3.42]	[1.100]	[6.99]	[11.02]
	{0.44}	{0.50}	{0.58}	{0.59}
Fraction of stocks delisted	0.10	0.08	0.049	0.028

Notes: Summary statistics are based on the NLRB election, Compustat, and CRSP data. Standard deviations are in brackets. For Compustat variables, the average percentile rank, relative to all Compustat companies in the year and quarter of the election, are in braces. Market value, shareholder equity, total assets, pretax income, and sales are in millions of dollars. Summary statistics for market value are derived from both the CRSP and Compustat databases. These measures differ because there are more missing values in the Compustat database. Fraction of stocks delisted is computed as the fraction of stocks with a non-missing delisting return in a two year window surrounding the NLRB case closure month. Profit margin = pre-tax income/sales. Dividend ratio = dividends/pre-tax income.

Table 2: Estimates of post-election cumulative abnormal returns

	(1) Size-matched benchmark	(2) Size × industry-matched benchmark	(3) Broad-market benchmark
Panel A: Union Victory			
ACAR(0,24)	-0.092 (0.028)	-0.096 (0.028)	-0.103 (0.029)
ACAR (-24,-4)	-0.010 (0.020)	-0.009 (0.020)	-0.010 (0.020)
Adjusted- ACAR (0,24)	-0.100 (0.042)	-0.103 (0.042)	-0.111 (0.042)
Panel B: Union Loss			
ACAR (0,24)	0.029 (0.021)	0.020 (0.020)	0.016 (0.022)
ACAR (-24,-4)	0.034 (0.022)	0.004 (0.014)	-0.009 (0.014)
Adjusted- ACAR (0,24)	0.029 (0.030)	0.016 (0.032)	0.028 (0.031)

Notes: ACAR(X,Y) denotes the average cumulative abnormal return from month X to month Y relative to the NLRB case closure month. There are 414 elections in the sample in Panel A, and 1022 elections in Panel B.   See Section 4.2 for details on the construction of the benchmark portfolios and estimation.

Table 3: Fama-French Calendar Time Portfolio Estimates

Panel A: Union Win Portfolio (≥5% sample)

Event-window:	Alpha	MKTRF	HML	SMB
(0,24)	-0.0051	0.909	0.421	1.12
	(0.0014)	(0.035)	(0.054)	(0.048)
(-24,-4)	-0.0015	0.996	0.487	1.14
	(0.0015)	(0.038)	(0.062)	(0.054)

Panel B: Union Loss Portfolio (≥5% sample)

Event-window:	Alpha	MKTRF	HML	SMB
(0,24)	-0.0001	1.04	0.469	1.01
	(0.0017)	(0.031)	(0.048)	(0.043)
(-24,-4)	-0.0005	0.970	0.264	1.04
	(0.0011)	(0.020)	(0.040)	(0.035)

Panel C: Union Win Portfolio (<5% sample)

Event-window:	Alpha	MKTRF	HML	SMB
(0,24)	0.0010	1.10	0.395	0.222
	(0.0014)	(0.037)	(0.055)	(0.048)
(-24,-4)	-0.0009	1.10	0.283	0.373
	(0.001)	(0.026)	(0.042)	(0.037)

Panel D: Union Loss Portfolio (<5% sample)

Event-window:	Alpha	MKTRF	HML	SMB
(0,24)	-0.0015	1.14	0.509	0.212
	(0.0007)	(0.023)	(0.035)	(0.030)
(-24,-4)	-0.0009	1.10	0.220	0.335
	(0.0008)	(0.017)	(0.031)	(0.027)

Note: The "≥5% sample" consists of elections where at least 5% of the firm's workforce voted. The "<5% sample" corresponds to elections where less than 5% of the firm's workforce voted. MKTRF is the monthly return of the CRSP value-weighted NYSE/AMEX/NYSE broad market index, SMB is the monthly return on the zero investment portfolio for the common size factor in stock returns, and HML is the monthly return on the zero investment portfolio for the common book-to-market equity factor in stock returns. The unit of observation is the calendar month. Observations are weighted by the number of firms in the event-window.

Table 4: Compustat Analysis

	(1) Ln(Assets)	(2) ln(Shareholder equity)	(3) Ln(PPE)	(4) ln(Sales)	(5) ln(pretax income)	(6) Dividend Ratio	(7) Profit margin	(8) ROA	(9) Tobin's Q	(10) Liabilities/ Assets
post	0.150	0.106	0.137	0.132	0.168	-0.197	0.0001	-0.004	-0.054	-0.001
	(0.021)	(0.020)	(0.028)	(0.019)	(0.031)	(0.118)	(0.002)	(0.001)	(0.026)	(0.005)
post × union win	-0.110	-0.098	-0.113	-0.077	-0.168	0.045	-0.005	-0.001	0.031	0.003
	(0.037)	(0.035)	(0.048)	(0.034)	(0.062)	(0.263)	(0.003)	(0.002)	(0.038)	(0.008)
Observations	14,319	16,220	14,223	17,028	14,042	6,127	14,585	13,960	14,035	5,791
R-squared	0.97	0.95	0.96	0.94	0.75	0.084	0.64	0.32	0.66	0.28

Notes: Variables are derived from Compustat data; 1973-1999. Each column corresponds to a different model estimated using OLS. Standard errors clustered on election are in parentheses. Observations are event quarter × firm cells. The dependent variables are demeaned, where the mean is taken over all non-missing observations in an election panel. Sample sizes vary due to the presence of missing values. PPE stands for plant, property, and equipment. ROA stands for return on assets.

Table 5: Cumulative excess returns and vote share

	CER(0,24):				CER(-24,-4):				Adjusted-CER(0,24):			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Constant	0.029	-0.065	-0.075	-0.064	0.0003	-0.018	-0.029	-0.035	0.029	-0.024	-0.029	-0.002
	(0.021)	(0.030)	(0.039)	(0.035)	(0.016)	(0.025)	(0.033)	(0.030)	(0.030)	(0.045)	(0.058)	(0.053)
Union won	-0.121	0.048	0.080	0.049	0.003	0.037	0.021	0.027	-0.129	-0.032	0.035	-0.019
	(0.035)	(0.054)	(0.066)	(0.053)	(0.028)	(0.046)	(0.057)	(0.047)	(0.051)	(0.081)	(0.101)	(0.082)
Union won ×vote share				-0.016				0.332				-0.439
				(0.321)				(0.255)				(0.469)
vote share		-0.616		-0.610		-0.123		-0.235		-0.353		-0.205
		(0.160)		(0.207)		(0.126)		(0.162)		(0.233)		(0.301)
p(vote share)			X				X				X	
Observations	1436	1436	1436	1436	1436	1436	1436	1436	1436	1436	1436	1436

Notes: Robust standard errors are in parentheses. The sample consists of all elections where at least 5% of the workforce voted. The variable "vote share" denotes the union vote share, minus 0.5. Following Dinardo and Lee (2004), the vote share is aggregated to 20 discrete bins. The dependent variable is the cumulative abnormal return from months 0 to 24 relative to case closure (columns 1-4), the cumulative abnormal return from -24 through -4 months relative to case closure (columns 5-8), and the adjusted-cumulative abnormal return from 0 to 24 (columns 9-12),. See Section 5.4 for details on the construction of these variables. The term p(vote share) denotes a fourth-order polynomial in the union vote share.
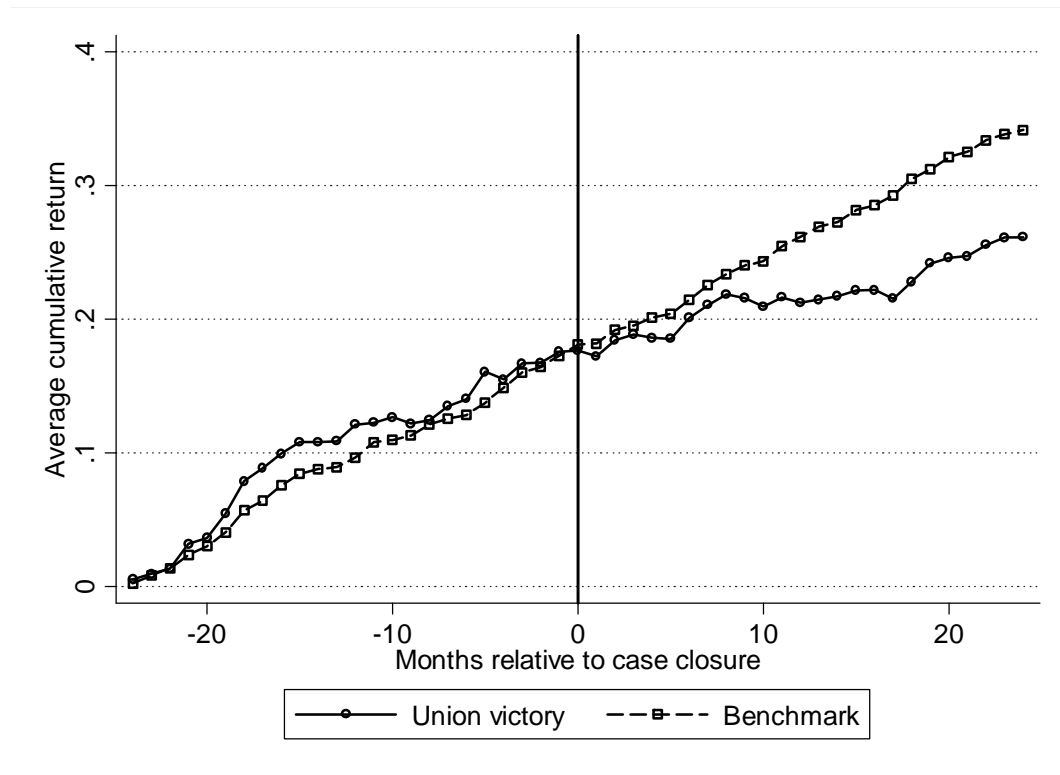
Appendix Figure 1: Average cumulative returns; union victory portfolio and sized-matched benchmark; non-imputed data
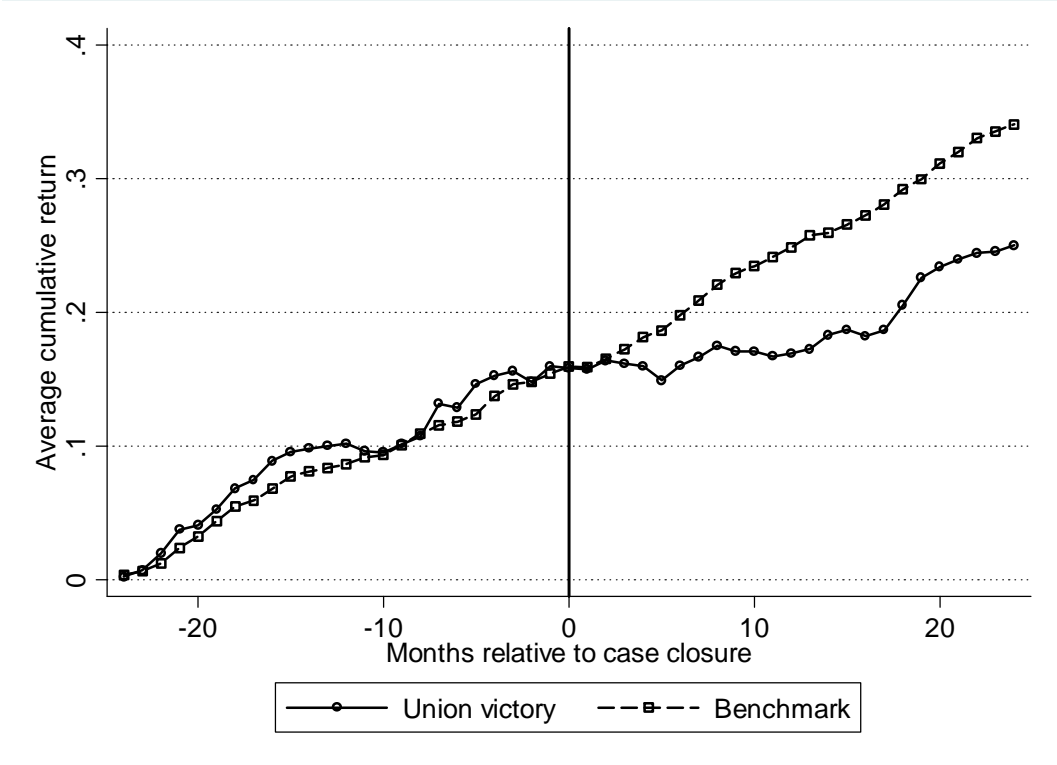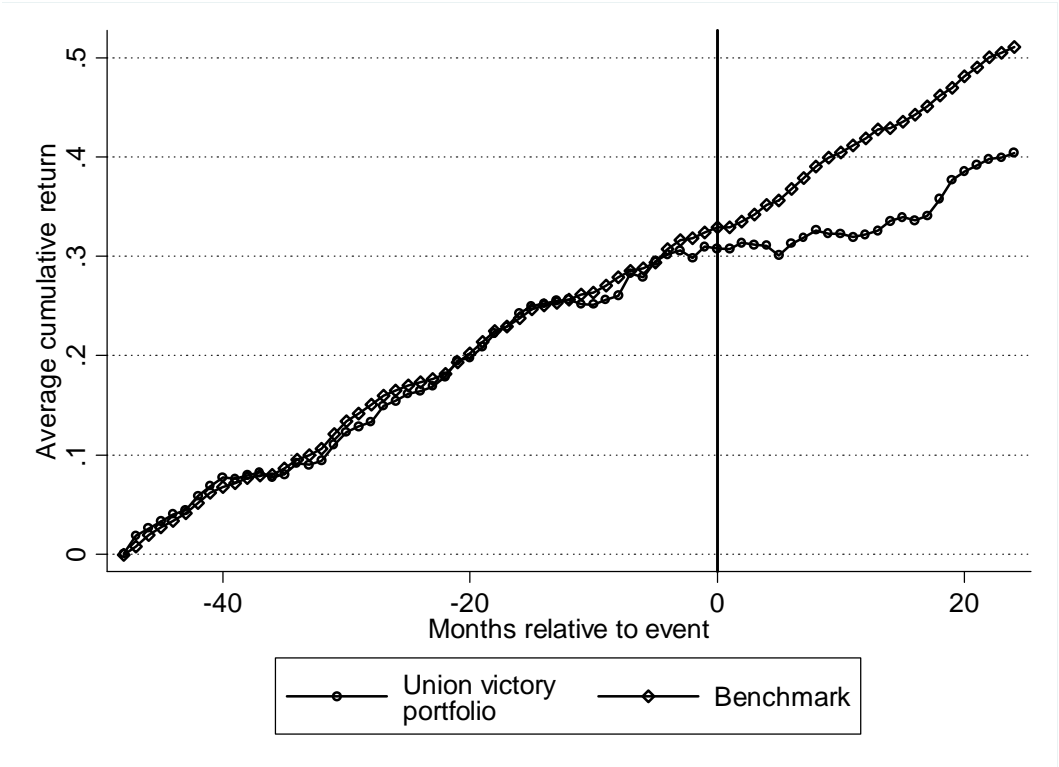
Appendix Figure 2: Average cumulative returns; union victory portfolio and sized-matched benchmark; Balanced panel
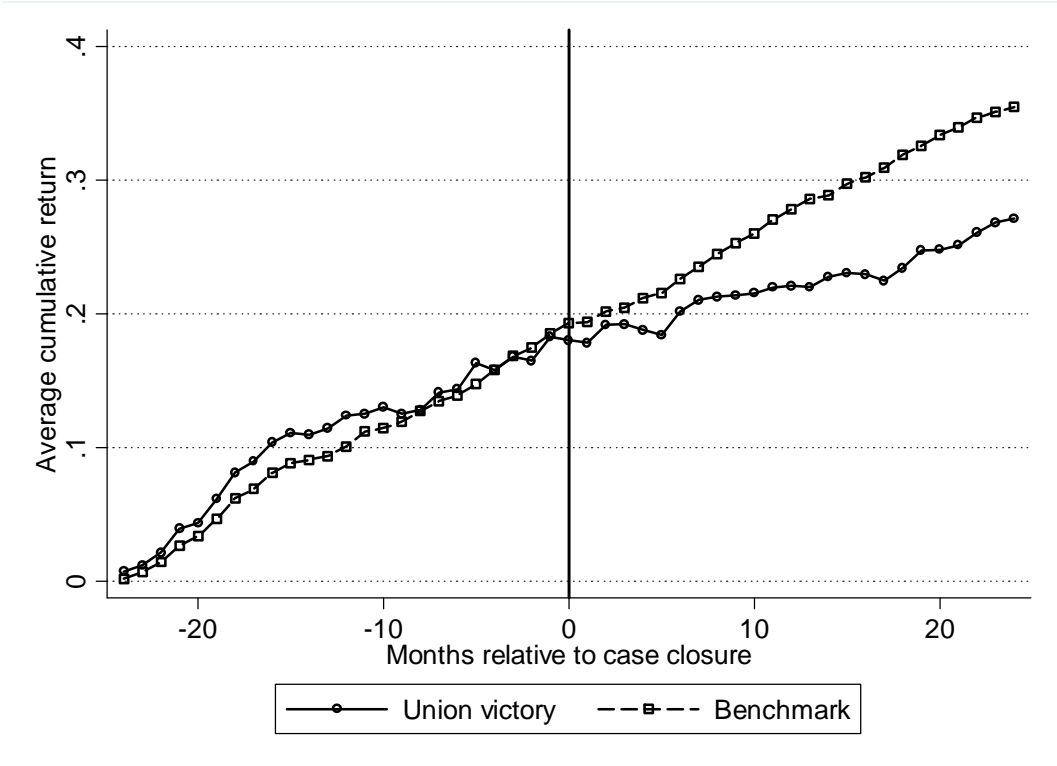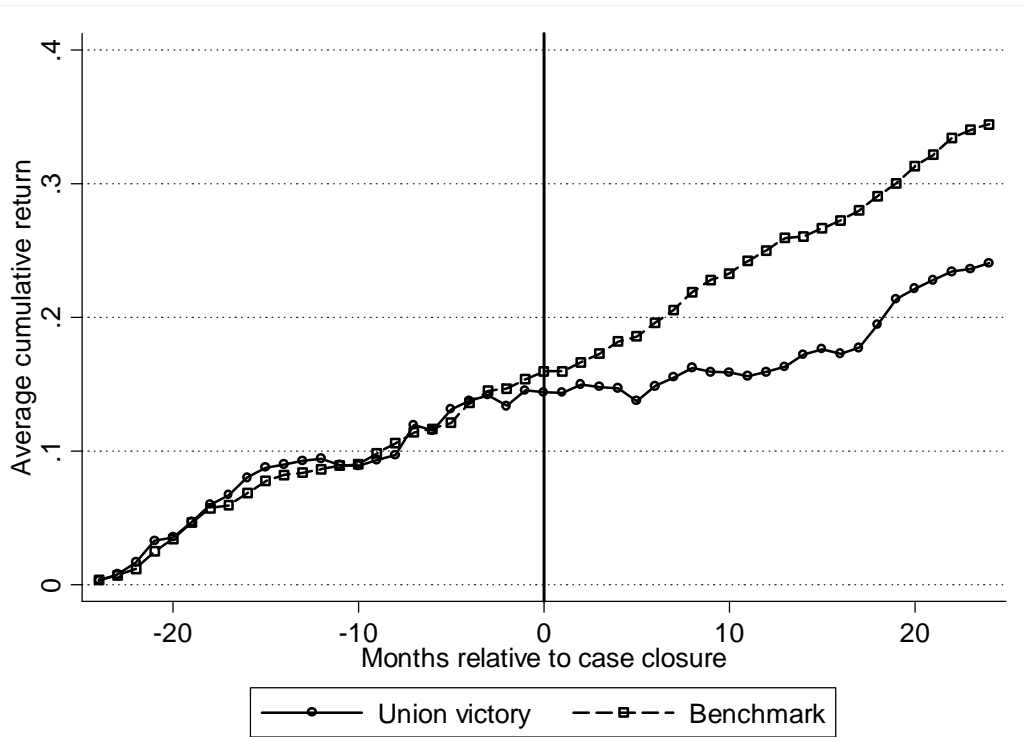
Appendix Figure 3: Average cumulative returns; union victory portfolio and sized-matched benchmark; Robustness probe: eliminate 5% most positive and 5% most negative post-event abnormal return elections;
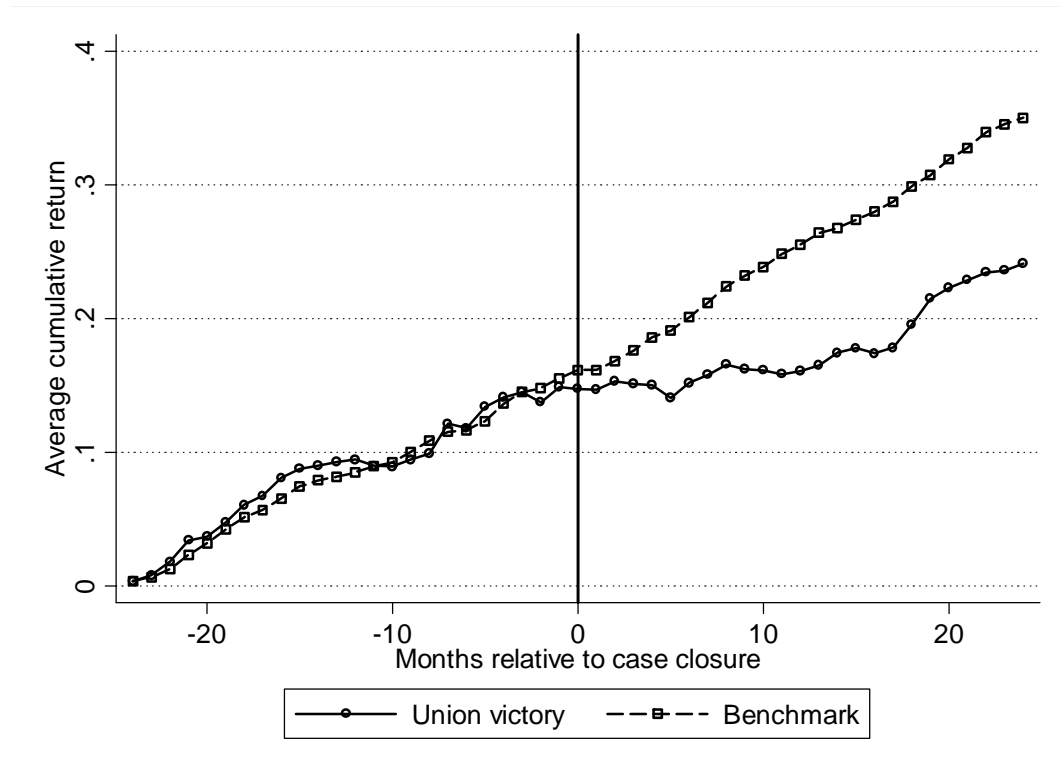
Appendix Figure 4: Average cumulative returns; union victory portfolio and sized-matched benchmark; Four year pre-event window

Appendix Figure 5: Average cumulative returns; union victory portfolio and industry×sized-matched benchmark

Appendix Figure 6: Average cumulative returns; union victory portfolio and CRSP equally-weighted index benchmark
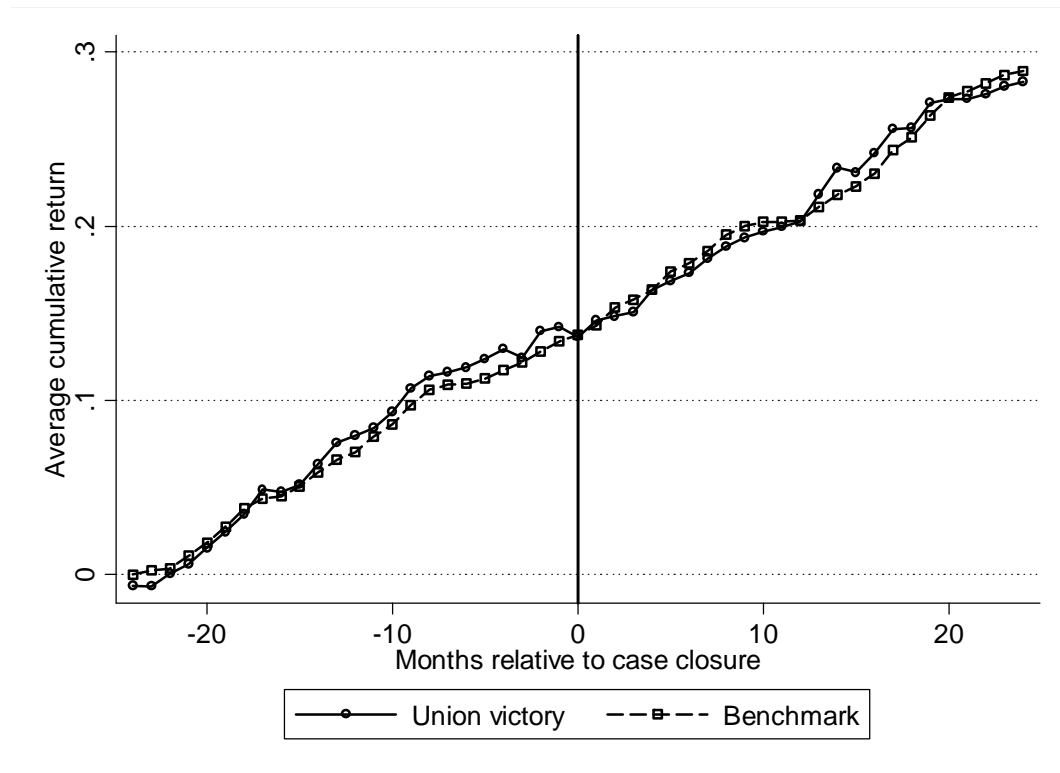
Appendix Figure 7: Average cumulative returns; union loss portfolio and sized-matched benchmark; non-imputed data
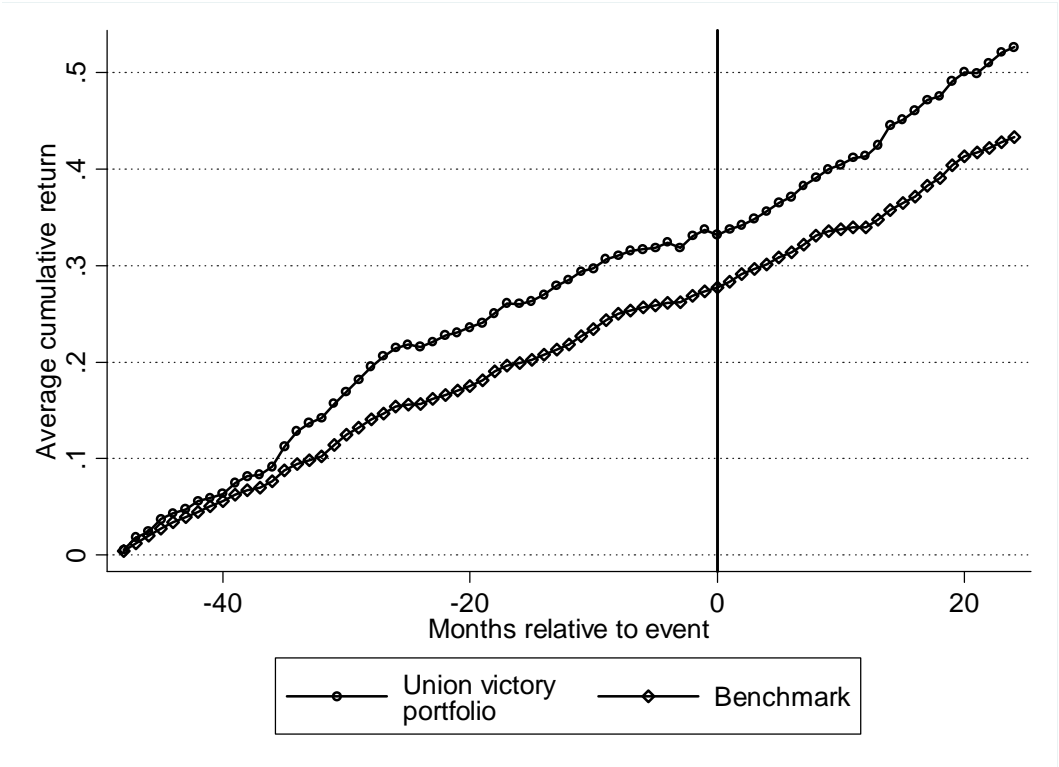
Appendix Figure 8: Average cumulative returns; union loss portfolio and sized-matched benchmark; Balanced panel
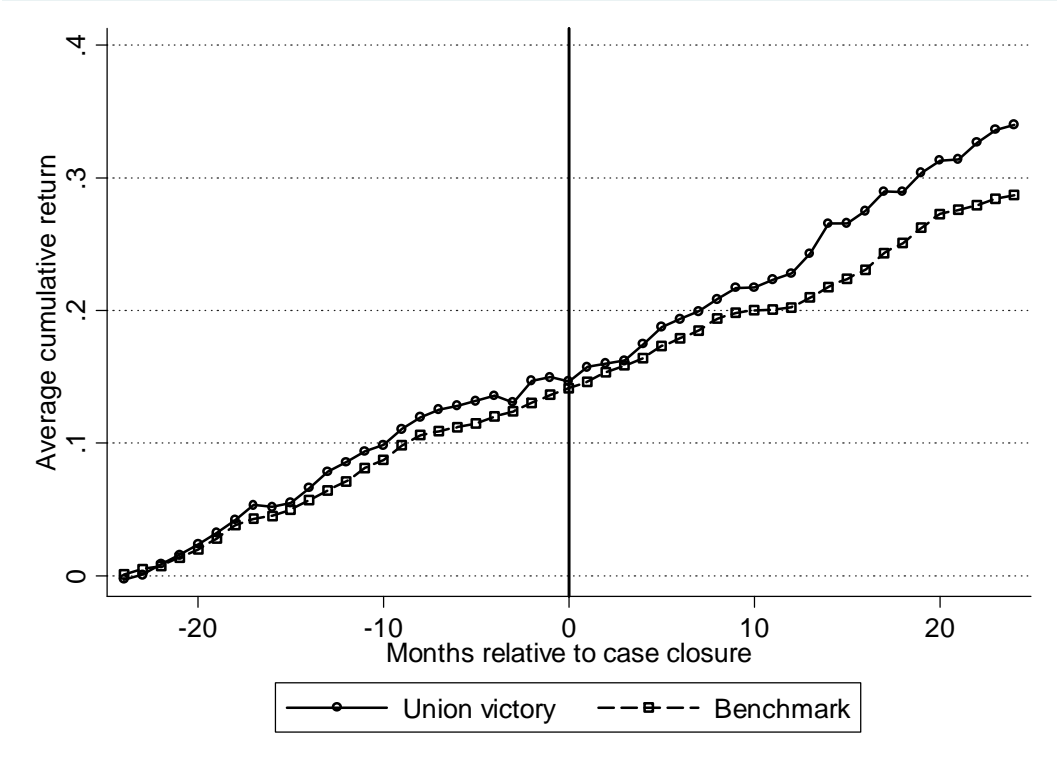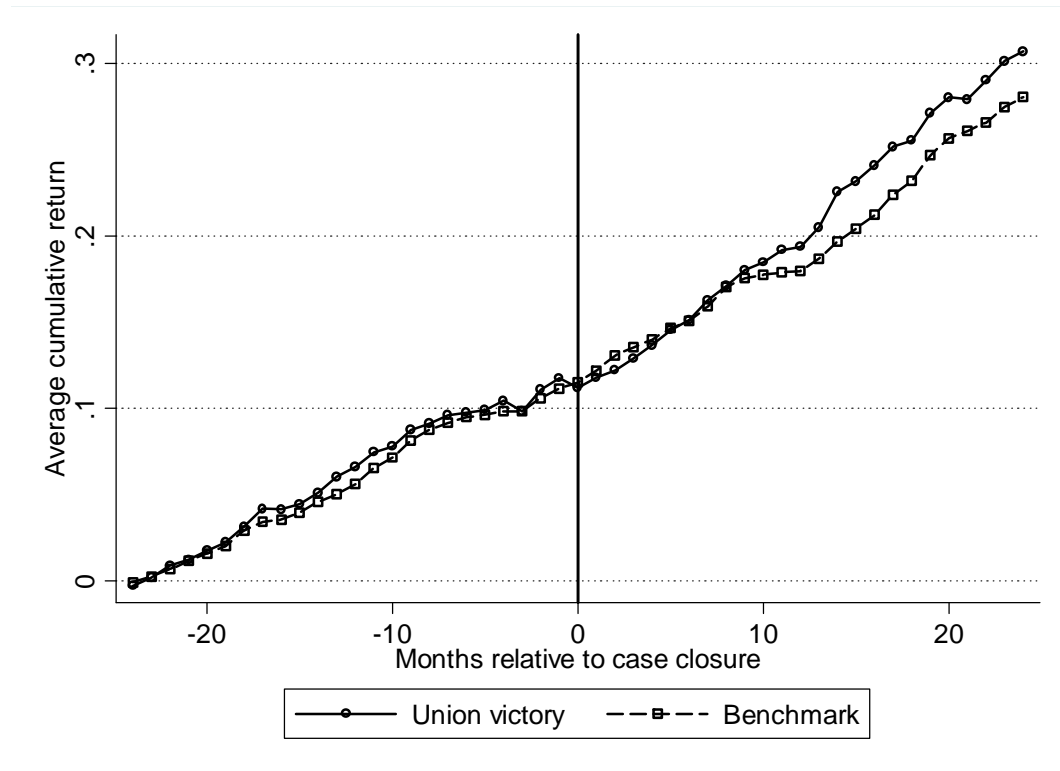
Appendix Figure 9: Average cumulative returns; union loss portfolio and sized-matched benchmark; Robustness probe: eliminate 5% most positive and 5% most negative post-event abnormal return elections;

Appendix Figure 10: Average cumulative returns; union loss portfolio and sized-matched benchmark; Four year pre-event window

Appendix Figure 11: Average cumulative returns; union loss portfolio and industry×sized-matched benchmark

Appendix Figure 12: Average cumulative returns; union loss portfolio and CRSP equally-weighted index benchmark