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## Do unions shape political ideologies at work?

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# Do Unions Shape Political Ideologies at Work?

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**AWI DISCUSSION PAPER SERIES NO. 719**

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# Do Unions Shape Political Ideologies at Work? \*

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## Abstract

Labor unions' largest potential for political influence likely arises from their direct connection to millions of individuals at the workplace. There they may change the political views of both unionizing workers as well as of their non-unionizing management which is arguably the most relevant out-group. In this paper, we analyze the impact of unionization on workers' and managers' campaign contributions at the workplace over the 1980-2016 period in the United States. Therefore, we combine establishment-level union election data with transaction-level campaign contributions to federal and local candidates. In stacked Difference-in-Differences models, we find that unionization results in a leftward shift of campaign contributions. Unionization increases the support for Democrats relative to Republicans not only among workers but also among managers. To test the validity of these findings, we perform Regression Discontinuity exercises which show that there are no differential trends along placebo vote share cutoffs and that the results hold when comparing increasingly close elections. Moreover, we provide evidence that our results are not driven by compositional changes of the workforce.

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# 1 Introduction

Labor unions are powerful political organizations. They have been an important factor for the allocation of resources and power and have shaped country’s welfare systems, labor market institutions, and political balance of power. Even today, US unions still draw on significant political resources: they employed over 3,000 full-time workers for political activities and spend \$700 million in 2010, a figure that rose to \$1.8 billion in 2020 ([WSJ, 2012](#); [NILRR, 2021](#)). Labor unions have been termed the central interest group in politics for the working-class ([Lipset, 1983](#)). They directly aim to influence politics by endorsing candidates, mobilizing members to vote and channeling campaign contributions. Their main activities are, however, still confined to the workplace where they strengthen workers’ bargaining power and influence the management-worker relations as they communicate worker preferences to management. It is also the workplace through which unions most likely exert their biggest political influence. Unions have the potential to sway elections if they align political views of their more than 14 million members and other employees at the unionized workplace. Their aggregate political influence on the workplace is far from clear, however. While they might be able to assemble unionized workers around their political positions, the firm’s management could counter these efforts. Any backlash in the political behavior of this powerful out-group may prevent unions from achieving their political agenda.

In this paper, we examine the political influence of labor unions at the workplace by studying how unionization affects employees’ campaign contributions. Campaign contributions are viewed as essential for candidates to win elections. Their influence on the set of candidates that run and win elections has been documented recently (e.g., [Barber, 2016b](#); [Bekkouche and Cagé, 2018](#)). Moreover, donors prefer to give to ideologically proximate candidates on average such that campaign contribution patterns reveal the political ideology of donors (e.g., [Bonica, 2014](#)). An assessment of campaign contribution patterns can therefore highlight the influence of unions on an important input into the political process and permits conclusions about shifts in political ideology. We combine establishment-level data on 6,065 union elections with transaction-level data on 357,434 campaign contributions to federal and local candidates over the 1980-2016 period. In the contribution data we can differentiate between unionizing workers and their management. This allows us to analyze the differential impact of unionization on workers’ and managers’ campaign contributions at the establishment-level in the United States.

Labor unions have a strong relationship to the Democratic Party with whom they share stances on many labor issues ([Dark, 2001](#)). It is thus no surprise that unions have overwhelmingly

supported Democratic candidates via campaign contributions ([OpenSecrets, 2018](#)). Likewise, the Democratic Party is aware of the value of unions evident by President Biden announcing to become the “most pro-union President [...] in American history” ([Biden](#) on Sept. 8th 2021). However, the relationship might go beyond common interests.

Unions have the potential to influence the formation of preferences of workers and their management as well as the interaction between the groups. If they are successful, they can influence election outcomes far beyond their direct contributions by leveraging the political power of their members and their superiors. The direct influence of unions on their members has received most attention in the literature. [Kerrissey and Schofer \(2013\)](#) have argued that unions provide their members with political capital - they inform, engage and mobilize members, largely favoring the Democratic Party. Several studies have documented a significant association between union membership and political outcomes like voting ([Freeman, 2003](#)), preferences for redistribution ([Mosimann and Pontusson, 2017](#)) and trade liberalization support ([Ahlquist et al., 2014](#); [Kim and Margalit, 2017](#)) by comparing union members to non-union members. We build on that literature by assessing the causal impact of unions on campaign contribution patterns of workers.

The aggregate impact of unions might be multiplied or reversed depending on the reaction of out-groups, most importantly the firm’s management. Repeated interactions between workers and their management makes feedback mechanisms likely. If a unions’ actions influence members, they likely influence the management at workplaces where they are present. Ex-ante the reaction of management is not clear. On one side, labor unions can foster managements’ understanding of worker issues due to several reasons. First, unions institutionalize conflicts by establishing rules on bargaining between managers and workers. This leads to an increase in both the quantity and quality of communication between managers and workers ([Verma, 2005](#)). Labor unions give workers a voice as they enhance the formation and communication of workers’ preferences and present them on an equal footing ([Freeman and Medoff, 1979, 1984](#)). Contact theory suggest that this increase in cooperative interactions can enhance perspective taking and reduce worker stereotypes hold by management (e.g., [Allport, 1954](#)).<sup>1</sup> Second, while the employee staying at the margin has the largest negotiating power in a setting without unionization, it is the median voter on behalf of which the union negotiates ([Freeman and Medoff, 1979, 1984](#)). The median

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<sup>1</sup>Views on policy issues like redistribution, minimum wage, unemployment benefits, and income mobility are linked to beliefs about the determinants of success (e.g., [Alesina et al., 2018](#)). Survey evidence points to large gaps in the perception of the determinants of success. While 58% of those with a family income below \$30,000 believe that external circumstances are to blame for poverty, only 41% of those earning more than \$75,000 do so ([Pew, 2012](#)). Contact between the rich and the poor may reduce such stereotypical differences in beliefs.

worker is older and less mobile - traits that correlate with more conservative political views (Pew, 2012). Lower levels of polarization between workers' position and management might in turn enable mutual understanding. Third, labor unions introduce fairer rules at the workplace. For example, unions can introduce formal grievance systems for employees and ensure representation of workers at the board of directors that can itself lower tensions between management and workers (Verma, 2005).

On the other hand, there are also reasons why labor unions might exacerbate conflicts. First, labor unions could increase the salience of labor conflicts. If true, that likely increases polarization as groups tend to adopt the stereotypes of the salient identity (Bonomi *et al.*, 2021). Second, the increase in workers' bargaining power implies a loss of status and power for management. A large psychological literature has shown that tensions between groups can increase if one feels threatened by the other (e.g., Sherif *et al.*, 1961; Campbell, 1965). Overall, it is not clear whether labor unions are able to persuade workers and their management or whether they enhance the opposition of management to workers' positions.

Assessing the causal impact of unionization on political views is challenging since union members differ from non-union members. We assess the causal impact of unionization in a Difference-in-Differences (DiD) framework at the establishment level. We compare establishments with a documented interest of workers in union elections at the same point in time. Thus, our sample can be expected to be more similar than a random sample of establishments. Within that sample, we compare campaign contributions of establishments where workers voted for unionization with establishments that voted against unionization by applying a stacked DiD model. Thereby, we rely on the assumption that campaign contributions in losing establishments would have developed in parallel to campaign contributions from winning establishments in the absence of unionization. The plausibility of that assumption is checked in a number of tests.

First, we show that trends in the three election cycles prior to a union election are parallel, supporting the plausibility of the assumption. Second, we test whether our outcomes are correlated with the pro-union vote share among the establishments that lost the union election. Since the treatment status discontinuously changes at the 50% threshold, there should be no effect on establishments with different vote shares below 50%. Indeed, we do not find any evidence for a differential effect across different vote-shares, which allows us to rule out that any sizeable confounding factors correlated with the pro-union vote share and the timing of the election drive the results. Finally, we restrict the sample to establishments with increasingly close elections. Our results are robust to a wide range of vote share bandwidths around the 50% cutoff. We find significant effects for workers for a maximum bandwidth of up to 10% and for management for a

maximum bandwidth of up to 5%. Overall, we view the combination of the stacked DiD model with various placebo and robustness tests on the parallel trends assumption as a convincing strategy to estimate treatment effects for elections that win by large margins of support while still accounting for selection into winning versus losing elections.<sup>2</sup>

Our main results indicate a leftward shift of both managers and workers. Quantitatively, the DiD estimates show that winning the union election increases the percentage difference in donations to Democrats versus to Republicans by 12 percentage points for workers and by 20 percentage points for managers. These patterns are not consistent with an increase in tensions between unionized workers and their management but rather point towards an alignment of political preferences. At the same time, we do not find much evidence for an effect on total contribution amounts. Only for workers we see a marginally significant increase in total spending in the cycle of the union election which is consistent with a short-run political mobilization of workers through a successful union campaign at the workplace.

The observed influence of labor unions on workers and managers could be explained by a change in the composition of the donating management and labor force. In order to differentiate between compositional and individual-level effects, we develop two specifications. First, we take out any direct effect of unionizing on contributions and focus only on compositional changes. We compare contribution patterns before the union election of donors that donated after the election in establishments where the union won relative to establishments where the union lost. We do not find any sizeable effect. Thus, it seems unlikely that the leftward shift in campaign contributions after unionization is driven by compositional changes. Second, we study individual-level effects by restricting our sample to individuals that have been employed at the establishment before and after the union election and have donated before and after. We find a significant leftward shift in donations for workers as well as managers. In sum, these results are consistent with labor unions persuading members and their management to support labor friendly candidates.

Moreover, we document considerable within-party variation in the effects on contributions to different candidates. Liberal candidates gain and conservative candidates lose, while moderate candidates are not significantly impacted on average. In addition, we show that our results hold when focusing on either federal or local candidates. They also extend to contributions towards political action committees (PACs). In particular, we find that unions are able to mobilize workers increasing their donations to labor and membership PACs. At the same time, unions decrease managers' contributions to corporate PACs. The increased support for labor and civil society

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<sup>2</sup>We also check the robustness of our results to employing different DiD estimators introduced by the literature for a setting with staggered treatment timing (Borusyak *et al.*, 2021; Callaway and Sant'Anna, 2021) which replicate our main results from the stacked DiD model.

interest groups by workers and the reduced support of business interest groups by managers match with the observed pro-liberal shift in their contributions to candidates.

We contribute to the literature by shedding new light on the political effects of unions through the combination of an individual-level political outcome with establishment-level unionization data. To our knowledge we are the first to consider within-firm dynamics and in particular the reaction of management to unionization - a key actor when it comes to political influence. The existing literature on the political impact of unions has either focused on self-reported union members and their households (e.g., [Freeman, 2003](#)) or aggregate outcomes comprising the whole county or state population (e.g., [Feigenbaum \*et al.\*, 2018](#)).<sup>3</sup> By focusing on the unionizing workplace, we directly observe the out-group that arguably reacts most strongly and unpredictably. By examining different political reactions at the workplace, we also relate to a literature studying political spillovers at work ([Babenko \*et al.\*, 2020](#); [Stuckatz, 2022](#)).

Moreover, we complement the literature on the economic impacts of unions by providing insights on the political channel. Numerous studies have assessed the impact of unionization on wages and employee compensation at the establishment level ([DiNardo and Lee, 2004](#); [Frandsen, 2021](#); [Knepper, 2020](#)). These studies can be characterized by the absence of any large wage effects at the establishment level but potentially some positive effects for employee compensation. These findings contrast with evidence on aggregate inequality. [Farber \*et al.\* \(2021\)](#) document a negative effect on income inequality that they argue is difficult to explain by income changes of union members alone, suggesting a potential link between unions and distributional legislation.

The paper is organized as follows. Section 2 describes the institutional background while section 3 introduces the data. The empirical specification is outlined in section 4, after which section 5 presents the results. We explore potential mechanisms and extensions in section 6 and conclude in section 7.

## 2 Institutional Background

### 2.1 Unionizing through NLRB Elections

Since 1935, the National Labor Relations Act (NLRA) gives most private-sector workers in the U.S. the right to organize in unions and take collective action such as bargaining and strikes. Collective bargaining between unions and employers takes place at the establishment level. Traditionally, workers unionize through a National Labor Relations Board (NLRB) secret ballot election at their establishment. The unionization procedure involves three main steps: a petition

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<sup>3</sup>[Feigenbaum \*et al.\* \(2018\)](#) evaluate the effect of Right-To-Work laws. They compare border counties in states with and without Right-To-Work laws and find negative effects on turnout and the Democratic vote share.



drive, an election, and certification.

The organizing drive can be initiated either by the workers at an establishment or by a union organization. The initiator first needs to gather the signatures of at least 30% of workers in the proposed bargaining unit who thereby express a desire for unionization. With these signatures, an election petition is filed to the NLRB. The NLRB decides whether to accept the petition by ascertaining whether workers in the proposed bargaining unit share common interests that can be adequately represented by the union. Employers often delay this procedure through disputing which employees qualify to be in the bargaining unit (Levitt and Conrow, 1993). If the petition is accepted, the NLRB schedules a secret ballot election which usually takes place at the workplace.<sup>4</sup> The union wins the election if it obtains a strict majority of the votes cast. In case of union victory, the NLRB certifies the union as the sole authorized representative of employees in the bargaining unit.

Union certification requires the employer to bargain “in good faith” with the union. This bargaining generally aims at concluding a first contract between union and employer. While there is no legal obligation to reach such an agreement, evidence suggests that in 55-85% of winning elections a first contract is reached within three years of the election (Cooke, 1985; Ferguson, 2008; Reed, 1990). When both parties cannot reach a first agreement (or when subsequently they are disputing over the terms and conditions of the first contract), workers have the right to strike and employers may lock out employees from work.<sup>5</sup> Alternatively, both parties may voluntarily agree to consult a neutral third party to resolve disputes via mediation or arbitration.

The NLRA also lays out which employees may form a bargaining unit. While a bargaining unit may generally include all professional and nonprofessional employees at an establishment, managers and supervisors are always excluded.<sup>6</sup> These employees are considered to be part of a firm’s management rather than its labor force and can therefore not join a union or be part of a bargaining unit. In our empirical analysis, managers and supervisors will thus constitute the “out-group” that is expected to oppose unionization. All other occupations form the “in-group” as they are potentially in the bargaining unit and directly benefit from unionization.

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<sup>4</sup>While the NLRB does not provide a specific timetable, most elections are held within two months of the petition filing (CRS, 2013).

<sup>5</sup>In a lockout, workers involved in the dispute are not allowed to work and are not paid.

<sup>6</sup>The NLRA uses a rather broad definition for supervisors. It includes all individuals who have authority to assign and direct work of other employees, as long as this involves some independent judgement. There is no restriction to the actual share of working time that involves supervisory duties. See Appendix C.3 for details.

## 2.2 Money in US Politics - A Brief Overview

Money plays a dominant role in U.S. politics. Monetary resources are viewed essential for candidates to take part and be successful in the political process. There is indeed increasing evidence that campaign donations can influence who runs for and who wins elections (e.g., [Barber, 2016b](#); [Bekkouche and Cagé, 2018](#); [Schuster, 2020](#)). While much of the public debate on campaign finance regulations centers around donations from corporations and other interest groups, the large majority of campaign contributions in the U.S. actually comes from individual donors. For the 2020 elections, 77% of the total money received by candidates for the U.S. Congress came from individuals. This share has increased over time from 55% in the 2002 elections ([FEC, 2022](#)). While political spending is certainly concentrated among the wealthy ([Bonica and Rosenthal, 2018](#); [Hill and Huber, 2017](#)), it is a prevalent form of political participation for a substantial share of the U.S. electorate. [Bouton et al. \(2021\)](#) estimate that 12.7% of the adult US citizen population have made at least one campaign contribution between 2006 and 2020.

Unlike corporations which are prohibited by U.S. federal law to support candidates directly out of treasury funds, individual donors are allowed to make direct contributions to political candidates.<sup>7</sup> There are, however, restrictions to the maximum amount that an individual can donate to a candidate. The limit varies by recipient type and election cycle. For the 2018 federal elections, for example, individuals were allowed to spend at most 2,700 USD to a single candidate and 5,000 USD to a PAC ([Whitaker, 2018](#)). Recipients are obligated to itemize all individual contributions larger than 200 USD and report the donor’s identifying information along with the amount and date of the contribution. Donations smaller than 200 USD are not required to be itemized but are included in the total amount that the recipient reports to the Federal Election Commission (FEC).

Political scientists differentiate between two broad motivations for why individuals may contribute to political candidates. First, contributions can be seen as consumption goods that give individuals consumption value from participating in politics and from sponsoring candidates that are ideologically close to their own political position ([Ansolabehere et al., 2003](#)). Second, donors may view contributions as investment goods that can buy access to politicians and benefit their own material interests. There is extant evidence that individuals’ donations are ideologically motivated. Individual donors self-report that candidate ideology has a high importance when deciding to whom to give ([Barber, 2016a](#)). Moreover, in comparison to access-seeking PACs

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<sup>7</sup>To make campaign donations, companies must set up a Political Action Committee (PAC) which may only solicit contributions from the firm’s employees. The PAC can in turn donate directly to political candidates or other recipients.

who prefer donating to moderate candidates, individuals tend to support more ideologically extreme candidates (Barber, 2016b; Stone and Simas, 2010). In merged survey-administrative data, contribution-based ideology measures are also found to predict policy preferences of donors, even of donors from the same party (Bonica, 2019). While for the rank-and-file there is consistent evidence in line with ideology being the main driver of political spending, for corporate elites the motivations are more debated. Teso (2022) shows that a business leader’s likelihood of spending to a Congress member increases when the politician becomes assigned to a committee that is policy relevant to the business leader’s company. Based on the estimates, Teso (2022), however, concludes that only 13% of the observed gap in donations to policy-relevant versus other politicians is driven by an influence-seeking motive in line with corporate elites lobbying on behalf of their company. Moreover, Bonica (2016) finds that donations from corporate board members are ideologically quite diverse, both across and within companies. Compared to corporate PACs, business leaders also tend to support more non-incumbent candidates and less powerful legislators. In summary, the evidence suggests that individuals primarily donate to candidates for ideological reasons.

### 3 Data

Previous studies were unable to assess the political impact of unions at the establishment level due to a lack of matched employer-employee data for political outcomes. We alleviate these constraints by constructing an establishment-level dataset linking union elections to campaign contributions of employees.

#### 3.1 Union Elections

Our analysis builds on a comprehensive dataset for the universe of U.S. union representation elections between 1961 and 2018. Specifically, we combine data collected by Henry Farber with public data from NLRB election reports.<sup>8</sup> Each data point represents a union election at a single establishment and contains vote counts for and against unionization, the dates of the petition filing and of the actual election, as well as the name of the union organization. Moreover, it includes the establishment’s name and address which we exploit to match campaign contributions.<sup>9</sup>

**Sample restrictions.** Before matching elections to campaign contributions, we impose several

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<sup>8</sup>We obtain the dataset originally assembled by Henry Farber from the replication package of Knepper (2020). The data contain information on elections held between 1961 and 2009. For elections between 2010 and 2018, we retrieve data from NLRB election reports available on <https://www.nlr.gov/reports/agency-performance/election-reports>.

<sup>9</sup>See Appendix C.1 for details on the union election data.

sample restrictions. First, we only consider elections held between 1985 and 2010. Given that our contribution data covers the years 1979-2016, this allows us to observe trends in contributions for three election cycles before and after each union election. Second, we follow [Frandsen \(2021\)](#) and restrict the sample to union elections where at least 20 votes were cast. This restriction ensures that winning establishments are affected by a non-trivial rise in union representation. Moreover, it helps to exclude small establishments which are more likely to have come into existence recently and have a lower probability of survival over our period of analysis. Third, following [Knepper \(2020\)](#) and [Wang and Young \(2021\)](#), we only keep the first union election in each establishment.<sup>10</sup> Excluding non-inaugural elections avoids having multiple observations for the same establishment with reversed treatment status over time and helps alleviating election manipulation issues if managers or unions learn how to apply manipulation tactics in repeat elections. Our estimates should thus be interpreted as the effects of winning a first union election.<sup>11</sup>

**Summary statistics.** Table 1, Panel A, shows summary statistics for characteristics of the matched 6,065 elections that are included in our final estimation sample (see details on the matching below). 44% of the elections were won by the union, with an average union vote share of 50%. On average, 119 votes were cast in each election which yields a total of 723,571 voters who participated in all elections of our sample.<sup>12</sup>

### 3.2 Campaign Contributions

To measure the political participation and ideology of employees, we use the Database on Ideology, Money in Politics and Elections (DIME) compiled by [Bonica \(2019\)](#).<sup>13</sup> DIME provides transaction-level data on campaign contributions that are registered with the FEC and other state and local election commissions. We exploit the universe of campaign contributions from

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<sup>10</sup>In the election data, we identify an establishment as a unique address or a unique combination of the standardized firm name and commuting zone. For a firm that has multiple establishments within the same commuting zone, we thus only consider the first election among these establishments.

<sup>11</sup>This does not perfectly correspond to the effects of union representation in all post-election periods due to two reasons. First, establishments may lose representation after a decertification election but we keep those in the treatment group. Second, establishments, that after losing the first election, hold another successful election are kept in the control group. We thus accept an attenuation bias in our estimates relative to the effect of union representation.

<sup>12</sup>Appendix Figure A.1 investigates whether union elections follow political cycles. There are no strong differences in the number of hold union elections and the probability of winning a union election across years with and without federal elections, in particular not around the week of federal elections. Thus, we do not see evidence that employers or unions successfully manipulate union election dates to change union support around federal election cycles.

<sup>13</sup>Other papers have used these data to study, among others, the political consequences of import competition ([Autor et al., 2020](#)), immigration ([Dreher et al., 2020](#)), contribution limits ([Barber, 2016b](#)), advertising firms ([Martin and Peskowitz, 2018](#)), or consultant networks ([Nyhan and Montgomery, 2015](#)).

individuals to all candidates running for office at the federal and local level (specifically the House of Representatives, Senate, President, Governor, and upper and lower chambers of state legislature), as well as to all PACs (including single-party or single-candidate and interest-group PACs). The dataset covers the 1979-2016 period and includes the amount and exact date of the donation, as well as identifying information on the donor and recipient.

Campaign contribution data provide unique advantages for studying the political effects of unionization. First, to ensure transparency in politicians’ campaign funds, contributors are required to disclose their name, employer, address, and occupation.<sup>14</sup> The employer and location information allows us to link donors to union election results of their employers. We are not aware of any other large-scale data on political behavior with employer information in the U.S. which would allow this link. Further, we can use occupation information to study the effects of unionization not only on directly affected non-managerial workers but also on potentially indirectly affected managers and supervisors. Second, Bonica (2019) deploys identity resolution techniques to assign unique identifiers to each donor. The identifiers allow us to track donors’ contributions over time which we exploit to study whether establishments-level effects are driven by compositional changes from leaving and newly hired employees. Finally, the DIME includes measures for the political ideology of recipients and donors, so-called campaign finance (CF) scores, which are derived by Bonica (2014) from solving a spatial model of contributions. The model formalizes the idea that donors contribute more to candidates with a similar ideological position and estimates ideal points of both recipients and donors along a typical liberal-conservative scale. Using the ideology scores, we can go beyond previous papers which only relate unions to Democratic versus Republican party affiliation and study how unionization affects ideological contribution patterns for candidates within the same party.

**Matching algorithm.** We link the campaign contributions to the employing establishments with union elections by combining a spatial match with a fuzzy match of firm names. We start by restricting potential matches to the same local labor market using 1990 commuting zones. 92% of the population live and work in the same local labor market, making it very likely that a donor in our sample works at an establishment in the same local labor market (Fowler and Jensen, 2020). The restriction substantially reduces the computational requirements for the fuzzy match and ensures that for multi-establishment firms we do not wrongly match employees to establishments of the same firm in other locations.<sup>15</sup> For matching the employer name in the

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<sup>14</sup>Accurate reporting of this information is enforced by the FEC through regular audits, as well as fines and further legal action in case of non-compliance. See FEC (2022) for enforcement statistics.

<sup>15</sup>We accept measurement error from assigning donors to the wrong establishment if a firm has several es-

contribution data to the establishment name in the union election data we use an automated record-linkage program introduced by [Blasnik \(2010\)](#) and [Wasi and Flaaen \(2015\)](#). The linkage process first standardizes employer names and then calculates bigram scores for the similarity of each string pair. Lastly, we manually review all matches with a score above a minimum threshold.<sup>16</sup>

To arrive at an establishment-level panel of employee contributions, we sum up all matched contributions within an establishment and two-year election cycle. Our period of analysis covers three cycles before to three cycles after each union election. Moreover, we only include establishments for which we have at least one matched contribution over this period.<sup>17</sup> This leaves us with an estimation sample of 6,065 matched establishments (and 42,455 establishment-cycle observations). As Panel B of Table 1 reports, our sample is built from 357,434 matched contributions that amount to 88.8 million USD spent by 46,719 different donors to 9,942 different recipients.

**Classification of occupations.** In order to differentiate between workers eligible for unionization and their managers and supervisors who are always excluded from the bargaining unit, we classify self-reported occupations of donors. Here we only briefly describe the classification procedure and provide more details in Appendix C.3. We start by mapping the free-text occupation descriptions in the DIME to the 6-digit Standard Occupation Classification (SOC). For this, we combine an ensemble classifier called SOCcer ([Russ \*et al.\*, 2016](#)), sub- and fuzzy string matching to an extensive crosswalk of laymen’s occupation titles from O\*NET, as well as manual reviews of the most common occupation titles. We are able to assign a SOC code to 72% of all candidate contributions in our matched sample. Appendix Figure A.2 shows the occupation distribution for those classified donations. While the largest share (44%) is spent by donors in management occupations, we also see substantial shares of contributions originating from lower-tier white-collar occupations such as healthcare, education, culture and sports, or financial operations workers. Blue-collar occupations, in contrast, account for small shares of the overall number of contributions, which is not surprising given that wealth is a strong predictor of political donating.

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establishments within a commuting zone. However, within-firm interactions may generate spillover effects across establishments. The results of [Knepper \(2020\)](#), for example, imply large spillovers in the effects of unionization on firm-level employee compensation.

<sup>16</sup>See Appendix C.2 for details on the matching process.

<sup>17</sup>Appendix Table A.1 compares characteristics of matched and non-matched establishments. Elections in our matched sample involve more voters, i.e., are likely to be larger, and tend to be held in more recent years as contribution numbers have sharply increased over time. At the same time, the matching does not appear to strongly affect the selection of union elections in terms of voting outcome and industry composition.

With the classified SOC codes at hand, we differentiate between occupations eligible and not eligible for unionization. In particular, we categorize donors into managers and supervisors versus rank-and-file workers. The NLRA excludes supervisors, which are seen as representing a firm’s management, from joining a union. We identify contributions from managers and supervisors by first using all contributions from “Management Occupations” (SOC group 11). Then, we add to the group all occupations that involve a significant amount of supervising following the NLRA definition of supervisor-tasks. To identify occupations where supervisor-tasks are of importance, we leverage relevant occupational task descriptions from O\*NET.<sup>18</sup> Finally, we identify as rank-and-file workers all remaining donors to whom we were able to assign a SOC code. With these definitions, we obtain the following occupational composition in our sample of candidate contributions: 42% of contributions originate from managers and supervisors (hereafter only termed “managers”), 30% from rank-and-file workers (hereafter only termed “workers”), and for 28% we are unable to obtain a classification. Due to the non-negligible share of unclassified occupations, we report results not only separately for managers and workers, but also for all employees together (including those without a classification).<sup>19</sup>

Table 2 reports mean contribution amounts after aggregation at the establishment-election cycle level. Managers spent on average 1,339 USD per cycle, while workers contribute 314 USD. Both groups support different recipients. The majority of contributions by managers are donated to Republican candidates (54%), whereas workers tend to favor Democratic candidates (65% of the average amount is donated to Democrats). Moreover, managers spent a larger share of donations to committees than to candidates. In contrast, workers more often contribute directly to candidates.

## 4 Empirical Strategy

We aim at estimating the causal effect of unionization on political participation and ideology of employees. A simple comparison of individuals in unionized and non-unionized workplaces will fail to account for differences between these groups along a number of dimensions. These arise, since the decision to unionize is likely endogenous and correlated with many characteristics, among them potentially political behavior. Figure 1 depicts average campaign contribution amounts across winning and losing union elections before and after the election. Due to their shared interest in a union election at the same time, these establishments are expected to be more

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<sup>18</sup>See Appendix C.3 for details on the standardization of occupations and the classification of supervisors.

<sup>19</sup>In Appendix C.2 we also provide evidence that the likelihood of a missing occupation classification is not affected by unionization and therefore unlikely to drive our results.



similar than a random sample of unionized and non-unionized establishments.<sup>20</sup> Pre-existing ideological differences are nevertheless visible: Workplaces that vote for unionization donate more to Democratic candidates and less to Republican candidates already before the union election.

To account for pre-existing differences, we implement a Difference-in-Differences (DiD) approach and compare campaign contribution patterns before and after the union election in establishments where the union won versus where it lost. We complement the DiD design with methods originating from the Regression Discontinuity (RD) literature to probe the validity of the underlying parallel trends assumption. In particular, we exploit that we observe the pro-union vote share that discontinuously determines unionization at the 50% threshold. We use the vote share to estimate placebo tests for differential trends by vote shares among losing union elections as well as to examine the robustness of our DiD estimates when restricting the sample to establishments with increasingly close election results.<sup>21</sup>

**Stacked DiD.** We start by estimating the following stacked DiD model:

$$y_{ik} = \alpha_i + \beta_{kg_i} + \delta_{\text{DiD}} \times \left( \mathbb{1}[k \geq 0] \times \mathbb{1}[V_i > .5] \right) + \epsilon_{ik}, \quad (1)$$

where  $y_{ik}$  denotes a political outcome of employees in establishment  $i$  and relative event time  $k$ . We observe each establishment for three cycles before to three cycles after the union election, i.e.,  $k = \{-3, -2, \dots, 3\}$ , where  $k = 0$  refers to the cycle in which the union election takes place. Our effect of interest is captured by  $\delta_{\text{DiD}}$ . It is the coefficient of an interaction term between a post-treatment dummy and a dummy indicating whether the election was won by the union, i.e., by whether the pro-union vote share,  $V_i$ , is above 50%.  $\alpha_i$  denotes establishment fixed effects that capture all time-invariant differences between winning and losing establishments. Further, we introduce event-time  $\times$  cohort fixed effects  $\beta_{kg_i}$ , where cohort  $g_i$  refers to the election cycle in which the union election was held, i.e.,  $g_i = \{1985/86, 1987/88, \dots, 2009/10\}$ . Importantly, with these fixed effects our identifying variation only comes from comparing changes across winning and losing elections within the same cohort. Thereby, it avoids “forbidden comparisons” between late and early-treated establishments that may lead to negative weights when averaging

<sup>20</sup>Dinlersoz *et al.* (2017) examine selection into union election and find that elections are more likely to be held at younger, larger, more productive, and higher-paying establishments. Our strategy avoids such selection by comparing only establishments which hold union elections.

<sup>21</sup>Many papers on the effects of unionization follow RD designs by comparing establishments where the union barely won versus where it just lost (e.g. Campello *et al.*, 2018; DiNardo and Lee, 2004; Ghaly *et al.*, 2021; Lee and Mas, 2012; Sojourner *et al.*, 2015; Sojourner and Yang, 2022). This approach is complicated by the fact that unions and employers can influence election outcomes even after the election, through challenging the validity of individual ballots or filing charges of unfair labor conditions. Frandsen (2021) and Knepper (2020) provide evidence for discontinuities at the 50% threshold in the vote share distribution as well as in pre-election establishment characteristics. Figure A.3 verifies that also in our matched sample of elections there is a significant discontinuity in the vote share density at the 50% cutoff which indicates a manipulation of close elections.



potentially heterogeneous, cohort-specific treatment effects in staggered DiD settings such as ours (de Chaisemartin and D’Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). Our DiD model is equivalent to the stacking approach first implemented by Cengiz *et al.* (2019). This approach first creates cohort-specific datasets of treated units and an appropriate set of control units that are never or not-yet-treated. Then, one stacks the cohort-specific datasets in relative time to the treatment start in order to estimate an average treatment effect across all cohorts. By stacking and aligning cohorts in relative time, this strategy mimics a setting where all treatments occur contemporaneously, and thus avoids using already-treated units in the comparison group. Note that in our case the selection of appropriate control units for the stacking is facilitated by the possibility that we can naturally compare treated establishments to untreated establishments that have a lost election in the same cycle. Finally, we cluster standard errors at the level of treatment, the establishment.

Model (1) pools all periods after treatment which yields the maximum power when estimating average treatment effects. To examine how treatment effects vary by event time, we also estimate the following stacked event-study model:

$$y_{ik} = \alpha_i + \beta_{kg_i} + \sum_{s=-3, s \neq -1}^{s=3} \delta_s \times \left( \mathbb{1}[k = s] \times \mathbb{1}[V_i > .5] \right) + \epsilon_{ik} \quad (2)$$

where the  $\delta_s$  coefficients capture dynamic treatment effects relative to the cycle before the union election was held (the interaction with  $k = -1$  is omitted).

**Parallel trends assumption.** Our identifying assumption is that campaign contributions for winning establishments would have evolved in parallel to contributions in losing establishments had the union not won the election:

$$E[Y_{i,k \geq 0}^0 - Y_{i,k < 0}^0 | V_i > .5] = E[Y_{i,k \geq 0}^0 - Y_{i,k < 0}^0 | V_i \leq .5]$$

where  $Y_i^0$  denotes the potential outcome of an establishment if the union loses the election.

We run different tests to examine the validity of this assumption. First, we analyze whether outcomes have developed in parallel before the election. Figure 1 provides first visual evidence that pre-election changes in contribution amounts to Republican and Democratic candidates are very similar across winning and losing elections. The pre-election  $\delta_s$  coefficients estimated in the event-study model will provide a formal test of pre-trends.

Second, even in absence of significant pre-trends there may still be unobserved shocks that drive union voting results at the time of the election and that may be related to changes in

contribution patterns. To test whether such shocks likely violate our identifying assumption, we follow the approach of Wang and Young (2022) and analyze whether changes in outcomes are different among losing elections with different vote shares. If unobserved shocks were driving voting results that lead to union victory or loss, we would expect that they also affect outcomes in losing elections with different union vote shares.<sup>22</sup> To implement this test, we modify the DiD model as follows:

$$y_{ik} = \alpha_i + \beta_{kgi} + \sum_g \delta_g \times \left( \mathbb{1}[k \geq 0] \times \mathbb{1}[V_i \in \nu^g] \right) + \epsilon_{ik}, \quad (3)$$

where  $\nu^g$  denotes a complete set of vote share categories. In particular, we divide the vote share distribution into the following six groups: 0-20%, 20-35%, 35-50%, 50-65%, 65-80%, 80-100%. In the model we omit the 20-35% vote share category, such that all estimated effects must be interpreted relative to that group. Significant estimates for the 0-20% or 35-50% categories would then indicate the presence of unobserved shocks that drive both voting results and campaign contribution behavior.

Finally, we relax the parallel trends assumption by restricting the sample to elections where the union won or lost by an increasingly close margin. Establishments with closer election results can be expected to be more similar not only in terms of baseline characteristics but also in terms of shocks that they are exposed to over time. Specifically, we examine the robustness of the DiD estimates when restricting the sample to increasingly small vote share bandwidths around the 50% cutoff. In the limit, when comparing establishments where the union barely lost versus where it just won, we approach the discontinuity-in-differences model estimated by Frandsen (2021) and Knepper (2020). For our baseline results from models (1) and (2), however, we follow Wang and Young (2021) and consider all elections with a pro-union vote share between 20% and 80%. This improves power and allows us to generalize effects for a broader sample of union elections.

**Definition of outcome variables.** We consider two main outcomes of employees' political behavior at the establishment level. The first one is the total amount of campaign contributions to all political candidates which we interpret as a measure of political participation and mobilization of employees. We use the inverse hyperbolic sine (IHS) transformation to approximate log changes in contribution amounts, while retaining zero values.<sup>23</sup> Our second main outcome is

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<sup>22</sup>Wang and Young (2022) formulate the identifying assumption as parallel trends across all vote shares, i.e.,  $E[Y_{i,k \geq 0}^0 - Y_{i,k < 0}^0 | V_i] = E[Y_{i,k \geq 0}^0 - Y_{i,k < 0}^0]$ , which yields the testable implication that trends should be parallel between losing elections with different vote shares.

<sup>23</sup>The inverse hyperbolic sine function is defined as  $IHS(x) = \ln(x + \sqrt{x^2 + 1})$ . For sufficiently large  $x$ ,  $IHS(x) \approx \ln(x) + \ln(2)$ . The function thus approximates the natural logarithm function for positive values but is also well defined for zero values. Applied econometrics papers frequently apply it to transform non-negative

the difference between the IHS-transformed contribution amounts to Democratic and Republican candidates. This measure proxies the percentage difference in support for Democrats versus Republicans. Given the extant evidence on ideological motivations driving individuals' donation behavior, we interpret it as a measure of employees' ideological position.

## 5 Results

### 5.1 Main Results

Figure 1 presents first descriptive evidence on the political impact of unionization by displaying trends in mean contribution amounts from all employees of an establishment to Republican and Democratic candidates. Before the election, contributions develop very similarly in establishments where union elections are won and where they are lost. The strong upward trend is explained by the fact that campaign contributions have strongly gained in importance in more recent election campaigns. At the time of the election, we see that contribution patterns start to diverge between winning and losing elections. The rise in donations to Republicans appears considerably smaller in unionized than in non-unionized establishments. In contrast, donations to Democrats seem to slightly increase in winning relative to losing union election establishments. Overall, the figure suggests a shift of contributions from Republican to Democratic candidates after successful unionization.

We now turn to the estimation results from the stacked DiD and event-study models (1) and (2). Figure 2 displays the dynamic treatment effects  $\delta_s$  along with the pooled average treatment effect  $\delta_{\text{DiD}}$ . We start with the effects on the total amount of campaign contributions depicted on the left-hand side of the figure. The upper panel plots the results for all employees in an establishment. Note the absence of any significant differential trends between establishments winning and establishments losing the union election in the three election cycles (six years) before the election. The effect of unionization on the amount of contributions is small and insignificant in all post-election periods, but we see a moderate spike in contributions in the cycle of the election (which we are not able to estimate precisely, though). Differentiating between contributions made by workers and managers in the lower panels highlights that workers drive the increase in contributions in the cycle of the union election. This pattern is consistent with a short-run political mobilization of workers through a successful union campaign at the workplace. Overall, however, the DiD coefficients indicate that there is no significant average effect on the amount of contributions over the six years after a union election.

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variables with zeros (e.g. Bahar and Rapoport, 2018; Bursztyn *et al.*, 2022; McKenzie, 2017).

We continue with assessing compositional changes in campaign contributions. If unions are able to change individuals' political views or mobilize different subgroups at the workplace, campaign contributions would shift to different candidates. The right-hand side of Figure 2 plots the effect of unionization on the difference between the (IHS-transformed) amounts spent to Democratic versus Republican candidates. First focusing on all employees, we again see no differential trends in contribution composition before the election. After the election, however, there is a significant increase of contributions donated to Democratic relative to Republican candidates. The effect on partisan support appears to be strongest in the long run, i.e., six years after the election. The DiD coefficient indicates that over all post-election periods unionization increases the difference in contributions to Democrats versus Republicans by 24 percentage points (significant at the 1% level). Differentiating again between workers and management in the lower two panels reveals that the effect is driven similarly by both groups. Not only workers, but also managers significantly shift contributions from Republican to Democrat candidates in response to successful unionization. Quantitatively, the DiD estimates show that winning the union election increases donations to Democrats relative to Republicans by 12 percentage points for workers and by 20 percentage points by managers (both significant at the 1% level). These patterns are not consistent with an increase in tensions between unionized workers and their management but rather point towards an alignment of political preferences.

Next, we present results on our RD-motivated tests to probe the validity of the underlying parallel trends assumption and to test for potential heterogeneities in treatment effects among elections won by large versus small margins of support. Figure 3 focuses on the results for our measure of partisan contribution composition, while results for the total amount of contributions are presented in Appendix Figure A.4. Results are always reported separately for workers and managers. We first analyze the heterogeneous effects of unionization across the vote share distribution. Panel (a) of Figure 3 displays the  $\delta_g$  coefficients from model (3) on the interaction between the post-election dummy and different vote share categories. The results show that there are no significantly different trends among losing elections with a vote share of 0-20% or 35-50% relative to those with 20-35%, both for contributions from workers and managers. The post-treatment partisan contribution composition thus appears to evolve similarly across losing establishments with different vote shares. Therefore, we do not find evidence for unobserved shocks correlated with voting results that could drive our results.<sup>24</sup> Moreover, the results

<sup>24</sup>In Appendix Figure A.5 we also investigate whether pre-trends in the contribution composition are similar across the vote share distribution. For this, we estimate the following modified version of model (3):

$$y_{ik} = \alpha_i + \beta_{kg_i} + \sum_g \delta_g^{PRE} \times \left( \mathbb{1}[k < -1] \times \mathbb{1}[V_i \in \nu^g] \right) + \sum_g \delta_g^{POST} \times \left( \mathbb{1}[k \geq 0] \times \mathbb{1}[V_i \in \nu^g] \right) + \epsilon_{ik} \quad (4)$$

indicate whether treatment effects are heterogeneous across vote shares among winning union elections. For the composition of contributions from managers, the estimate is significant across all vote share categories above 50%. Thus, the political response of managers does not appear to depend on whether workers won the union election with large or small margins of victory. For workers, the effect on partisan support is significant only for vote shares between 50 and 80% and appears smaller for elections won by a large margin.

Panel (b) of Figure 3 presents coefficients from the DiD model (1) when restricting the sample to establishments with increasingly close election results. Establishments with more similar voting results can be expected to be more similar in other characteristics and to be exposed to more similar shocks which makes the parallel trends assumption more plausible. Results are reported in 5% steps of the union vote share bandwidth around the 50% cutoff. Our baseline results from Figure 2 included only elections with a pro-union vote share between 20 and 80%, i.e., a bandwidth of 30%. Figure 3 shows that treatment effects are very similar when instead using all elections. More importantly, the results are also very stable when focusing on closer elections. Even when restricting the sample to establishments that won with a maximum vote margin of 5%, we see a positive and significant effect on the composition of campaign contributions for managers. Similarly, for workers a maximum vote margin of 10% already yields a positive and significant effect.

## 5.2 Robustness

We now discuss further robustness checks for our main DiD estimates. Results are presented in Appendix Tables A.2, A.3, and A.4.

**Alternative staggered DiD estimators.** The recent econometrics literature has proposed different methods to circumvent issues of treatment effect heterogeneity in staggered DiD designs. All the proposed estimation strategies have in common that they restrict the set of effective comparison units by ruling out that early-treated units are used in the estimation of treatment effects for currently-treated units. They differ, however, in terms of how exactly comparison units are identified and used in the estimation, as well as in terms of how cohort- or individual-specific treatment effect estimates are aggregated.<sup>25</sup> In Panels B and C of Appendix Table A.2,

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The results show that none of the estimated  $\delta_g^{PRE}$  coefficients is significantly different from zero which indicates that also before the union election contribution patterns evolved similarly across establishments with different voting results.

<sup>25</sup>In our stacking approach of model (1), we effectively only compare winning elections to losing elections that were held in the same period, i.e., we only use never-treated units in the comparison group. The strategies by Borusyak *et al.* (2021) and Callaway and Sant’Anna (2021), in contrast, also allow including not-yet-treated units in the comparison group. Both approaches differ in that Borusyak *et al.* (2021) use the average pre-treatment

we present results from the imputation approach of [Borusyak \*et al.\* \(2021\)](#) and the estimator developed by [Callaway and Sant’Anna \(2021\)](#). The estimates are very similar to our baseline results.

**Alternative outcome transformations.** [Roth and Sant’Anna \(2021\)](#) point out that the parallel trends assumption of a DiD design generally implies a functional form restriction on potential outcomes. Transformations of the outcome may imply different parallel trends assumptions. We therefore test the sensitivity of our results to alternative outcome transformations. First, instead of transforming contribution amounts by the IHS function, we use the log function and add one to the amounts to retain zero values. Second, we leave amounts untransformed (in 2010 USD). Results, shown in Panels D and E, yield qualitatively the same conclusions as the results for the IHS-transformed outcomes.

**Alternative manager-worker classifications.** In Appendix Table [A.3](#), we check whether our results are sensitive to the exact definition of managers/supervisors versus rank-and-file workers. To see whether the political response is different for lower- and upper-tier managers, we use more stringent definitions of managers/supervisors. First, we vary the cutoff for the importance of supervisor tasks (Panels B and C). Second, we only consider “Management Occupations” (SOC group 11) and treat all other occupations (including those with a high importance of supervisor tasks) as workers (Panel D). The results do not change much with these alternative classifications. Even for more upper-tier managers unionization leads to an increase in the support of Democrats relative to Republicans.

**Contributions to federal versus local candidates.** We also study whether our effects are limited to contributions towards candidates in either federal or local (i.e., state) elections. U.S. legislation on labor issues, which unions may particularly focus on when endorsing candidates and policies at the unionized workplace, is enacted not only at the federal level, but also at the state-level (e.g., state-specific minimum wages, right-to-work laws). In line with this, Panels F and G of Appendix Table [A.2](#) show that our estimates are driven by contributions to both federal and local candidates. Effect sizes are a bit larger for contributions to candidates running for federal offices, but at both levels we see a significant shift in donations from Republicans to

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outcome over all pre-treatment periods, whereas [Callaway and Sant’Anna \(2021\)](#) use only the outcome one period before treatment start. In terms of aggregation, [Gardner \(2021\)](#) shows that the stacking approach identifies a convexly weighted average of cohort-specific treatment effects where the weights are given by the number of treated units and the variance of treatment within each cohort. In comparison, [Borusyak \*et al.\* \(2021\)](#) and [Callaway and Sant’Anna \(2021\)](#) first estimate unit- or cohort-specific effects and then aggregate by a simple average across treated units. [Callaway and Sant’Anna \(2021\)](#) also allow other weights, but we use the default option where cohort-specific estimates are weighted by the number of treated units in each cohort.

Democrats in response to unionization (and no effect on total amounts).

**Effects of losing a union election.** Our DiD results measure the differential change in contributions from establishments where the union won versus establishments where the union lost the election. The observed relative shift in donations could not only be explained by the effects of unionization after winning the election but also by an effect of holding and losing an election. Interaction with the union organization in preparation of the union election as well as a potentially increased salience of worker issues and distributional conflicts may affect the political behavior of employees, in particular in the short run, even if the union election is lost. We test this by estimating the effects of losing an election compared to holding no election. To avoid selection into which establishment hold (and lose) an election, we exploit only variation in the timing of union elections and use establishments who hold (and lose) an election in the future as control group. Given that we observe each establishment up to three cycles before the union election, for treated elections held in a given cycle, we can use elections held in the two cycles ahead as control units in a DiD design. This allows us to examine short-run effects of losing an election (for event times  $k = 0, 1$ ). We implement this approach in a stacked DiD model similar to our baseline model (1).<sup>26</sup> Results are presented in Appendix Table A.4. We obtain small and throughout insignificant estimates for our two main outcomes and for both workers and managers with a precision similar to our baseline results. This suggests that losing a union election can indeed be viewed as an untreated counterfactual and that our results are driven by the effect of unionization after winning a union election.

Overall, our estimates provide robust evidence that unionization changes the composition of employees' campaign contributions in favor of Democratic (relative to Republican) candidates. Importantly, this effect is robustly found for both workers and managers.

## 6 Potential Mechanisms and Extensions

In this chapter, we aim at providing evidence on what mechanisms may drive the positive effect of unionization on support of Democratic relative to Republican candidates for workers and managers. Moreover, we examine whether the observed changes in contributions towards candidates extend to contributions towards PACs.

**Compositional versus individual-level effects.** One potential explanation for the establishment-

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<sup>26</sup>See Appendix B.1 for details of the stacking implementation. We also implement the DiD estimators by Borusyak *et al.* (2021) and Callaway and Sant'Anna (2021) which yield similar results.

level effects may be compositional changes regarding which employees separate from and are newly hired in unionized establishments. Frandsen (2021) shows that unionization leads older and higher-paid workers leave and younger workers join union jobs. Separations and hirings may also be selective in terms of political ideology. For example, conservative union-avoiding managers may want to leave unionized workplaces and may be replaced with more liberal ones. If this is the case, our establishment-level results may be fully explained by composition effects rather than by individual-level changes in political behavior. To differentiate between the two, we exploit the donor identifiers in the DIME which allow us to track a donors' contributions over time.

First, we seek to examine pure composition effects. In other words, we take out any direct effect on individuals in unionized workplaces. For this, we modify the construction of our establishment-level aggregates of employee donations in the following way. For each post-election event time  $k \geq 0$ , we still consider the set of donors that have at least one contribution matched to the respective establishment in that period. Then, instead of using these donors' contributions in that period, we trace their contributions before the election (in the three pre-election cycles) and use them in the establishment-level aggregation. As a result, the post-election aggregates only reflect pre-existing contribution patterns. We use them along with the actual pre-election aggregates (constructed as before from the actual matched contributions in those periods) in our DiD model. Results, presented in Table 3, columns (1) and (2), show very small and almost always insignificant DiD estimates, indicating that the set of post-election workers does not differentially change in unionized versus non-unionized establishments in terms of pre-existing contribution amounts.<sup>27</sup> Only for workers, we see a marginally significant estimate in line with more Democratic workers entering union jobs (or less Democratic workers leaving union jobs). The effect size, however, is much smaller than in our main estimates which suggests that composition effects are unlikely to fully explain the results.

Second, we aim at directly studying employee-level effects of unionization, i.e., we consider the direct effect of unionization on individuals. For this, we focus on a sample of individuals who are employed in the same establishment before and after the union election, which we identify as having at least one matched contribution to the same union election establishment at least once before and once after the union election. We then aggregate all matched contributions of these individuals over our 7-cycle-window into one pre- and one post-election observation and estimate a two-period DiD (with individual and cohort  $\times$  post-election fixed effects).<sup>28</sup> Estimates

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<sup>27</sup>While we focus here on candidate contributions, Appendix Table A.5 also reports results for PAC contributions. Also for these, we find little evidence that compositional effects drive our results.

<sup>28</sup>We refrain from aggregating contributions for each relative cycle  $k$  separately. If individuals do not donate



are reported in columns (3) and (4) of Table 3. For all employees jointly, we find no significant effect on the total contribution amounts but a significant increase in the amount donated to Democratic relative to Republican candidates.<sup>29</sup> When restricting the sample to workers, we see a (marginally significant) rise in total spending, which is, however, entirely driven by an increase in support of Democrats. For managers, the results indicate a shift from Republicans to Democrats without a change in total amounts. Overall, the results point to the conclusion that our establishment-level effects are driven by individual-level changes in spending patterns rather than by compositional effects.<sup>30</sup>

**Ideological shifts.** Our main results show that unionization increases support of Democratic relative to Republican candidates. The change in party composition may reflect a change in employees’ ideological position or merely an increased signaling of party affiliations. To further examine the ideological patterns in campaign contributions, we study within-party ideological differences of candidates. For this, we make use of Bonica’s (2014) CF scores that assign each recipient an ideal point along a liberal-conservative scale. Democratic candidates are categorized as “moderate” versus “liberal” if their CF score lies above the median CF of all Democrats observed in our sample of matched contributions. Similarly, we distinguish “moderate” versus “conservative” Republicans using the median Republican CF score. Table 4 shows results from our DiD model (1) where the outcome is the amount contributed to each of the candidate types. Considering first all employees jointly, we see strong differences in the effects of unionization by within-party ideological positions of candidates. Unionization significantly increases employees’ support of the most liberal Democrats and decreases support of the most conservative Republicans. In contrast, contributions to moderate Democrats or Republicans are not significantly affected. These results are very similar when we focus on donations by managers only, and also for workers the increased support of Democrats is more pronounced for more liberal Democrats. Overall, our effects appear to be driven by a shift in contributions from clearly distinguishable conservative to liberal candidates (instead of a shift at the margin from moderate Republicans to moderate Democrats).

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in a given cycle, we do not know their employing establishment. We would therefore not be able to construct a balanced panel over all cycles which includes observations with zero amounts.

<sup>29</sup>Note that the substantially larger magnitude of the estimates in comparison to the establishment-level results is likely because we have aggregated all pre- and post-election cycles for the individual-level analysis.

<sup>30</sup>Another composition effect potentially explaining our establishment-level result may arise from transitions of individuals across occupational groups. To rule out that promotions of workers to management positions are driving our results for managers, in columns (3) and (4) of Table 3 we have classified individuals as “managers” only if they hold a manager position both before and after the election. On the other hand, individuals who have some matched contributions with an occupation categorized as “manager” and some categorized as “worker” are all included in the “workers” subsample.

**Contributions to committees.** So far, we have considered only contributions to candidates running for political offices. In Table 2 we have shown that contributions to PACs also account for a large share of political contributions by employees. If unions particularly encourage workers to contribute to candidates, this may come at the detriment of workers’ contributions towards committees. On the other hand, if unions mobilize workers to participate by donating to labor PACs, then we would underestimate the total effect of unionization on political donations. Table 5 reports DiD estimates from model (1) for PAC contributions. We distinguish between single-party/candidate PACs and interest-group PACs, where the latter are further disaggregated into corporate, trade association, membership organization, and labor organization PACs. Besides considering the total amount to these committees, we also measure partisan support by the difference in contribution amounts towards Democratic versus Republican PACs. For interest-group PACs, party affiliation is determined from the recipients of the PAC’s own campaign contributions.<sup>31</sup> Considering first contributions from all employees of an establishment to party/candidate PACs, the results mimic those for candidate contributions. While there is no effect on total amounts, unionization leads to a significant shift from Republican to Democratic committees. Among interest-group PACs, there is a significant decrease in donations to corporate PACs. When distinguishing between donations from workers and managers, results differ somewhat. For workers, we see a significant increase in the total amounts spent to both party/candidate committees and interest-group PACs which implies that unions are successful in mobilizing PAC contributions by workers. The increase in spending appears to be driven by membership and labor organizations, pointing towards an increased support for civil society and labor interest groups. In contrast to our results on candidates, however, we do not see a significant shift across party affiliations. For managers, the results are very similar to those on candidate contributions. While there is no effect on overall PAC spending, managers increasingly donate towards Democratic rather than Republican-affiliated PACs. In particular, donations towards corporate PACs drop which highlights that unionization can decrease managers’ support of business interest groups. Overall, these results match with the observed pro-liberal shift in workers’ and managers’ contributions to political candidates.

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<sup>31</sup>To track contributions that PACs send themselves, we exploit that Bonica (2019) has matched recipient identifiers to contributor identifiers for recipient’s own contributions. Based on the matched outgoing contributions of PACs, we define an interest-group PAC as “Democratic” (“Republican”) if its campaign contributions go to more (less) than 50% to Democratic candidates in a given election cycle.

## 7 Conclusion

Labor unions employ vast resources to influence legislation and aggregate income inequality patterns suggest they are successful (Farber *et al.*, 2021). To understand the political power of labor unions it is important to understand their influence on millions of affected individual members and non-members. One likely channel through which unions sway elections is their influence of political behavior of individuals at the workplace. Importantly, their aggregate impact on politics hinges on their effect on the in-group as well as the reaction of the out-group. It is thus important to understand their impact on unionizing workers as well as non-unionizing managers.

We build an establishment-level dataset combining the universe of union elections with transaction-level campaign contribution data spanning the 1980-2016 period in the United States. Our data enable us to estimate a stacked DiD model comparing establishments with an interest in unionization that won and lost the union election. We find that unionization increases contributions to Democratic candidates relative to Republican candidates by 12 percentage points for workers and 20 percentage points for managers, while we do not find a permanent impact on the overall amount of contributions. These effects do not seem to be driven by a change in the composition of donors but hold at the individual level. Overall, we show that labor unions do not only influence the political behavior of union members but also of their firms' management.

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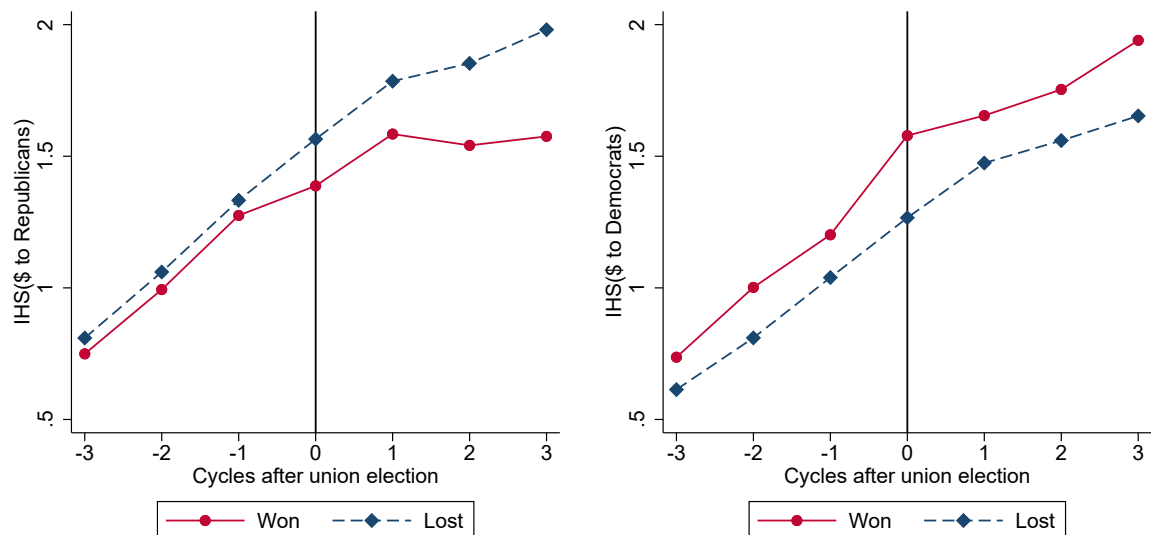
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## Figures and Tables

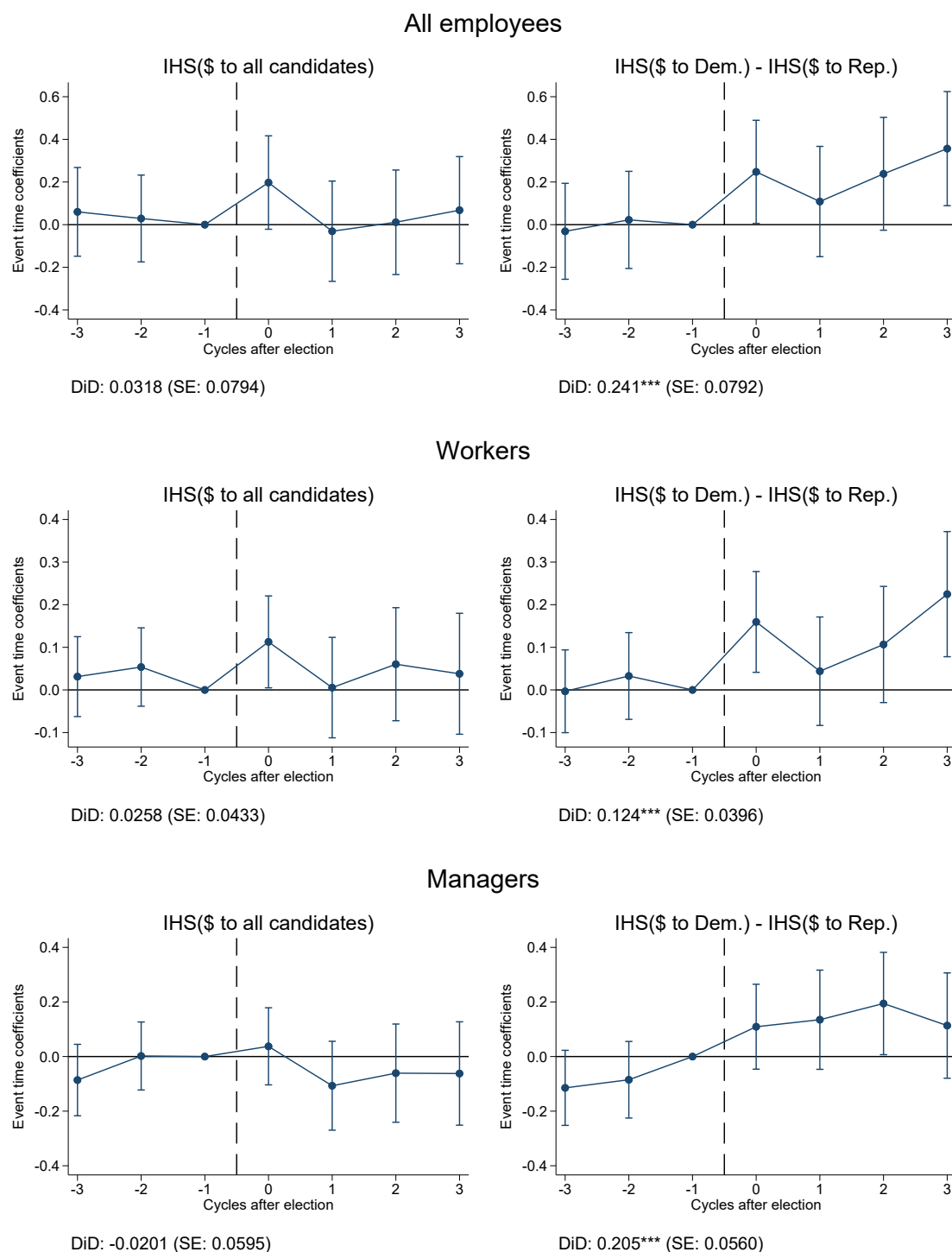
Figure 1: Trends in Contributions for Won and Lost Union Elections



**Notes:** The figure depicts trends in mean contribution amounts of all employees in an establishment by union election outcome and election cycles relative to the union election. The left (right) graph shows means of IHS-transformed amounts to Republican (Democratic) candidates.  $N = 42,455$  establishment-cycle observations.

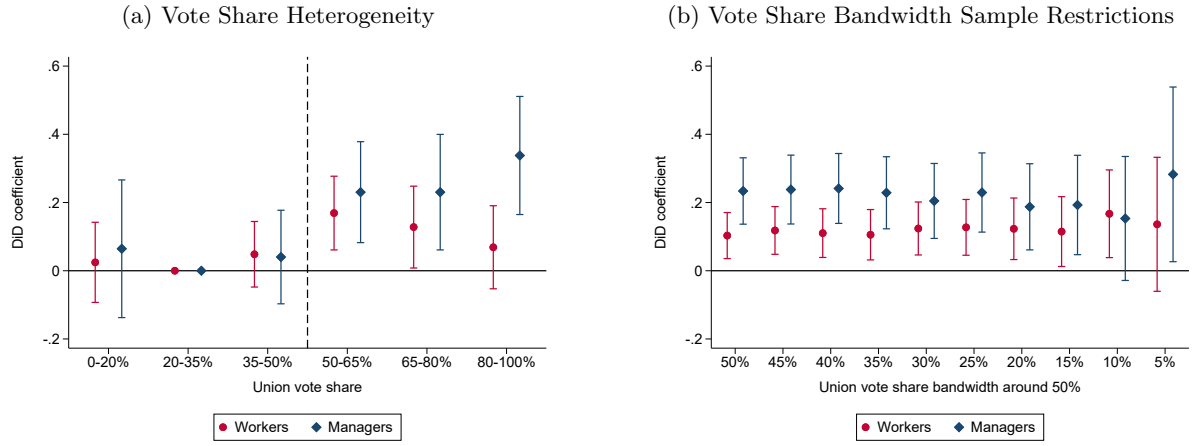


Figure 2: Effects of Unionization on Candidate Contributions



**Notes:** The figures report the  $\delta_s$  event-study coefficients from model (2). The sample includes all establishments with a pro-union vote share between 20% and 80% and covers three election cycles before and after the union election.  $N = 33,117$  establishment-cycle observations. Below each graph the DiD coefficient from model (1) is reported. In the graphs on the left side, the outcome is the IHS-transformed total amount contributed to all candidates. In the graphs on the right side, the outcome is the difference between the IHS-transformed amount contributed to Democratic and Republican candidates. Results are reported for contributions from all employees (top part), only from non-managerial workers (middle part), and only from managers and supervisors (lower part). 95% confidence intervals are depicted for standard errors clustered at the establishment level.

Figure 3: Effects of Unionization on Democratic versus Republican Support - Regression Discontinuity Tests



**Notes:** The graphs show RD-type placebo and robustness tests for the effect of unionization on the difference between the IHS-transformed amount contributed to Democratic and Republican candidates. Panel (a) reports the  $\delta_g$  coefficients from model (3). The vote share distribution is partitioned into six bins, indicated on the x-axis. The omitted reference group is 20-35%. Panel (b) reports DiD coefficients estimated in model (1). Each dot refers to a single DiD coefficient that is estimated among elections with a union vote share in a given bandwidth around the 50% cutoff. Estimates from smaller bandwidths compare changes between increasingly close elections. Results are always shown separately for contributions by non-managerial workers (“workers”) as well as managers and supervisors (“managers”). 95% confidence intervals are depicted for standard errors clustered at the establishment level.

Table 1: Election and Contribution Descriptive Statistics

	All	Union Loss	Union Win
[A] Election characteristics			
Number of elections	6,065	3,399	2,666
Union vote share (average)	.4950	.3204	.7175
Number of votes (average)	119.30	135.19	99.04
Number of votes (total)	723,571	459,523	264,048
[B] Contribution characteristics			
Amount (total, in million 2010 USD)	88.81	54.61	34.20
Number of contributions (total)	357,434	204,788	152,646
Number of donors (total)	46,719	26,656	20,248
Number of recipients (total)	9,942	7,205	5,682

**Notes:** Data from NLRB unions certification elections, which have at least one employee contribution matched in any of seven election cycles around the union election (three before, cycle of union election, three after). Contribution characteristics refer to the total numbers over all these seven election cycles.

Table 2: Contributions by Donor and Recipient

Donor: Recipient:	All employees	Workers	Managers
All	2,492.55	313.70	1,339.01
Candidates	1,181.70	173.37	594.31
Democratic candidates	575.72	112.75	261.69
Republican candidates	585.95	56.59	320.61
Political action committees	1,310.84	140.33	744.69
Party/candidate PACs	364.79	52.50	192.70
Interest-group PACs	936.92	86.34	549.13

**Notes:** The table reports mean values for the amount contributed in each of the 42,455 establishment-cycle combinations in the estimation sample. All amounts are in 2010 USD. Values are reported separately for contributions from all employees, only from non-managerial workers (“workers”), and only from managers and supervisors (“managers”). The difference in the amounts for all employees and the total of workers and managers is driven by contributions for which we were unable to classify the occupation.

Table 3: Composition versus Individual-level Effects

	Composition effects		Individual-level effects for stayers	
	All candidates (1)	Dem – Rep (2)	All candidates (3)	Dem – Rep (4)
<b>[A]: All employees</b>				
$\delta_{\text{DiD}}$	-0.0211 (0.0687)	0.0688 (0.0628)	0.222 (0.193)	0.547** (0.214)
N	33117	33117	5740	5740
<b>[B]: Workers</b>				
$\delta_{\text{DiD}}$	0.0470 (0.0357)	0.0526* (0.0290)	0.637* (0.340)	0.646** (0.328)
N	33117	33117	2052	2052
<b>[C]: Managers</b>				
$\delta_{\text{DiD}}$	-0.0614 (0.0506)	0.0379 (0.0447)	-0.0233 (0.194)	0.531* (0.288)
N	33117	33117	2890	2890

**Notes:** The table reports DiD coefficients for the effect of unionization on IHS-transformed contribution amounts. In columns (1) and (2), the establishment aggregates for the post-election periods are constructed from pre-election contributions of those donors matched to an establishment in the respective post-election period. Aggregates for the pre-election periods are constructed as before from the actual contributions in those periods. Columns (3) and (4) show results for individual-level regressions in a sample of donors who have a matched contribution to the same union election establishment at least once before and once after the union election. We aggregate all matched contributions into one pre- and one post-period observation and estimate a two-period DiD version of model (1) with individual and cohort  $\times$  post-election fixed effects. All samples only include all establishments / individuals from establishments with a pro-union vote share between 20 and 80%. Standard errors clustered at the establishment level are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 4: Differentiating Candidates by Within-party Ideology

	Democrats			Republicans		
	All (1)	Moderate (2)	Liberal (3)	All (4)	Moderate (5)	Conservative (6)
<b>[A]: All employees</b>						
$\delta_{\text{DiD}}$	0.0920 (0.0633)	-0.0188 (0.0544)	0.121*** (0.0462)	-0.149** (0.0654)	-0.0693 (0.0547)	-0.154*** (0.0494)
<b>[B]: Workers</b>						
$\delta_{\text{DiD}}$	0.0733** (0.0351)	0.0309 (0.0237)	0.0554* (0.0298)	-0.0506 (0.0317)	-0.0154 (0.0224)	-0.0313 (0.0257)
<b>[C]: Managers</b>						
$\delta_{\text{DiD}}$	0.0733 (0.0467)	0.0123 (0.0391)	0.0900*** (0.0347)	-0.131*** (0.0490)	-0.0569 (0.0396)	-0.123*** (0.0369)

**Notes:** The table reports DiD coefficients estimated in model (1) for the effect of unionization on IHS-transformed amounts contributed to different candidate groups. Moderate (liberal) Democrats refer to Democratic candidates with a CF score above (below) the median CF score of all Democratic candidates observed in our sample of matched contributions. Moderate and conservative Republicans are differentiated accordingly using the median Republican CF score. The sample includes all establishments with a pro-union vote share between 20 and 80%.  $N = 33,117$  establishment - cycle observations. Results are reported for contributions from all employees (Panel A), only from non-managerial workers (Panel B), and only from managers and supervisors (Panel C). Standard errors clustered at the establishment level are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 5: Contributions to Political Action Committees

	Party/candidate PACs		Interest-group PACs					
	All	Dem – Rep	All	Corporation	Trade assoc.	Member orga.	Labor orga.	Dem – Rep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>[A]: All employees</b>								
$\delta_{DiD}$	-0.0250 (0.0522)	0.0967** (0.0478)	-0.0816 (0.0635)	-0.0925** (0.0409)	-0.0259 (0.0440)	-0.00874 (0.0311)	0.0169 (0.0109)	0.0601 (0.0407)
<b>[B]: Workers</b>								
$\delta_{DiD}$	0.0628** (0.0320)	0.00991 (0.0275)	0.0880** (0.0346)	-0.0198 (0.0205)	0.0211 (0.0158)	0.0462** (0.0190)	0.0188*** (0.00709)	0.0242 (0.0266)
<b>[C]: Managers</b>								
$\delta_{DiD}$	-0.000275 (0.0344)	0.102*** (0.0315)	-0.0924* (0.0488)	-0.0817** (0.0340)	-0.0257 (0.0330)	0.000776 (0.0179)	0.00369 (0.00684)	0.0811** (0.0324)

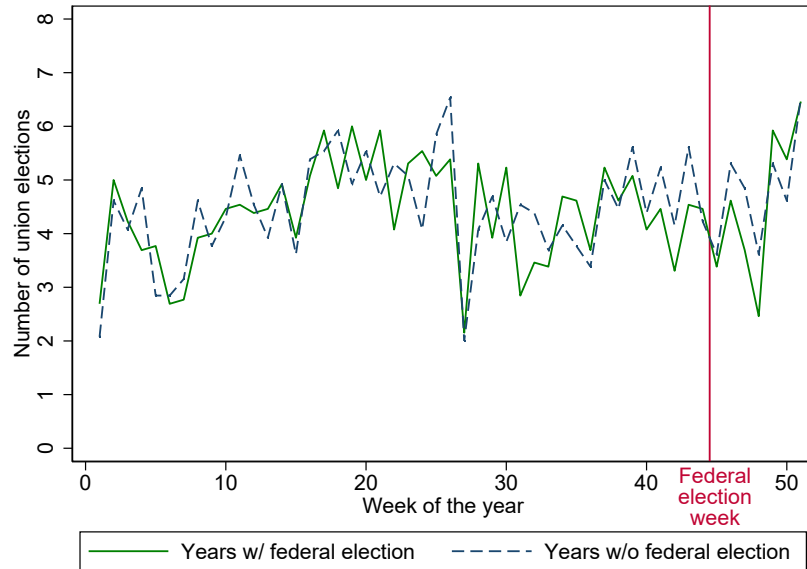
**Notes:** The table presents DiD coefficients estimated in model (1) for the effect of unionization on IHS-transformed amounts contributed to different committee groups. In columns (2) and (7) the dependent variable is the difference between the IHS-transformed amount contributed to Democratic and Republican committees. Interest-group PACs are categorized as “Democratic” (“Republican”) if their own campaign contributions go to more (less) than 50% to Democratic candidates. The sample includes all establishments with a pro-union vote share between 20 and 80% and covers three election cycles before and after the union election.  $N = 33,117$  establishment - cycle observations. Results are reported for contributions from all employees (Panel A), only from non-managerial workers (Panel B), and only from managers and supervisors (Panel C). Standard errors clustered at the establishment level are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

# Online Appendix

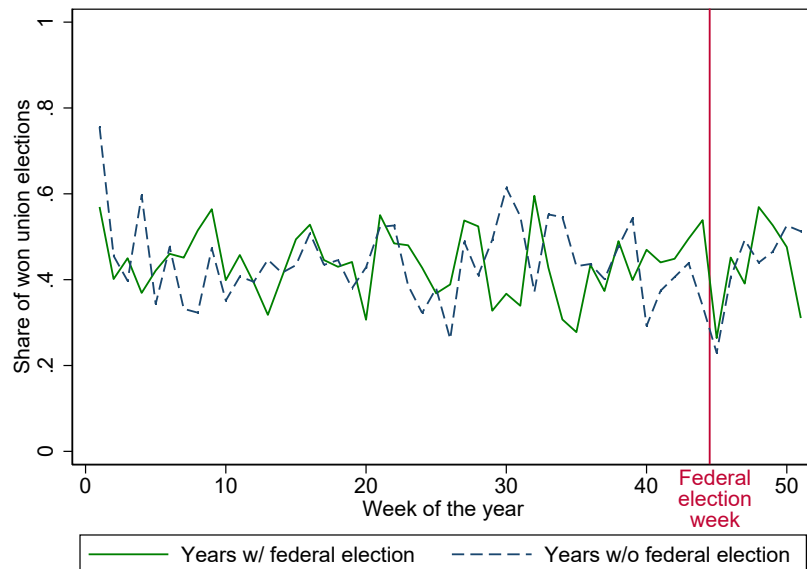
## A Additional Figures and Tables

Figure A.1: Cyclicalty of Union Elections

(a) Number of Union Elections per Week of the Year

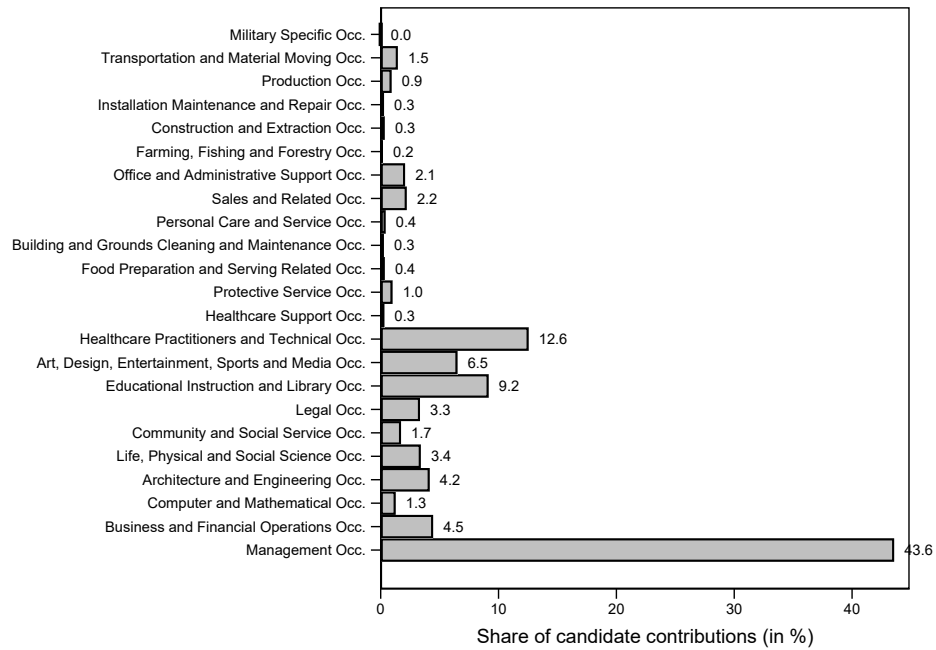


(b) Share of Won Union Elections per Week of the Year



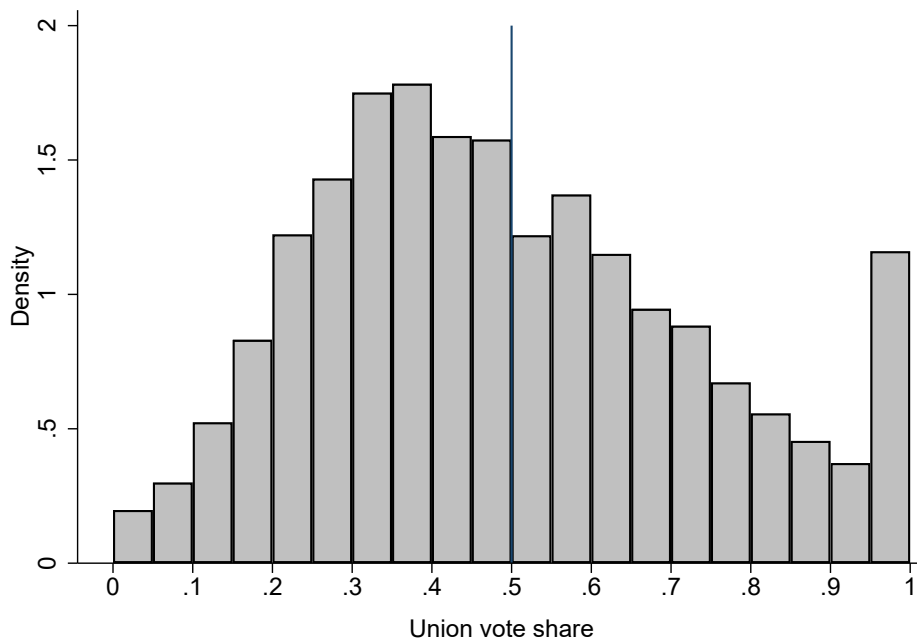
**Notes:** The graphs show the mean number of elections (Panel (a)) and mean share of won union elections (Panel (b)) per week of the year across all years in our period of analysis, i.e. between 1985 and 2010. The means are based on our matched estimation sample. We distinguish between years with and without federal elections. The red line highlights the week of federal elections which is calendar week 44 or 45).

Figure A.2: Donor Occupations



**Notes:** The figure shows the distribution of occupations for all candidate contributions that are included in our matched estimation sample and have a classified occupation. For 28.1% of the contributions we were not able to assign an occupation code. Occupation groups are 2-digit codes of the 2018 Standard Occupational Classification (SOC). See Appendix C.3 for details on the occupation classification procedure.

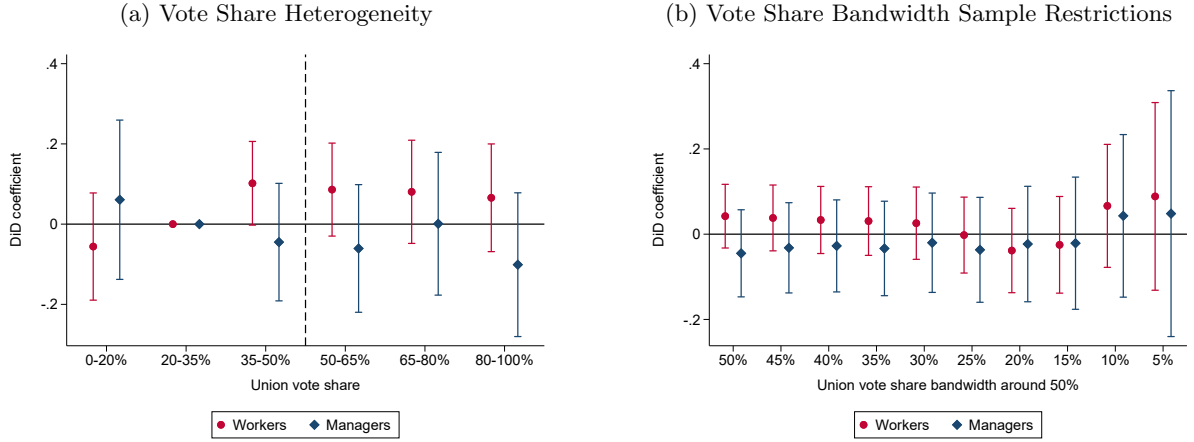
Figure A.3: Vote Share Distribution



**Notes:** The figure plots the density of union vote shares for all 6,065 union elections included in our matched estimation sample. The Frandsen (2017) test strongly rejects continuity in the union vote share density at the 50% cutoff (p-value = .002 for  $k = 0$  and p-value = .003 for  $k = .02$ ).

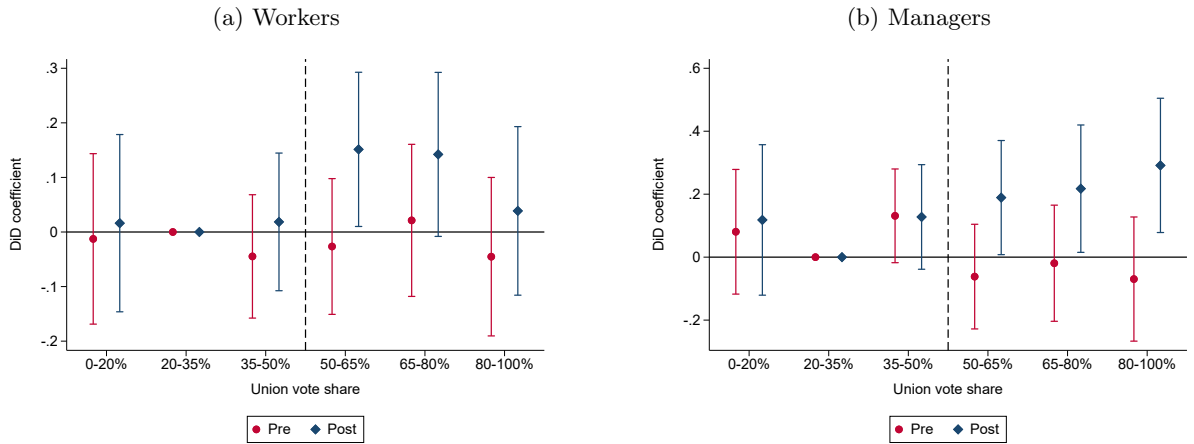


Figure A.4: Effects of Unionization on Total Contribution Amounts - Regression Discontinuity Tests



**Notes:** The graphs show RD-type placebo and robustness tests for the effect of unionization on the IHS-transformed total amount contributed. Panel (a) reports the  $\delta_g$  coefficients from model (3). The vote share distribution is partitioned into six bins, indicated on the x-axis. The omitted reference group is 20-35%. Panel (b) reports DiD coefficients estimated in model (1). Each dot refers to a single DiD coefficient that is estimated among elections with a union vote share in a given bandwidth around the 50% cutoff. Estimates from smaller bandwidths compare changes between increasingly close elections. Results are always shown separately for contributions by non-managerial workers ("workers") as well as managers and supervisors ("managers"). 95% confidence intervals are depicted for standard errors clustered at the establishment level.

Figure A.5: Effects of Unionization on Democratic versus Republican Support - Vote Share Heterogeneity in Pre- versus Post-Effects



**Notes:** The graphs reports coefficients for interactions between union win, six vote share categories, and two dummies for pre- versus post-union election periods. The regressions modify model (3) by including an additional interaction with a pre-period dummy (three and two cycles before the union election). The reference event time is the cycle before the union election and the reference vote share category is 20-35%. Results are shown separately for contributions by non-managerial workers ("workers") as well as managers and supervisors ("managers"). 95% confidence intervals are depicted for standard errors clustered at the establishment level.

Table A.1: Characteristics of Matched and Non-matched Union Elections

	Matched	Not matched
Number of elections	6,065	22,758
Union win (dummy)	.4396	.4404
Union vote share	.4950	.4955
Number of votes	119.30	81.93
Number of eligible voters	139.18	94.01
Industry: mining	.0397	.0388
Industry: manufacturing	.3336	.3731
Industry: transport	.1787	.1731
Industry: trade	.1397	.1251
Industry: finance	.1007	.0584
Industry: services	.1835	.2193
Years 1985-89	.1617	.2796
Years 1990-94	.1908	.2529
Years 1995-99	.2318	.2261
Years 2000-04	.2547	.1617
Years 2005-10	.1609	.0798

**Notes:** The table reports mean characteristics of matched and non-matched union elections. Matched elections form our estimation sample and are defined as those for whom we were able to match at least one employee contribution in any of seven election cycles around the union election (three before, cycle of union election, three after).

Table A.2: Robustness of Main Results

	\$ to all candidates			\$ to Dem. – \$ to Rep.		
	All (1)	Workers (2)	Managers (3)	All (4)	Workers (5)	Managers (6)
[A]: Baseline						
$\delta_{\text{DiD}}$	0.0318 (0.0794)	0.0258 (0.0433)	-0.0201 (0.0595)	0.241*** (0.0792)	0.124*** (0.0396)	0.205*** (0.0560)
[B]: Borusyak, Jaravel, and Spiess (2021)						
$\delta_{\text{DiD}}$	0.0882 (0.0747)	0.0417 (0.0422)	0.00760 (0.0576)	0.237*** (0.0741)	0.131*** (0.0390)	0.183*** (0.0545)
[C]: Callaway and Sant’Anna (2021)						
$\delta_{\text{DiD}}$	0.0138 (0.0827)	0.0414 (0.0444)	-0.0387 (0.0606)	0.245*** (0.0870)	0.137*** (0.0453)	0.135** (0.0619)
[D]: Log(Amount+1)						
$\delta_{\text{DiD}}$	0.0273 (0.0725)	0.0219 (0.0386)	-0.0176 (0.0541)	0.215*** (0.0719)	0.107*** (0.0351)	0.180*** (0.0506)
[E]: Untransformed amounts						
$\delta_{\text{DiD}}$	-27.54 (60.16)	2.461 (10.34)	-22.95 (33.01)	116.9*** (36.87)	15.62** (6.222)	65.51*** (20.12)
[F]: Only federal candidates						
$\delta_{\text{DiD}}$	0.0469 (0.0751)	0.0255 (0.0390)	-0.0179 (0.0534)	0.208*** (0.0764)	0.0990*** (0.0364)	0.182*** (0.0519)
[G]: Only local candidates						
$\delta_{\text{DiD}}$	-0.0476 (0.0499)	0.0242 (0.0285)	-0.0343 (0.0427)	0.159*** (0.0440)	0.0454* (0.0245)	0.131*** (0.0383)

**Notes:** The table presents robustness checks for our DiD estimates of the effect of unionization on the total amount contributed (columns (1) - (3)) and on the difference between the amount contributed to Democratic and Republican candidates (columns (4) - (6)).  $N = 33,117$  establishment-cycle observations. Panel A shows the baseline results from the stacked DiD model (1) with IHS-transformed amounts. Panels B presents results from the imputation approach introduced by [Borusyak \*et al.\* \(2021\)](#). Panel C implements the DiD estimator of [Callaway and Sant’Anna \(2021\)](#) where we use both never-treated establishments (i.e., lost elections) and not-yet-treated establishments (i.e., won elections in later cycles) as comparison units. In Panel D, outcomes are transformed as  $\log(\text{amount} + 1)$ , while in Panel E we use untransformed amounts. In Panels F and G only contributions to candidates in federal (congressional and presidential) or state elections are considered, respectively. Standard errors clustered at the establishment level are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.3: Robustness to Alternative Worker-Manager Classifications

	IHS(\$ to all candidates)		IHS(\$ to Dem.) – IHS(\$ to Rep.)	
	Workers	Managers	Workers	Managers
	(1)	(2)	(3)	(4)
[A]: Baseline (80 <sup>th</sup> percentile of supervisor tasks)				
$\delta_{\text{DiD}}$	0.0258	-0.0201	0.124***	0.205***
	(0.0433)	(0.0595)	(0.0396)	(0.0560)
[B]: 90 <sup>th</sup> percentile of supervisor tasks				
$\delta_{\text{DiD}}$	0.0430	-0.0418	0.141***	0.202***
	(0.0458)	(0.0585)	(0.0421)	(0.0546)
[C]: Supervisor tasks “very important” (4 out of 5 in ranking)				
$\delta_{\text{DiD}}$	0.0270	-0.0227	0.132***	0.204***
	(0.0432)	(0.0597)	(0.0394)	(0.0561)
[D]: Non-managerial supervisors as workers				
$\delta_{\text{DiD}}$	0.0401	-0.0516	0.163***	0.184***
	(0.0481)	(0.0570)	(0.0448)	(0.0529)

**Notes:** The table presents robustness checks for alternative worker-manager classifications. Reported are the DiD coefficients estimated in model (1) for the effect of unionization on the IHS-transformed total amount contributed (columns (1) and (2)) and on the difference between the amount contributed to Democratic and Republican candidates (columns (3) and (4)).  $N = 33,117$  establishment - cycle observations. Panel A shows the baseline results in which “managers” are defined as donors in “Management occupations” (SOC group 11) or in occupations above the 80% percentile of supervisor tasks and independent judgment. “Workers” are all remaining donors with a classified occupation. In Panel B, we increase the cut-off for supervisor tasks and independent judgment to the 90% percentile. Panel C, instead, uses an absolute cutoff for the importance of supervisor tasks and independent judgement (both need to be “very important”, i.e., have a score of 4 or above in the 5-score-ranking). In Panel D, we only consider “Management occupations” (SOC group 11) as “managers” and treat all other classified occupations as “workers” (including those with high importance in supervisor tasks and independent judgment). See Appendix C.3 for more details on the classifications. Standard errors clustered at the establishment level are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.4: Effects of Losing a Union Election

	IHS(\$ to all candidates)			IHS(\$ to Dem.) – IHS(\$ to Rep).		
	All	Workers	Managers	All	Workers	Managers
	(1)	(2)	(3)	(4)	(5)	(6)
[A]: Stacking						
$\delta_{\text{DiD}}$	-0.0483 (0.0880)	-0.0262 (0.0395)	0.0713 (0.0529)	0.0561 (0.0966)	-0.0132 (0.0428)	0.0359 (0.0574)
N	31521	31521	31521	31521	31521	31521
[B]: Borusyak, Jaravel, and Spiess (2021)						
$\delta_{\text{DiD}}$	-0.0468 (0.0900)	-0.0284 (0.0446)	0.0759 (0.0590)	0.0783 (0.100)	-0.00698 (0.0489)	0.0471 (0.0641)
N	16668	16668	16668	16668	16668	16668
[C]: Callaway and Sant’Anna (2021)						
$\delta_{\text{DiD}}$	-0.0421 (0.0947)	-0.0380 (0.0468)	0.0629 (0.0637)	0.0747 (0.105)	-0.00701 (0.0515)	0.0519 (0.0698)
N	16668	16668	16668	16668	16668	16668

**Notes:** The table presents DiD estimates for the effect of losing a union election versus holding no election. We compare establishments with a lost union election in a given cycle (treated cohort) with establishments with a lost union election in one of the next two cycles (control cohorts) in a DiD design. Thereby, we estimate short-run effects of losing an election (for event times  $k = \{0, 1\}$ ). Panel A shows results from a stacked DiD model, and Panels B and C implement the staggered DiD estimators of [Borusyak et al. \(2021\)](#) and [Callaway and Sant’Anna \(2021\)](#). See Appendix B.1 for details of the implementation. Standard errors clustered at the establishment level are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.5: Composition versus Individual-level Effects - PAC Contributions

	Party/candidate PACs		Interest-group PACs					
	All	Dem – Rep	All	Corporation	Trade assoc.	Member orga.	Labor orga.	Dem – Rep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>[A]: Composition effects</b>								
[A.1]: All employees								
$\delta_{DiD}$	-0.0237 (0.0482)	0.0766* (0.0461)	-0.0432 (0.0501)	-0.0152 (0.0312)	-0.0174 (0.0355)	-0.00968 (0.0273)	-0.00573 (0.00844)	0.0114 (0.0295)
N	33117	33117	33117	33117	33117	33117	33117	33117
[A.2]: Workers								
$\delta_{DiD}$	0.0135 (0.0276)	0.0373 (0.0255)	0.0277 (0.0267)	0.00247 (0.0150)	0.00632 (0.0173)	0.0213 (0.0152)	0.00268 (0.00447)	0.0105 (0.0159)
N	33117	33117	33117	33117	33117	33117	33117	33117
[A.3]: Managers								
$\delta_{DiD}$	0.0199 (0.0352)	0.0385 (0.0337)	-0.0178 (0.0374)	0.00808 (0.0246)	-0.0156 (0.0265)	-0.0106 (0.0189)	-0.000309 (0.00607)	0.0102 (0.0222)
N	33117	33117	33117	33117	33117	33117	33117	33117
<b>[B]: Individual-level effects for stayers</b>								
[B.1]: All employees								
$\delta_{DiD}$	0.361*** (0.130)	0.174 (0.131)	0.112 (0.146)	-0.116 (0.0939)	0.0792 (0.0884)	-0.0583 (0.0702)	0.00175 (0.0106)	0.304** (0.153)
N	5740	5740	5740	5740	5740	5740	5740	5740
[B.2]: Workers								
$\delta_{DiD}$	0.580** (0.230)	0.473** (0.230)	0.499** (0.246)	0.0865 (0.138)	0.187 (0.127)	0.0646 (0.117)	0.0272 (0.0234)	-0.206 (0.288)
N	2052	2052	2052	2052	2052	2052	2052	2052
[B.3]: Managers								
$\delta_{DiD}$	0.202 (0.180)	0.0866 (0.189)	-0.0706 (0.197)	-0.279* (0.147)	0.0765 (0.130)	-0.132 (0.0845)	-0.0148 (0.0148)	0.682*** (0.217)
N	2890	2890	2890	2890	2890	2890	2890	2890

**Notes:** The table presents DiD coefficients estimated in model (1) for the effect of unionization on IHS-transformed amounts contributed to different committee groups. In Panels A.1 to A.3, the establishment aggregates for the post-election periods are constructed from pre-election contributions of those donors matched to an establishment in the respective post-election period. Aggregates for the pre-election periods are constructed as before from the actual contributions in those periods. Panels B.1 to B.3 show results for individual-level regressions in a sample of donors who have a matched contribution to the same union election establishment at least once before and once after the union election. All matched contributions are aggregated into one pre- and one post-period observation. All samples only include all establishments / individuals from establishments with a pro-union vote share between 20 and 80%. Standard errors clustered at the establishment level are in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## B Additional Analyses

### B.1 Effects of Losing a Union Election

We estimate the effects of losing a union election compared to holding no election by using establishments who hold and lose an election in the future as a control group. Consider the treatment cohort of elections that were held and lost in the cycle 1985/86. Given that we observe each establishment only up to three cycles before the union election, we can use elections held and lost in the next two cycles as control cohorts. The untreated pre-election observations of the 1987/88 control cohort refer to the cycles 1981/82, 1983/84, and 1985/86 (event times  $k = \{-2, -1, 0\}$  of the treated cohort), and those of the 1989/90 control cohort refer to the cycles 1983/84, 1985/86, and 1987/1988 (event times  $k = \{-1, 0, 1\}$  of the treated cohort). Note that later cohorts are not observed before the treated cohort hold their election and can therefore not be used in a DiD comparison. Consequently, we only have untreated observations that we can compare to the treated cohort's observations in cycles 1981/82, 1983/84, 1985/86, and 1987/88 (event times  $k = \{-2, -1, 0, 1\}$ ). This means we can only identify short-run effects.

Given these considerations, we implement a stacked DiD model as follows. For each cohort of lost elections in cycle  $g$ , we create a cohort-specific dataset that is built from cycles in event times  $k = \{-2, -1, 0, 1\}$  of the treated cohort  $g_i = g$  and from the three pre-election cycles of lost elections in the control cohorts  $g_i = \{g+1, g+2\}$ . Then, the stacked DiD model is estimated as:

$$y_{ik} = \alpha_{ig} + \beta_{kg} + \delta_{\text{DiD}} \times \left( \mathbb{1}[k \geq 0] \times \mathbb{1}[g_i = g] \right) + \epsilon_{ik} \quad (5)$$

where  $k$  now denotes the number of cycles relative to the cycle when the treated cohort held its union election. Establishment fixed effects are now saturated with indicators for the cohort-specific dataset  $g$  to account for the fact that establishments enter several datasets. The DiD coefficient  $\delta_{\text{DiD}}$  is given by the interaction between a dummy for post-election cycles of the treated cohort ( $k \geq 0$ ) and a dummy for the treated cohort ( $g_i = g$ ). Results are reported in Panel A of Table A.4.

In Panels B and C of Table A.4, we also show results for the alternative staggered DiD estimators by [Borusyak \*et al.\* \(2021\)](#) and [Callaway and Sant'Anna \(2021\)](#). In line with our stacking implementation, in settings with no never-treated units, both estimators use not-yet-treated observations as controls. The methods differ from the stacked DiD model in the number of pre-treatment periods used and the aggregation of unit- or cohort-specific effects. In our results, however, the estimates are very similar to those of the stacked DiD model.

## C Data Appendix

### C.1 Union Election Data

**Data sources.** We start by accessing data on NLRB union representation elections between 1961 and 2009 from the replication package of [Knepper \(2020\)](#). The data was originally compiled by Henry Farber. Then, we add data on elections between 2010 and 2018 from NLRB election reports available on <https://www.nlr.gov/reports/agency-performance/election-reports>. Together, our data covers the universe of union elections between 1961 and 2018 and includes information on vote counts, voting outcome, petition filing and election date, establishment name, address, and industry, as well as the name of the union organization.

**Sample restrictions.** Before matching campaign contributions, we impose the following restrictions to the sample of union elections:

- We only consider elections where a union seeks to be certified and drop elections that stem from petitions of either employers or employees seeking to remove an existing union.
- We delete duplicate entries (multiple records of the same election).
- For multiple entries that reflect elections where more than one union is on the ballot or where different worker groups formed different bargaining units, we follow Frandsen (2021) and retain only the entry with the largest union vote share.
- We further drop a few elections where the voting outcome (won or lost) is not consistent with the vote counts.
- Following the RD literature on union elections, we restrict the sample to union elections where at least 20 votes were cast.
- We only keep the first union election in each establishment. For this, we identify an establishment as a unique address or a unique combination of the standardized firm name and commuting zone. For a firm that has multiple establishments within the same commuting zone, we thus only consider the first election among these establishments.
- Finally, we only use elections held between 1985 and 2010 to be able to observe employee contributions for three election cycles before and after each union election.

After these restrictions, we are left with 28,823 union elections.

### C.2 Details on the Matching of Elections and Campaign Contributions

We link the campaign contributions of employees to union elections in their employing establishment by combining a spatial match with a fuzzy match of firm names.



**Geocode commuting zones.** In preparation for the spatial match, we first geocode all union election establishments based on their city and state (using the Open Street Map and Google Maps APIs) and assign the 1990 commuting zone. For the employees' campaign contributions, we rely on donor addresses geocoded by [Bonica \(2019\)](#) up to 2016.<sup>32</sup> We use these geocodes to match the 1990 commuting zone to it.

**Firm name cleaning.** Firm names in both the union election and the contribution data are cleaned and harmonized using the `stnd_compname` Stata command developed by [Wasi and Flaaen \(2015\)](#). The algorithm removes non-standard characters and whitespaces, doing-as-business and FKA names, as well as business entity types (e.g., CORP, INC, LLC). Moreover, it abbreviates common strings in firm names (e.g., Manufacturing → MFG, Professional → PROF).

**Linkage algorithm.** For each commuting zone, we create lists of all cleaned firm names from the union election and the contribution data. Then, we use the `reclink2` Stata command from [Wasi and Flaaen \(2015\)](#) to compare the string similarity of firm names.<sup>33</sup> For each possible pair of firm names within the commuting zone, the command computes modified bigram scores. We keep potential matches with a score of at least .98 and manually review all of them. We identify roughly 70% of them as correct matches.<sup>34</sup> In our review, we generally took a conservative approach and were more tolerant to possibly reject a true match than to retain a wrong match. This means that we measure a lower bound for the sum of contributions from all employees of an establishment. To demonstrate the spatial dimension of the matching procedure, Figure C.1 shows an example for the location of a union election establishment and all campaign contributions matched to it.

**Establishment-level aggregation.** As a last step, we use all contributions with a matched establishment name and sum them up at the establishment - election cycle - level. Our period of analysis covers three cycles before to three cycles after each union election, i.e. we observe each establishment over a period of seven cycles (14 years). While we generally keep establishment-cycle-observations without any matched contribution and code them as zero, we retain only establishments for which we observe at least one matched contribution over the 14-year-period. Out of the initial 28,823 union election establishments, we thereby keep 6,065 matched establishments which form our final estimation sample. Table A.1 compares the characteristics of

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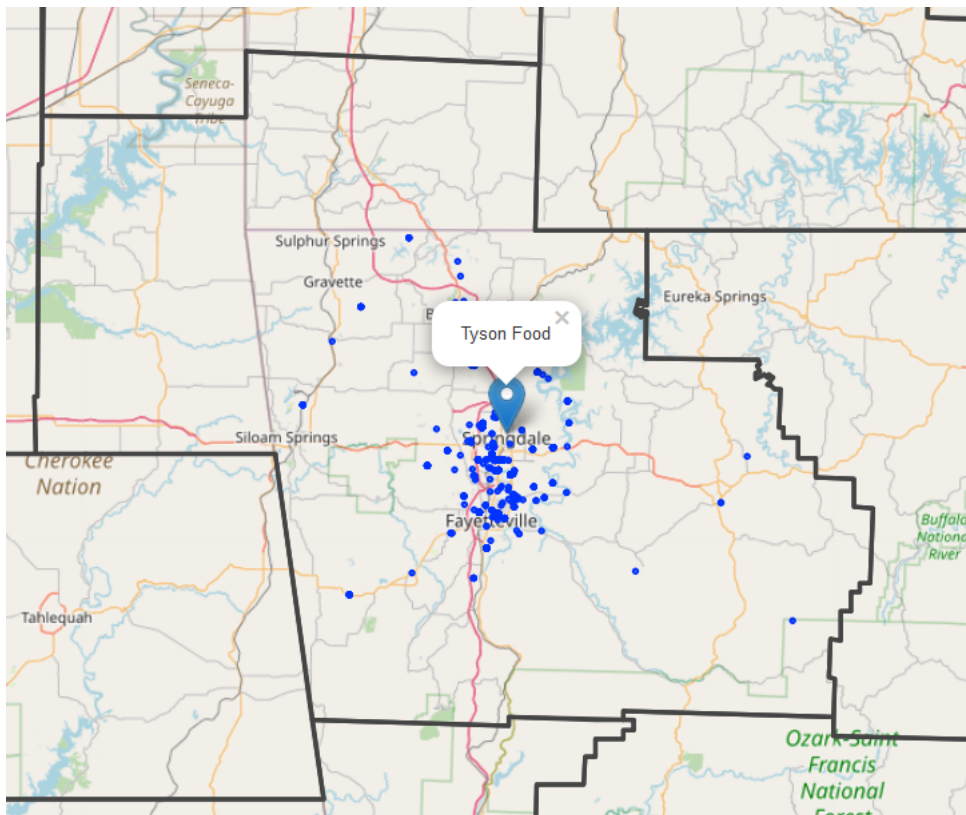
<sup>32</sup>[Bonica \(2019\)](#) contains campaign contributions until 2018 but geocodes are only provided until 2016.

<sup>33</sup>`reclink2` builds on `reclink` written by [Blasnik \(2010\)](#).

<sup>34</sup>The share of matches identified as correct is strongly increasing in the bigram score. For scores between .995 and 1, we keep 90% of the potential matches, while for scores between .98 and .985 this share is only 34%. We also tried keeping potential matches with a lower score (.95) but a manual review of a subsample of those revealed that a very low share of them represented correct matches.

matched and non-matched establishments.

Figure C.1: Example of Spatial Matching Procedure



**Notes:** The map shows the location of the establishment “Tyson Foods” in Springdale (Arkansas) which hold a union election on 22/06/2006. Blue dots represent the location of all matched campaign contributions to the establishment. Black lines are 1990 commuting zone borders.

### C.3 Occupation Classification

**NRLA definitions.** We rely on the definition of the National Labor Relations Act (NLRA) to differentiate between employees eligible for unionization and employees banned from unionizing. The NLRA passed by Congress in 1935 sets rules for the unionization of private sector employees. It establishes who can and who cannot join a union. Section 7 describes the right for employees to join a union:

“Employees shall have the right to self-organization, to form, join, or assist labor organizations, to bargain collectively through representatives of their own choosing [...] and shall also have the right to refrain from any or all of such activities [...]” (29 U.S.C. § 157)

The NRLA restricts the right to unionize explicitly to employees. It does not extend it to

individuals with management and supervisory responsibilities as they are part of the company’s management: The term employee “shall include any employee [...] but shall not include any individual [...] employed as a supervisor.” (29 U.S.C. § 152(3)) The distinction between supervisors and employees is, however, not clear-cut and the NLRA goes on to define supervisors as follows:

“The term ‘supervisor’ means any individual having authority, in the interest of the employer, to hire, transfer, suspend, lay off, recall, promote, discharge, assign, reward, or discipline other employees, or responsibly to direct them, or to adjust their grievances, or effectively to recommend such action, if in connection with the foregoing the exercise of such authority is not of a merely routine or clerical nature, but requires the use of independent judgment.” (29 U.S.C. § 152(11))

To differentiate between the labor force eligible for unionization and the company’s management, we follow two steps: i) we harmonize occupations and ii) we calculate the supervisory element of each occupation based on the NLRA definition.

**Occupation harmonization.** The free-text occupations reported in DIME are not standardized. Thus, we map them to the 6-digit Standard Occupation Classification. For this, we combine an ensemble classifier called SOCcer (Russ *et al.*, 2016), fuzzy string matching to an extensive crosswalk of laymen’s occupation titles from O\*NET, as well as manual reviews from Dreher *et al.* (2020) and manual reviews of the most common occupation titles. In particular, we implement the following steps to identify good matches between a free-text occupation and a SOC code. First, we keep a match determined by SOCcer if the score of the first best match is higher than 0.3 and the difference to the second best match is larger than 0.1. Secondly, we search for exact matches of any substring of the free-text occupations and a list of laymen’s occupation titles, abbreviations and reported titles by experts obtained from O\*NET. Thirdly, we fuzzy match the lists from O\*NET with the free-text occupations and keep matches with a score above 0.99. Fourthly, we add matches from Dreher *et al.* (2020) which are based on a manual review. Finally, we manually review the free-text occupations that appear more than 50 times in our database of candidate contributions. With that procedure, we are able to assign a SOC code to 72% of all candidate contributions in our matched sample.

Since the share of non-classified occupations is not negligible, we seek to understand whether non-classification can impact our results on the effects of unionization. For this, we use the contribution-level dataset and estimate our baseline model (1) with an indicator for missing occupation classification as the dependent variable. The model yields an insignificant DiD coefficient of .0058 (p-value = 0.76). Thus, the likelihood of occupation non-classification does not

appear to be related to unionization.

**Manager/supervisor versus worker classification.** We follow the NLRA and classify an individual as supervisor if *independent* judgement and a *supervisor-task* are important for her occupation. In order to identify occupations with these characteristics, we merge the Occupational Information Network database (O\*NET, version 26.3) containing task- and skill-content of 6-digit SOC occupations to our DIME occupations. The information in O\*NET supported by the U.S. Department of Labor and is based on surveys of workers working in the respective occupation. Only the importance of specific skills and abilities for an occupation is determined by occupational analysts. We select six variables that closely resemble at least one work activity of a supervisor as define in the NLRA to identify occupations with *supervisor-tasks*. The variables are listed in Table C.1 and measure the importance of the activity in each occupation. We classify an occupation as containing *supervisor-tasks* if the importance of at least one listed task is equal or above the 80<sup>th</sup> percentile of all 6-digit SOC occupations.<sup>35</sup> We then go on to evaluate whether the occupation requires independent judgement, the second condition that we identify in the NLRA definition of a supervisor. We evaluate whether an occupation requires *independent* judgement based on the following four variables: Independence (Work Styles), Leadership (Work Styles), Structured versus Unstructured Work (Work Context), Freedom to Make Decisions (Work Context). Again, we classify an occupation requiring *independent* judgement if the importance of at least one of the listed variables is equal or above the 80<sup>th</sup> percentile.<sup>36</sup> Finally, we classify individuals as managers or supervisors if they are classified as “Management Occupations” in SOC (SOC group 11) or contain a *supervisor-task* and *independent judgement* as defined above.<sup>37</sup> Examples for occupations in the top 95th percentile of both the *independent* judgement and *supervisor-task* score are *Chief Executives*, *Human Resource Managers* and *First-Line Supervisors of Retail Sales Workers*. Rank-and-file workers are then identified as all remaining donors to whom we were able to assign a SOC code. With these definitions, we obtain the following occupational composition in our sample of candidate contributions: 42% of contributions originate from managers and supervisors, 30% from rank-and-file workers, and for

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<sup>35</sup>In our robustness checks we also use the 90<sup>th</sup> percentile as cutoff and an absolute scale classifying any occupation as supervisor where a supervisor-task is at least “very important” (a score of 4 or above in the 5-score-ranking).

<sup>36</sup>Again, in our robustness checks we also use the 90<sup>th</sup> percentile as cutoff and an absolute scale classifying any occupation as supervisor where independence is at least “very important” (a score of 4 or above in the 5-score-ranking).

<sup>37</sup>We were not able to assign a 6-digit SOC code for some of the individuals in our data in cases where the free-text occupation was vague. Instead, we assigned 4-, 3- or 2-digit SOC codes. We classify a 2-digit SOC code occupation as supervisor if all 6-digit SOC code occupations have been classified as supervisor. We proceed accordingly for 3- and 4-digit SOC code occupations. We are thereby conservative and allow for some attenuation bias if supervisors are consequently wrongly coded as workers.

28% we are unable to obtain a classification.

Table C.1: Supervisor Tasks in NLRA and O\*NET Occupations

Tasks of a <i>supervisor</i> defined in NLRA	Corresponding O*NET work activity / skill / context
Hire / transfer / suspend / lay off / discharge	Staffing organizational units
Recall / assign	Management of personnel resources Coordinating the work and activities of others
Promote / reward / discipline	Guiding, directing, and motivating subordinates Resolving conflicts and negotiating with others
Direct employees / adjust their grievances	Management of personnel resources Guiding, directing, and motivating subordinates Coordinating the Work and Activities of Others Coordinate or Lead Others