

Do Unions Shape Political Ideologies at Work? *

Johannes Matzat [†] Aiko Schmeißer [‡]

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Abstract

Labor unions' greatest potential for political influence likely arises from their direct connection to millions of individuals at the workplace. There, they may change the political preferences of both unionizing workers and their non-unionizing management. In this paper, we analyze the impact of unionization on workers' and managers' campaign contributions over the 1980-2016 period in the United States. To do so, we link establishment-level union election data with transaction-level campaign contributions to federal and local candidates. Combining a difference-in-differences design with regression discontinuity tests and a novel instrumental variables approach, we find that unionization increases the support for Democrats relative to Republicans not only among workers but also among managers, which speaks against an increase in political cleavages between the two groups. We provide evidence that our results are not driven by compositional changes of the workforce and are weaker in states with Right-to-Work laws where unions can invest fewer resources in political activities.

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[†]Georg-August University Goettingen; Heidelberg University; e-mail: johannes.matzat@uni-goettingen.de.

[‡]University of Potsdam; Berlin School of Economics; e-mail: schmeisser@uni-potsdam.de.

1 Introduction

Can interest groups shape political preferences? Labor unions are powerful interest groups for the working-class. Political leaders credit unions for shaping welfare systems and labor market policies, such as the 8-hour day, minimum wage, safety standards, sick leave, weekends, family leave, overtime compensation, and retirement plans (e.g., [Biden, 2021](#); [King, 1965](#); [Obama, 2010](#)). Unions' influence is often attributed to their effect on work contracts via collective bargaining: "Through collective bargaining and grievance procedures, they have brought justice [...] to the shop floor" ([Kennedy, 1960](#)). The literature on the labor market effects of unions has found mixed evidence for the United States, ranging from negligible to considerable positive influence on wages and other benefits ([Card, 1996](#); [DiNardo and Lee, 2004](#); [Farber *et al.*, 2021](#); [Frandsen, 2021](#); [Freeman and Kleiner, 1990b](#); [Knepper, 2020](#)). Lasting change in welfare states and policies, however, is brought about by changing preferences and beliefs of workers and the broader public. Are unions able to shape political preferences?

Unions draw on significant political resources. In 2010, they employed over 3,000 full-time workers and spent 700 million USD on political activities, a figure that rose to 1.8 billion USD in 2020 ([WSJ, 2012](#); [NILRR, 2021](#)). Unions' greatest political leverage likely arises from their connection to more than 14 million union members and their colleagues at the unionized workplace. After family and friends, the workplace is the most important arena for political discussion ([Hertel-Fernandez, 2020](#)). Interactions among employees and social experiences at work make it a particularly influential space for unions. By providing political information and training as well as facilitating communication networks between members, they can affect the political participation and policy preferences of workers. Still, unions' aggregate political influence on the workplace is far from clear. While they might be able to assemble unionized workers around their political positions, it is unclear whether they can persuade the firm's management. Heightened tension between workers and managers, who represent the owners' interests, might yield adverse responses to labor issues. 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 influence of labor unions on political participation and preferences at the workplace in the United States. We combine establishment-level data on 6,063 union elections with transaction-level data on 357,436 campaign contributions to federal and local candidates over the 1980-2016 period. Linking a political outcome to unionization at the establishment level offers new opportunities for studying the political influence of unions. First, establishment-level union elections provide us with plausible identification strategies to causally

estimate the impact of unions on the political behavior of their members. Second, we study the political influence of unions at the workplace, where unionization takes place and where indirect effects on managers are likely to occur. Differentiating employees into workers and managers allows us to uncover within-firm dynamics that have been previously ignored. Our results show a sizeable shift in campaign contributions away from Republicans toward Democrats. Importantly, these effects are found not only for workers but also for managers.

This result is consistent with the strong relationship between labor unions and the Democratic party (Dark, 2001). Given that they share stances on many labor issues, it is no surprise that unions have overwhelmingly promoted Democratic candidates. They support Democrats financially by marshalling campaign contributions, directly endorse candidates and policies through voting recommendations, lobby legislators to introduce pro-labor policies, and mount get-out-the-vote campaigns to increase participation among the working class (Burns *et al.*, 2000; Rosenfeld, 2014; Schlozman, 2015). How can workplace unionization alter members' political behavior? Kerrissey and Schofer (2013) have argued that unions provide their members with political capital - they inform, engage, and mobilize members. Unions spend substantial resources on outreach and political education of their members. Most unions have newspapers and/or websites that seek to inform members about topics relevant for their working conditions. Moreover, they frequently hold meetings and workshops in which union members learn and exchange political views (Ahlquist and Levi, 2013; Iversen and Soskice, 2015; Macdonald, 2021). These tools may be particularly effective in the workplace, where competition between different information sources is less strong. Moreover, employee gatherings, voting for union officers, participation in hiring halls, and joint strike activities can improve communication networks between workers and create social experiences that transform them into more engaged citizens (Lindvall, 2013; McAdam *et al.*, 2001; Terriquez, 2011). Overall, we should expect workplace unionization to increase union members' participation and support for Democratic candidates.

Focusing the analysis on union members only would ignore an important out-group - the firm's management - that can alter any conclusion regarding the political influence of unions. The net effect of their reaction is ex-ante unclear. On one hand, labor unions may foster the management's understanding of worker issues and lead to an alignment of political preferences. Repeated interactions between workers and their management makes feedback mechanisms likely. Unionization establishes rules for the bargaining between managers and workers and may thus increase both the quantity and quality of communication between the two groups (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 suggests that this increase in cooperative interactions can enhance perspective-taking and reduce worker stereotypes held by management (e.g., [Allport, 1954](#)). Furthermore, labor unions aim to establish fairer rules at the workplace. For example, unions introduce formal grievance systems for employees and ensure representation of workers in the board of directors, which can itself lower tensions between the management and workers ([Verma, 2005](#)).¹

On the other hand, labor unions might cause a backlash from the management. Representing the interests of firm owners, managers typically are profoundly hostile to unionization.² The increase in bargaining power for workers implies a loss of status and power for the management. A large psychological literature has revealed that tensions between groups can increase if one feels threatened by the other (e.g., [Sherif et al., 1961](#); [Campbell, 1965](#)). Labor unions could thus increase the salience of labor conflicts. If true, that may increase polarization, as groups tend to adopt the stereotypes of the salient identity ([Bonomi et al., 2021](#)). Overall, it is not clear whether labor unions are able to persuade workers and their management or whether they enhance the the management’s opposition to workers’ political positions.

Identifying the causal impact of unionization on political preferences 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 only consider establishments with union elections, i.e., where workers have shown an interest in unionization. Thus, our sample can be expected to be more similar than a random sample of establishments. Within that sample, we compare campaign contributions from establishments where workers voted for unionization with establishments that voted against unionization by estimating a stacked DiD model. The stacked DiD accounts for issues arising in a setting with staggered treatment timing and heterogeneous treatment effects ([Goodman-Bacon, 2021](#)).³

Our main results indicate a leftward shift of both workers and managers. Quantitatively, the DiD estimates show that winning the union election increases the percentage difference in campaign contributions to Democrats versus Republicans by 12 percentage points for workers and by 20 percentage points for managers. These patterns are not in line with an increase in tensions between unionized workers and their management, but rather point toward a convergence

¹[Ash et al. \(2019\)](#) find that giving workers more authority through entitlements in collective bargaining agreements reduces labor conflicts, as measured through the frequency and intensity of strikes after negative wage shocks.

²In the run-up to union elections, employers frequently hire anti-union law firms and consultants, try to delay the election process, hold meetings in which employees are obligated to listen to the anti-unionization arguments, and - although legally restricted - threaten employees with dismissals and establishment closures ([Flanagan, 2007](#); [Freeman and Kleiner, 1990a](#); [Kleiner, 2001](#); [Logan, 2002](#); [Schmitt and Zipperer, 2009](#)).

³We 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.

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-term political mobilization of workers through a successful union campaign at the workplace.

The DiD design relies 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 do not find evidence that trends in the three election cycles prior to a union election evolved differently. Second, we test whether changes in 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 differential trends among establishments with different vote shares below 50%. Indeed, we do not find any evidence for differential changes across different vote-shares, which helps us to rule out the possibility that any sizeable confounding factors correlated with the pro-union vote share and the timing of the election drive the results. Third, we restrict the sample to establishments with increasingly close elections that are more likely to follow similar trends in contribution patterns. Our results are robust to a wide range of vote share bandwidths around the 50% cutoff, even when focusing on elections decided by only a 5-10% margin. Finally, we combine the DiD with a new instrumental variables (IV) approach in which we exploit arguably exogenous variation in union support from random shocks to the salience of workplace safety that are triggered by unexpected spikes in sector-level fatal work accidents shortly before the union election. The DiD-IV results support our main findings.

The observed influence of labor unions on workers and managers could be explained by a change in the composition of the employed workforce and management. 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 for 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. Second, we study individual-level effects by restricting our sample to individuals who were employed at the establishment before and after the union election and 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.

To study a potential mechanism underlying this result, we examine the role of Right-to-Work

(RTW) laws under which employees at unionized establishments do not have to pay union fees to reap the benefits of union representation. [Feigenbaum *et al.* \(2018\)](#) provide evidence that RTW laws put pressure on union revenues, forcing unions to shift scarce resources from political activities into membership recruitment activities and have aggregate consequences in terms of reduced turnout as well as fewer votes for Democratic candidates at the county level. Building on their analysis, we study how RTW laws affect the political responses of employees to unionization at the establishment level. We find the positive effects of unionization on contributions from workers and managers to Democratic versus Republican candidates to be smaller in states with RTW legislation. This finding highlights the role of unions' mobilization activities for their ability to raise support for their political agenda.

Finally, our data enable us to move beyond party preferences by considering candidates' ideological positions and the support of interest groups. For a start, 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. This suggests that our findings are not only driven by an increased signal of Democratic versus Republican partisan affiliation but reflect shifts between candidates with clearly distinguished ideological positions. In addition, we show that our results extend to contributions to 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 interest groups from workers and the reduced support for business interest groups from managers match with the observed pro-liberal shift in their contributions to candidates.

Our results contribute to several strands of literature. First, we complement the literature on the economic impacts of unions by providing insights on the political channel. Several studies have assessed the impact of unionization on wages and employee compensation at the establishment level ([DiNardo and Lee, 2004](#); [Frandsen, 2021](#); [Freeman and Kleiner, 1990b](#); [Knepper, 2020](#)). These studies document an absence of large wage effects but some positive effects on fringe benefits. The limited establishment-level effects are difficult to reconcile with evidence on the aggregate economic effects of unions. [Stansbury and Summers \(2020\)](#) show that declines in worker power can explain the entire decrease in the labor share of income in the US over the last decades. Moreover, [Western and Rosenfeld \(2011\)](#) and [Farber *et al.* \(2021\)](#) document negative effects of unions on income inequality, which they argue is hard to explain by income changes of union members alone, suggesting a potential link between unions and distributional legislation.⁴

⁴Several studies point toward an important role of unions in promoting greater political representation of

Second, we speak to the literature on the direct political influence of unions on their members. By comparing union members to non-union members, several studies have documented a significant association with political outcomes, such as voting (Freeman, 2003; Leighley and Nagler, 2007), preferences for redistribution (Mosimann and Pontusson, 2017), and trade liberalization support (Ahlquist *et al.*, 2014; Kim and Margalit, 2017).⁵ We add to these studies by assessing the causal impact of unions on campaign contribution patterns of workers. Campaign contributions are viewed as essential for candidates to win elections. Their influence on the set of candidates who run and win elections has been documented (e.g., Bekkouche and Cagé, 2018; Schuster, 2020). 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, 2018). 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.⁶

Third, we shed new light on the spread of political preferences at work through combining establishment-level union election data with an individual-level political outcome. The existing literature on the political impact of unions has focused either on individual union members and their households (e.g., Freeman, 2003) or on aggregate outcomes comprising the whole county or state population (e.g., Feigenbaum *et al.*, 2018). By focusing on the unionizing workplace, we are the first to consider within-firm dynamics and, in particular, the reaction of management – the out-group that is likely indirectly affected by unionization and a key actor when it comes to political influence. Thus, we relate to studies documenting contagion effects in political behavior in general (e.g., Nickerson, 2008), spillovers in political donations between managers and workers (Babenko *et al.*, 2020; Stuckatz, 2022), and effects of intergroup contact at the workplace on political preferences (Andersson and Dehdari, 2021).

The paper is organized as follows. Section 2 describes the institutional background, while Section 3 introduces the data. The empirical approach 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.

the working class. Sojourner (2013) shows that workers’ likelihood of serving as state legislator increases with their occupation’s unionization rate. Moreover, local union density is correlated with a more equal legislative responsiveness toward the poor vs. the rich (Flavin, 2018; Becher and Stegmüller, 2021). See also Ahlquist (2017) for a review on how unions affect economic and political inequalities.

⁵Union membership is also related to social attitudes more broadly, such as lower racial resentment (Frymer and Grumbach, 2021) and a stronger identification with the working class (Franko and Witko, forthcoming).

⁶Thus, we also contribute to the broader literature on political preference formation. It has been shown that context is a significant determinant of political behavior (Cantoni and Pons, 2022), while individual factors like early life experience (Jennings and Niemi, 2015) and education (Cantoni *et al.*, 2017) are also important. Our results highlight workplace unionization as one influential contextual factor that shapes political preferences.

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 secret ballot election at their establishment that is administered by the National Labor Relations Board (NLRB).⁷ The unionization procedure involves three main steps: a petition drive, an election, and certification.⁸

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. If the petition is accepted, the NLRB schedules a secret ballot election, which usually takes place at the workplace. 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 (CRS, 2013). 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. 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 can generally include all professional and nonprofessional employees at an establishment, managers and supervisors are always excluded.⁹ These employees are considered to be part of

⁷While union elections are the primary means by which private-sector workers gain union representation, there are alternative procedures for unionization. First, employers may voluntarily recognize unions without an election through neutrality agreements and “card checks”. These cases are less common, however, since employers generally oppose union organization (Schmitt and Zipperer, 2009). Second, some workers’ bargaining rights are not regulated by the NLRA. For example, the *Railway Labor Act* determines bargaining rights of airline and railroad workers and several federal, state, and local laws regulate the organization of public-sector employees.

⁸The description of the unionization process follows Frandsen (2021) and Wang and Young (2021).

⁹The NLRA uses a rather broad definition for supervisors. It includes all individuals who have the authority to assign and direct the work of other employees, as long as this involves some independent judgment. There is no

a firm’s management rather than its labor force and can therefore not join a union or be part of a bargaining unit. Representing the interests of capital owners, managers and supervisors typically oppose unionization and are thus treated as the “out-group” in our analysis. All other occupations form the “in-group”, as they are potentially in the bargaining unit and directly benefit from unionization.

2.2 Campaign Contributions in U.S. Politics

Money plays a dominant role in U.S. politics. Monetary resources are viewed as essential for candidates in order 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 increased over time from 55% in the 2002 elections (FEC, 2022a). 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.* (2022) estimate that 12.7% of the adult U.S. 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.¹⁰ 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 donate 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 greater 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 contribute to political candidates. First, contributions can be seen as consumption goods that give indi-

restriction as to the actual share of working time that involves supervisory duties. See Appendix B.3 for details.

¹⁰To make campaign donations, companies must set up a PAC, which may only solicit contributions from the firm’s employees. The PAC can in turn donate directly to political candidates or other recipients.

viduals consumption value from participating in politics and 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 great importance when deciding to whom to give (Barber, 2016a). Moreover, in comparison to access-seeking PACs, 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, 2018). 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 donating 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, however, Teso (2022) 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. A number of papers have therefore interpreted changes in campaign contribution patterns as indicators of changes in political ideology (e.g., Autor *et al.*, 2020; Bonica *et al.*, 2016; Dreher *et al.*, 2020).

3 Data

Previous studies have been unable to assess the political impact of unions at the establishment level due to a lack of matched employer-employee data for political outcomes. Campaign contribution data are uniquely suited to overcome this constraint. To ensure transparency in politicians' campaign funds, contributors are required to disclose their name, employer, address, and occupation. The employer and location information allows us to link donors to the 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. that would allow this link. Furthermore, we can use the occupation information to study the political effects of unionization not only on directly

affected non-managerial workers but also on potentially indirectly affected managers and supervisors. In the following, we describe how we construct a new establishment-level dataset that links union elections to campaign contributions from employees.

3.1 Union Elections

We start with a comprehensive dataset on the universe of U.S. union representation elections between 1961 and 2018. Specifically, we combine data collected by Farber (2016) with public data from NLRB election reports.¹¹ 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.

Sample restrictions. Before matching elections to campaign contributions, we impose several sample restrictions.¹² First, we only consider elections held between 1985 and 2010. Given that our contribution data cover 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.¹³ Excluding non-inaugural elections avoids having multiple observations for the same establishment with reversed treatment status over time, and helps alleviate 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 the first union election.¹⁴ These restrictions leave us with a sample of 28,823 union elections, which we seek to match to the campaign contribution data.

¹¹We obtain the dataset originally assembled by Farber (2016) 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>.

¹²See Appendix B.1 for details on the union election data and the sample restrictions we impose.

¹³In 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.

¹⁴This does not perfectly correspond to the effect of union representation in all post-election periods for 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.

Summary statistics. Table 1, Panel A, shows summary statistics for characteristics of the matched elections that are included in our final estimation sample (see details on the matching in the next subsection). 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 723,752 voters who participated in all elections of our sample.

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).¹⁵ DIME provides transaction-level data on campaign contributions registered with the FEC and other state and local election commissions. We exploit all campaign contributions from individuals to 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.¹⁶

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 establishment-level effects are driven by compositional changes from leaving and newly hired employees or by individual-level effects on employees remaining in the firm. Further, 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 that 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.

¹⁵Other 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).

¹⁶Accurate reporting of the donor information (name, employer, address, occupation) is enforced by the FEC through regular audits, as well as fines and further legal action in case of non-compliance. See FEC (2022b) for enforcement statistics.

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 incorrectly match employees to establishments of the same firm in other locations.¹⁷ To match the employer name in the 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.¹⁸ 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. Out of the 28,823 elections that we started with in the matching process, we only include establishments for which we have at least one matched contribution over this period. This leaves us with an estimation sample of 6,063 (21%) matched establishments (and 42,441 establishment-cycle observations).¹⁹ As Panel B of Table 1 reports, our sample is built from 357,436 matched contributions that amount to 105.8 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 B.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. Appendix Figure A.1 shows the occupation distribution for the classified donations. While the largest share (44%) is given 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

¹⁷We accept measurement error from assigning donors to the wrong establishment if a firm has several 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.

¹⁸See Appendix B.2 for details on the matching process.

¹⁹Appendix 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 strongly affect the selection of union elections in terms of voting outcome and industry composition.

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.

With the classified SOC codes at hand, we categorize donors into managers and supervisors versus non-managerial workers. We identify managers and supervisors by using all contributions from “Management Occupations” (SOC group 11) and adding all occupations that involve a significant amount of supervising following the NLRA definition of supervisor tasks and leveraging occupational task descriptions from O*NET. Non-managerial workers are then defined as all remaining donors to whom we were able to assign a SOC code. The occupational composition in our final sample of candidate contributions looks as follows: 42% of contributions originate from managers and supervisors (hereafter only termed “managers”), 30% from non-managerial workers (hereafter only termed “workers”), and for 28% we are unable to obtain an occupational 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).²⁰

Summary statistics. Table 2 reports mean contribution amounts after aggregation at the establishment-election cycle level. Managers donate 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 give a larger share of donations to committees than to candidates. In contrast, workers more often contribute directly to candidates.

Contributions spent by union organizations. To compare the contribution pattern of employees to those of unions, we also track campaign contributions originating from union organizations. Specifically, we consider all contributions from PACs associated with one of the unions in our matched sample, including local union branches. Appendix Table A.2 reports for each union the share of contributions to Democratic (as opposed to Republican) candidates as well as the ideology score obtained from Bonica (2014).²¹ On average, union PACs give 94% of their donations to Democrats, which demonstrates the strong link between labor unions and the Democratic Party.

²⁰In Appendix B.3 we also provide evidence that the likelihood of having a missing occupation classification is not affected by unionization and therefore unlikely to drive our results.

²¹For 685 out of the 6,063 elections in our estimation sample, we are not able to match any PAC contribution.

4 Empirical Strategy

We aim at estimating the causal effect of unionization on the 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 because 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 similar than a random sample of unionized and non-unionized establishments.²² Pre-existing ideological differences are nevertheless visible: Workplaces that vote for unionization donate more to Democratic candidates and less to Republican candidates even before the union election.

To account for pre-existing differences, we implement a Difference-in-Differences 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 the fact that we observe the pro-union vote share, which 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.²³ Finally, to cross-validate the causal interpretation of our results, we also develop a novel identification strategy which combines the DiD with an IV approach. For this, we exploit variation in unionization resulting from exogenous shocks to the salience of safety at work that are triggered by unexpected fatal workplace accidents shortly before the union election.

²²Dinlersoz *et al.* (2017) examine selection into union elections 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 that hold union elections.

²³Many papers on the effects of unionization follow RD designs by comparing establishments in which the union barely won versus where it barely 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. Appendix Figure A.2 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.

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 for employees in establishment i and relative event time k . We observe each establishment from 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 δ_{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., whether the pro-union vote share, V_i , is above 50%. α_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 β_{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 potentially heterogeneous, cohort-specific treatment effects in staggered DiD settings such as ours (de Chaisemartin and D’Haultfoeulle, 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 by time relative to 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 δ_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 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 δ_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 (2021) and analyze whether changes in outcomes are different among losing elections with different vote shares. If unobserved shocks were driving voting results that led to union victory or loss, we would also expect them to affect outcomes in losing elections with different union vote shares.²⁴ To implement this test, we modify the DiD model as follows:

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

where ν^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.

Third, 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

²⁴Wang and Young (2021) 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.

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 barely 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.

Alternative source of variation. Lastly, we describe our DiD-IV approach, which complements the DiD strategy with arguably exogenous variation in union support driven by spikes in work-related fatalities. After the NLRB accepts a petition to hold a union election, it sets the timeline of the unionization process and fixes an election date. Any random unexpected shocks between petition and election that shift union support are then potential candidates for an instrument. We focus on sector-level fatal work accidents in the 30 days before a union election.²⁵ Safety at work is a fundamental concern to all workers, especially when one’s life is in danger. Work-related fatalities are unfortunately still common in the United States. In 2019, the Occupational Safety and Health Administration (OSHA) reported 1,943 deaths at work, more than 5 per day on average. Unions often campaign on safety issues and are found to improve safety conditions at the workplace (e.g., [AFL-CIO, 2022](#); [Hagedorn *et al.*, 2016](#); [Li *et al.*, 2022](#)).

We implement the DiD-IV approach by estimating the following two-stage model:

$$V_i = \alpha_1 + \alpha_2 A_{st} + \alpha_3 A_{st}^2 + \alpha_4 A_{st} \times FR_s + \alpha_5 FR_s + \alpha_6 X_i + \gamma_t + \mu_m + \epsilon_i \quad (4)$$

$$\Delta y_i = \beta_1 + \beta_2 \mathbb{1}[\widehat{V}_i > .5] + \beta_3 FR_s + \beta_4 X_i + \gamma_t + \mu_m + \epsilon_i, \quad (5)$$

where Δy_i denotes the change in campaign contribution patterns in the three election cycles after the union election relative to the three cycles before (excluding the cycle of the union election). By using changes as the outcome variable, the specification builds on the DiD approach and accounts for time-invariant differences between establishments that may affect the level of campaign contributions. Our main instrument is A_{st} , which represents the number of fatal accidents in 2-digit sector s in the 30 days prior to the election after accounting for seasonal variation.²⁶ We allow for a non-linear effect by including A_{st}^2 and for a larger impact of fatalities

²⁵The median time between petition and election in our sample is 47 days. Only 1% of all elections are held within 30 days after the petition.

²⁶Data on fatal work accidents is obtained from OSHA in the form of Fatality and Catastrophe Investigation

in sectors where fatalities are common and where workers may be more concerned about workplace safety by the interaction term $A_{st} \times FR_s$ (FR_s denotes the share of fatal work accidents occurring in a given sector out of all fatal work accidents in the sample). Importantly, instead of directly instrumenting union victory in a standard 2SLS approach, the first stage explains the continuous pro-union vote share V_i . In the second stage, we then use an indicator for predicted victory that is based on the predicted vote share in the first stage. This approach resembles the treatment assignment process and exploits the maximum available information. To account for the uncertainty from the first-stage regression, we compute standard errors using bootstrapping. In addition, we include a number of control variables. First, we account for the main effect of FR_s . Second, we include the yearly number of fatalities in a sector to ensure that sector-specific trends in fatalities do not drive our results. Third, we add the log number of employees at the sector-year level and the log number of eligible voters as precision controls. Finally, we include year fixed effects γ_t and month-of-the-year fixed effects μ_m .

The exclusion restriction of the instrument relies on the notion that a shock in fatal work accidents in the same sector affects political behavior only through its impact on the likelihood that an establishment will unionize. Two points are worth highlighting in that regard. First, all individuals in our sample are potentially exposed to the information on fatal work accidents. However, only some vote on unionization in the following 30 days. That is to say, we do not exploit differences in the direct exposure to work accidents but differences in the timing of the union election relative to the information shock. Second, we are solely focusing on the medium-term impact of spikes in fatal accidents by considering campaign contributions in the three cycles after the union election. The result that common shocks in fatal work accidents influence political behavior years afterward in some but not other establishments would be difficult to explain other than through the path dependency triggered by the increase in the likelihood of unionization shortly after the accidents.

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.²⁷ Our second main outcome is

Summaries (OSHA form 170). Appendix Figure A.3 depicts the exploited time variation.

²⁷The 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 use it to transform non-negative variables with zeros (e.g., Bahar and Rapoport, 2018; Bursztyń *et al.*, 2022; McKenzie, 2017).

the difference between the IHS-transformed contribution amounts to Democratic and Republican candidates. This measure approximates 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 positions.

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 union election establishments relative to losing ones. Overall, the figure suggests a shift in 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 δ_s along with the pooled average treatment effect δ_{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 union 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-term 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.

Next, we assess changes in the party composition of campaign contributions. If unions are

able to change individuals’ political views or mobilize different subgroups at the workplace, campaign contributions will shift to different candidates. The right-hand side of Figure 2 plots the effect of unionization on the difference between the (IHS-transformed) amounts donated 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 in contributions donated to Democratic relative to Republican candidates. The effect on partisan support appears to be strongest in the long term, 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 toward an alignment of political preferences.

5.2 Addressing Identification Challenges

DiD-RDD. We continue presenting results for our RD-motivated tests to probe the validity of the underlying parallel trends assumption of the DiD model.²⁸ Figure 3 focuses on the measure of partisan contribution composition, while effects on the total amount of contributions are presented in Appendix Figure A.5. 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 δ_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%, for contributions from both 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 unob-

²⁸One particular concern for the parallel trends assumption would arise if union elections were endogenously timed around federal election dates. Appendix Figure A.4 investigates whether union elections follow political cycles. Across years with and without federal elections, there are no strong differences in the number of union elections held and the probability of winning a union election, 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.

served shocks correlated with voting results that could drive our results.²⁹ Moreover, the results 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 include 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.

DiD-IV. We also assess the sensitivity of our DiD results when exploiting arguably exogenous variation in unionization from shocks to the salience of workplace safety before the union election. Table 3 reports the results of our DiD-IV approach. The first-stage results show that sector-level fatal work accidents are a significant predictor of the union election outcome, with an F-statistic of 16.5. We find that the positive effect of spikes in work accidents on unionization is stronger in sectors where work accidents are more common, i.e., where workplace safety may be a greater concern for workers. The second-stage results confirm our main findings from the DiD model, highlighting a leftward shift in campaign contributions in response to unionization.³⁰

²⁹In Appendix Figure A.6, 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):

$$y_{ik} = \alpha_i + \beta_{kgi} + \sum_g \delta_g^{PRE} \times \left(\mathbb{1}[k < -1] \times \mathbb{1}[V_i \in \nu^g] \right) + \sum_g \delta_g^{POST} \times \left(\mathbb{1}[k \geq 0] \times \mathbb{1}[V_i \in \nu^g] \right) + \epsilon_{ik} \quad (6)$$

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

³⁰We also report results when estimating model (5) by OLS. Given that the outcome is the change in outcomes before vs. after the election, the results are very similar to those obtained from our main DiD model (1). Small

The magnitude of the coefficients is comparable but slightly larger than in the DiD model. As compliers respond to information on fatal work accidents, we deem it plausible that they also react more strongly to information provided by unions and to changes to their work environment induced by unionization. The estimates are considerably less precise, however. While the effects on the party composition of contributions from managers are still significant at the 5% level, the effects for workers are no longer significant. We thus focus our DiD-IV approach on validating the main results and proceed with our main DiD model for the analysis of mechanisms in Section 6.³¹

5.3 Robustness

We now discuss further robustness checks for our main DiD estimates. Results are presented in Appendix Tables A.3, A.4, and A.5.

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 the use of early-treated units 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.³² In Panels B and C of Appendix Table A.3, 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 stacked DiD results.

Alternative outcome transformations. [Roth and Sant’Anna \(forthcoming\)](#) 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

differences arise from the inclusion of additional controls in model (5) and the exclusion of the cycle in which the union election takes place.

³¹We also verify the DiD-IV approach with a falsification exercise. We re-estimate model (5) using the change in campaign contribution patterns between $t - 1$ and $t - 2$ as the outcome. We do not find any evidence for pre-existing differential trends related to spikes in fatal work accidents.

³²In 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 outcome over all pre-treatment periods, whereas [Callaway and Sant’Anna \(2021\)](#) only use 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 through 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.

assumptions. We therefore test the sensitivity of our results to alternative outcome transformations. First, instead of transforming contribution amounts with 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.4, we check whether our results are sensitive to the exact definition of managers and supervisors versus non-managerial 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 for Democrats relative to Republicans.

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 for 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 term, 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 establishments hold and lose elections, we exploit only variation in the timing of union elections and use establishments that hold and lose an election in the future as control group. We implement this approach in a stacked DiD model similar to our baseline model (1).³³ Results are presented in Appendix Table A.5. We obtain small and 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.

³³See Appendix C 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.

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 found for both workers and managers.

6 Potential Mechanisms and Extensions

6.1 Compositional versus Individual-Level Effects

One potential explanation for the establishment-level effects may be compositional changes regarding what type of employees separate from and are newly hired into unionized establishments. [Frandsen \(2021\)](#) finds that unionization leads older and higher-paid workers to leave and younger workers to 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 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 4, columns (1) and (2), show very small and almost always insignificant DiD estimates, indicating that the set of post-election employees does not differentially change in unionized versus non-unionized establishments in terms of pre-existing contribution amounts. Only for workers do we see a marginally significant estimate in line with more Democratic workers entering union jobs (or fewer 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.³⁴

³⁴Note that the compositional analysis is complicated by the fact that we only observe employees if they contribute. In principle, our compositional test may thus also pick up changes in the extensive margin in terms of which employees stop donating after the union election. As regards candidates' party affiliation, we would expect that unionization decreases [increases] the likelihood that employees stop donating to Democrats [Republicans].

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 from 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).³⁵ Estimates are reported in columns (3) and (4) of Table 4. 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.³⁶ When restricting the sample to workers, we see a significant rise in total donations, which is, however, entirely driven by an increase in support for Democrats. For managers, the results indicate a significant 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 donation patterns rather than by compositional effects.³⁷

6.2 Political Involvement and Ideology of Union Organizations

Unions are not a uniform political force, but can rather be understood as heterogeneous and evolving organizations that vary in their internal governance and institutional environments. Ahlquist and Levi (2013) and Kim and Margalit (2017) show that unions differ in the importance they place on political activities, in the intensity and form of communication with members, and in their policy views. We therefore seek to study the role that varying political activities and positions of union organizations play in moderating our results.

First, we examine how effects differ by whether or not unionization takes place under a state-level Right-to-Work law. RTW laws allow employees to enjoy the benefits of collective

Then, the extensive margin channel would yield a positive effect on contributions to Democrats relative to Republicans that post-election employees donated before the election, in line with what we expect for the actual compositional effect. Our results show that the sum of both effects is small, suggesting that both effects play a minor role.

³⁵We refrain from aggregating contributions for each relative cycle k separately. Since we do not know an individuals' employing establishment if the individual does not donate in a given cycle, we are not able to construct a balanced panel over all cycles that includes observations with zero amounts.

³⁶Note 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.

³⁷Another composition effect potentially explaining our establishment-level result may arise from transitions of individuals across occupational groups. To rule out that the promotion of workers to management positions is driving our results for managers, in columns (3) and (4) of Table 4 we have classified individuals as *managers* only if they held a manager position both before and after the election. Individuals who have some matched contributions with an occupation categorized as *manager* and some categorized as *worker* are all included in the *worker* subsample.

bargaining and union representation without having to become a union member and pay fees. [Feigenbaum et al. \(2018\)](#) study in detail the political consequences of RTW legislation. They provide evidence that RTW laws put pressure on union revenues and force unions to reallocate scarce resources from political activities (such as lobbying, voter mobilization, candidate recruitment, or donating campaign contributions) into membership recruitment activities. The reduced political involvement of unions following the passage of RTW laws is found to have aggregate consequences in terms of lower turnout and reduced vote shares for Democratic candidates at the regional level. We complement this analysis by studying how RTW laws moderate the effect of unionization on campaign contributions from employees at the establishment level. It is the unionized workplace where unions are directly connected to employees and where RTW laws may thus have a large impact on unions' political influence. To analyze this, we split our estimation sample based on whether or not the union election takes place in a state that has a RTW law in force at the time of the election. Results are presented Table 5, Panels A.1 and A.2. In states without RTW laws, we see significantly positive effects of unionization on support for Democratic (relative to Republican) candidates, while for RTW states the coefficients are smaller and not significant. This is true for all employees as well as for workers and managers separately. Thus, fewer political mobilization efforts under RTW legislation seem to decrease unions' ability to channel campaign contributions from employees at unionized workplaces.

Second, we investigate whether results vary across union organizations with different ideological positions. We exploit union-level differences in ideology scores, which are derived from [Bonica \(2014\)](#) and based on the campaign contributions that union organizations donate themselves. Note that all unions in our sample have ideology scores substantially below zero and can be clearly viewed as liberal donors (see Appendix Table A.2). Nevertheless, we partition the sample of union elections into union organizations with an ideology score below vs. above the sample median. The mean ideology scores in the two subsamples are $-.807$ and $-.654$, meaning that we only compare somewhat more and less liberal unions. The estimated effect sizes, shown in Panels B.1 and B.2 of Table 5, are similar across the two groups. We thus do not find evidence for differential effects by union ideology.³⁸ Together, the results suggest that unions' political activities matter more for their political impact on employees than their ideological position (in which we observe little variation).

³⁸Results are similar when we split by terciles of ideology scores.

6.3 Differentiating Recipients

So far, we have distinguished recipients of campaign contributions only with respect to their party affiliation. We now examine candidate heterogeneity more closely by considering ideological differences among candidates within the Democratic and the Republican party and by differentiating between candidates in federal and local elections. Moreover, we study whether the observed changes in contributions to candidates extend to contributions to political action committees.

Within-party ideological differences. Our results show that unionization increases support for 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 affiliation. To further examine the ideological patterns in campaign contributions, we study ideological differences among candidates within the same party. 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 between "moderate" and "conservative" Republicans using the median Republican CF score. [Table 6](#) shows results from our DiD model, 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 the within-party ideological positions of candidates. Unionization significantly increases employees' support for the most liberal Democrats and decreases support for the most conservative Republicans. In contrast, contributions to moderate Democrats or Republicans are not significantly affected. These results are similar when we focus on donations from managers only, and also for workers the increased support for Democrats is more pronounced for more liberal Democrats. Overall, our effects appear to be driven by a shift in contributions between clearly distinguishable conservative and liberal candidates (instead of a shift at the margin from moderate Republicans to moderate Democrats).

Federal versus local candidates. We continue by examining whether our effects are limited to contributions to 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.3](#) 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 Democrats in response to unionization (and no effect on total amounts).

Contributions to political action committees. In Table 2, we have shown that contributions to PACs account for a large share of political contributions from employees. If unions particularly encourage workers to donate to candidates, this may come at the detriment of workers' contributions to committees. On the other hand, if unions mobilize workers to participate by donating to labor PACs, then we will underestimate the total effect of unionization on political donations. Table 7 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 given to these committees, we also measure partisan support by the difference in contribution amounts to Democratic versus Republican PACs. For interest-group PACs, party affiliation is determined from the recipients of the PAC's own campaign contributions.³⁹ Considering first the 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 donated to both party/candidate committees and interest-group PACs, which implies that unions are successful in mobilizing PAC contributions from workers. The increase in donations appears to be driven by membership and labor organizations, pointing toward 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 to Democratic rather than Republican PACs. In particular, donations to corporate PACs drop, which highlights that unionization can decrease managers' support for business interest groups. Overall, these results match with the observed pro-liberal shift in workers' and managers' contributions to political candidates.

³⁹To track contributions that PACs donate themselves, we exploit that Bonica (2019) has matched recipient identifiers to contributor identifiers for recipients' own contributions. Based on the matched outgoing contributions from PACs, we define an interest-group PAC as "Democratic" ("Republican") if more (less) than 50% of its campaign contributions goes to Democratic candidates in a given election cycle.

7 Conclusion

Labor unions employ vast resources to shape labor policies and welfare regulations through political activities such as lobbying legislators or supporting candidates financially. Lasting change, however, requires changes in preferences and beliefs. Do unions influence political preferences? To understand the political power of labor unions, it is important to understand their effect on millions of individuals at the unionized workplace. At work, unions can provide information and shape social interactions among employees that affect their political behavior. Importantly, unions' aggregate political impact does not only depend on their effect on the in-group that benefits from unionization, but also hinges on the reaction of potential out-groups, in particular the firm's management. Managers' power at the workplace and in politics makes their response to unionization particularly relevant for the assessment of the overall impact of unionization.

This paper analyzes the political effects of workplace unionization, building on an establishment-level dataset that combines union elections with campaign contributions from employees spanning the 1980-2016 period in the United States. Comparing establishments with an interest in unionization that won and lost the union election in a stacked DiD model, 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 by changes of political behavior at the individual level. Overall, we show that labor unions influence the political preferences not only of union members but also of their firms' management.

The results are indicative of a reduction of worker-manager cleavages in political preferences, which is consistent with an improvement in workplace labor relations. If unionization fosters bargaining and communication between workers and managers on a more equal playing field, contact theory suggests an enhancement in managers' understanding of workers' political preferences. While the results may appear surprising in light of the strong opposition of employers toward unions in the United States, a distinction between ex-ante beliefs and ex-post effects of unionization seems crucial. The literature has found little evidence that unionization leads to higher wages (DiNardo and Lee, 2004; Frandsen, 2021; Freeman and Kleiner, 1990b) or reduced productivity (Dube *et al.*, 2016; Sojourner *et al.*, 2015), which could lower firms' profitability. We welcome future work that studies more closely how managers form beliefs about unionization.

Our findings may have implications for broader developments in US politics. The long-standing decline in private-sector union density from 24.2% in 1973 to 6.1% in 2021 (Hirsch

and Macpherson, 2022) implies that millions of individuals have forfeited the engagement with unions, which has led to lasting shifts in political preferences. The erosion of unionization can be an important contribution to the increased alignment of workers with the political right that has been observed over the last decades (Gethin *et al.*, 2022). More recently, labor shortages during the COVID-19 pandemic have led to a renewed interest in labor activism. Prominent examples of strikes and union petition drives in Starbucks shops, Amazon warehouses, and healthcare facilities suggest a moment of resurgence for labor organization. Whether this trend persists may be consequential for the balance of political power and support for pro-labor politics in the United States.

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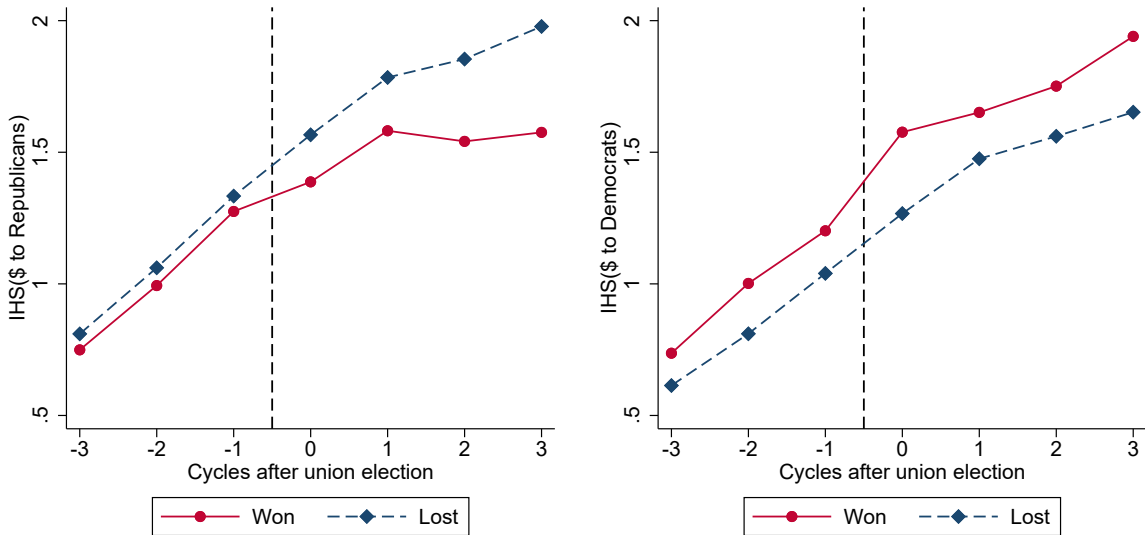
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Figures and Tables

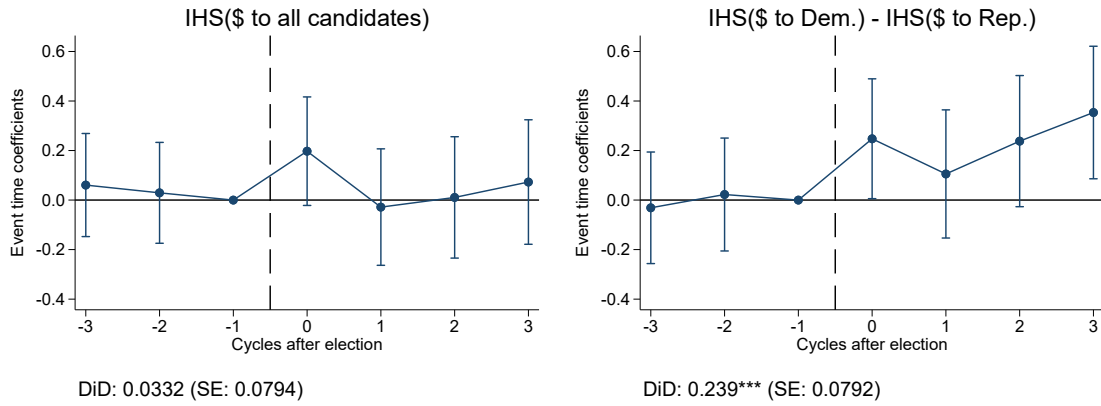
Figure 1: Trends in Contributions for Won and Lost Union Elections



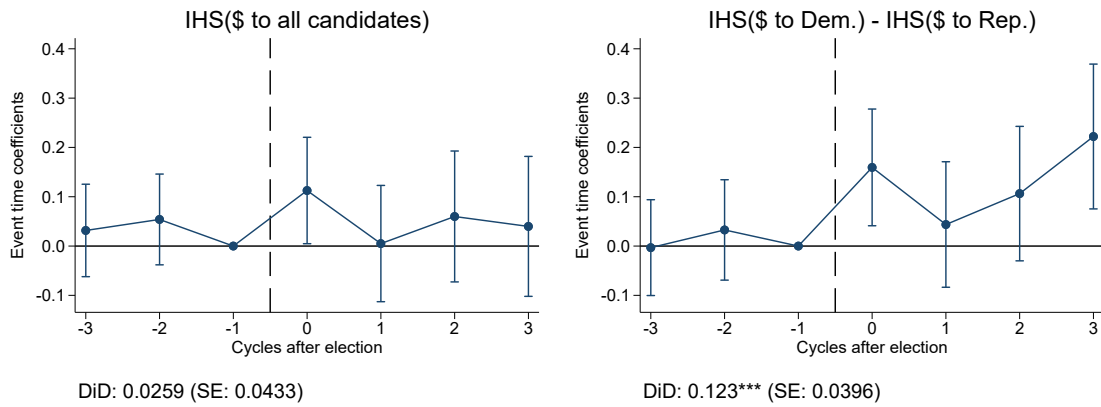
Notes: The figure depicts trends in mean contribution amounts of all employees in an establishment by union election outcome and election cycle relative to the union election. The left (right) graph shows means of IHS-transformed amounts to Republican (Democratic) candidates. $N = 42,441$ establishment-cycle observations.

Figure 2: Effect of Unionization on Candidate Contributions

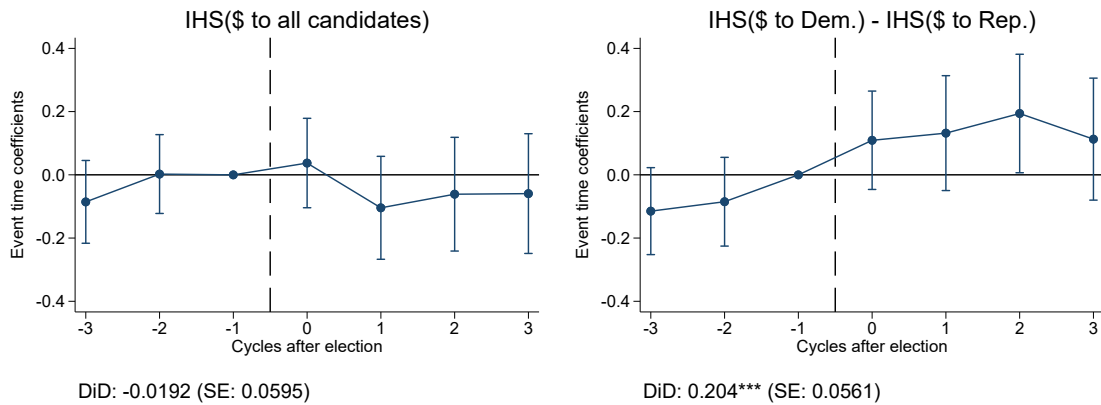
All employees



Workers

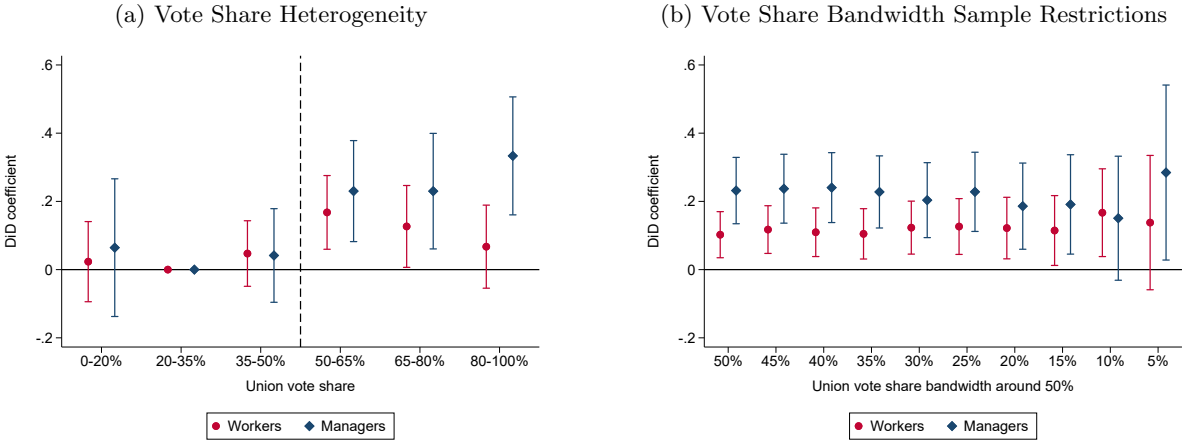


Managers



Notes: The figures report the event-study coefficients δ_s estimated in 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. $N = 33,103$ 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 amounts contributed to Democratic and Republican candidates. Results are reported for contributions from all employees (top part), from only non-managerial workers (middle part), and from only managers and supervisors (lower part). 95% confidence intervals are depicted for standard errors clustered at the establishment level.

Figure 3: Effect of Unionization on Democratic versus Republican Support - DiD-RDD Results



Notes: The graphs show RD-type placebo and robustness tests for the effect of unionization on the difference between the IHS-transformed amounts contributed to Democratic and Republican candidates. Panel (a) reports the δ_g coefficients estimated in 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 from non-managerial workers (“workers”) and from 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	6,063	3,397	2,666
Union vote share (average)	.4950	.3204	.7175
Number of votes (average)	119.37	135.31	99.06
Number of votes (total)	723,752	459,661	264,091
[B] Contribution characteristics			
Amount (total, in million 2010 USD)	105.82	65.38	40.43
Number of contributions (total)	357,436	204,797	152,639
Number of donors (total)	46,719	26,661	20,243
Number of recipients (total)	9,942	7,208	5,681

Notes: Data from NLRB union 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:	All employees	Workers	Managers
Recipient:			
All	2,493.24	313.80	1,339.38
Candidates	1,181.96	173.42	594.44
Democratic candidates	575.85	112.79	261.76
Republican candidates	586.98	56.61	320.66
Political action committees	1,311.28	140.38	744.94
Party/candidate PACs	364.92	52.52	192.77
Interest-group PACs	937.22	86.37	549.31

Notes: The table reports mean values for the amount contributed in each of the 42,441 establishment-cycle combinations in the estimation sample. All amounts are in 2010 USD. Values are reported separately for contributions from all employees, from only non-managerial workers (“workers”), and from only managers and supervisors (“managers”). The difference in the amounts from all employees and the total from workers and managers is driven by contributions for which we were unable to classify the occupation.

Table 3: DiD-IV Results

	IHS(\$ to all candidates)			IHS(\$ to Dem.) – IHS(\$ to Rep.)		
	All (1)	Workers (2)	Managers (3)	All (4)	Workers (5)	Managers (6)
[A]: OLS						
$\mathbb{1}[\widehat{V}_i > .5]$	-0.092 (0.082)	0.038 (0.044)	-0.072 (0.062)	0.227*** (0.079)	0.089** (0.041)	0.232*** (0.056)
[B]: 2nd stage						
$\mathbb{1}[\widehat{V}_i > .5]$	0.036 (0.174)	0.086 (0.097)	-0.042 (0.134)	0.334* (0.176)	0.115 (0.086)	0.260** (0.125)
[C]: 1st stage						
A_{st}	0.002 (0.007)	0.002 (0.007)	0.002 (0.007)	0.002 (0.007)	0.002 (0.007)	0.002 (0.007)
A_{st}^2	-0.003** (0.001)	-0.003** (0.001)	-0.003** (0.001)	-0.003** (0.001)	-0.003** (0.001)	-0.003** (0.001)
$A_{st} \times FR_s$	0.223*** (0.055)	0.223*** (0.055)	0.223*** (0.055)	0.223*** (0.055)	0.223*** (0.055)	0.223*** (0.055)
K-P F-stat	16.50	16.50	16.50	16.50	16.50	16.50
[D]: 2nd stage falsification: pre-trend						
$\mathbb{1}[\widehat{V}_i > .5]$	-0.007 (0.207)	0.093 (0.094)	0.033 (0.116)	0.124 (0.230)	-0.020 (0.100)	0.046 (0.129)

Notes: The table reports results from the DiD-IV approach for the effect of unionization on the IHS-transformed total amount contributed (columns (1) - (3)) and on the difference between the IHS-transformed amounts contributed to Democratic and Republican candidates (columns(4) - (6)). Panel A reports OLS coefficients, Panel B reports the second-stage coefficients from model (5), and Panel C reports the first-stage coefficients from model (4). In Panels A and B, the outcome is the difference between the average outcome in the three cycles after and the average outcome in the three cycles before the union election (excluding the cycle of the union election). In Panel D, the outcome is the change between one and two cycles before the union election. $N = 5,803$ establishments. Bootstrapped standard errors (with 500 replications), shown in parentheses, are clustered at the establishment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Composition versus Individual-Level Effects

	Composition effects		Individual-level effects for stayers	
	IHS(\$ to all candidates) (1)	IHS(\$ to Dem.) – IHS(\$ to Rep.) (2)	IHS(\$ to all candidates) (3)	IHS(\$ to Dem.) – IHS(\$ to Rep.) (4)
[A]: All employees				
δ_{DiD}	-0.0265 (0.0696)	0.0705 (0.0636)	0.196 (0.135)	0.552*** (0.188)
N	33103	33103	5740	5740
[B]: Workers				
δ_{DiD}	0.0455 (0.0363)	0.0534* (0.0294)	0.624*** (0.233)	0.648** (0.309)
N	33103	33103	2052	2052
[C]: Managers				
δ_{DiD}	-0.0666 (0.0514)	0.0371 (0.0454)	-0.0718 (0.186)	0.532** (0.261)
N	33103	33103	2890	2890

Notes: The table reports DiD coefficients for the composition and individual-level effects of unionization on the IHS-transformed total amount contributed (columns (1) and (3)) and on the difference between the IHS-transformed amounts contributed to Democratic and Republican candidates (columns (2) and (4)). In columns (1) and (2), the establishment-level outcomes for the post-election periods are constructed from pre-election contributions from 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 include establishments / individuals from establishments with a pro-union vote share between 20 and 80%. Standard errors, shown in parentheses, are clustered at the establishment level in columns (1) and (2) and at the individual level in columns (3) and (4). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Heterogeneous Effects by Political Involvement and Ideology of Union Organizations

	IHS(\$ to all candidates)			IHS(\$ to Dem.) – IHS(\$ to Rep.)		
	All (1)	Workers (2)	Managers (3)	All (4)	Workers (5)	Managers (6)
[A.1]: State without right-to-work law						
	0.0453 (0.0896)	0.0663 (0.0499)	-0.0394 (0.0672)	0.284*** (0.0884)	0.131*** (0.0456)	0.218*** (0.0635)
N	26208	26208	26208	26208	26208	26208
[A.2]: State with right-to-work law						
δ_{DiD}	-0.0548 (0.170)	-0.119 (0.0820)	0.00832 (0.125)	0.0164 (0.177)	0.0700 (0.0769)	0.142 (0.117)
N	6895	6895	6895	6895	6895	6895
[B.1]: More liberal union organization (below median CF score)						
δ_{DiD}	0.0250 (0.115)	0.00389 (0.0642)	-0.0780 (0.0857)	0.251** (0.116)	0.0826 (0.0596)	0.197** (0.0837)
N	14875	14875	14875	14875	14875	14875
[B.2]: Less liberal union organization (above median CF score)						
δ_{DiD}	0.0864 (0.124)	0.0406 (0.0615)	0.0416 (0.0912)	0.240** (0.120)	0.119** (0.0543)	0.186** (0.0834)
N	14882	14882	14882	14882	14882	14882

Notes: The table presents DiD coefficients, estimated in model (1), for the effect of unionization on the IHS-transformed total amount contributed (columns (1) - (3)) and on the difference between the IHS-transformed amounts contributed to Democratic and Republican candidates (columns (4) - (6)). Panels A.1 and A.2 distinguish between establishments in states with versus without right-to-work laws in the union election year. Panels B.1 and B.2 report results for elections of union organizations with an ideology score below vs. above the median ideology score of all elections in our estimation sample. Unions' ideology scores are derived from [Bonica \(2014\)](#) and based on the campaign contributions that union organizations donate themselves (see [Table A.2](#)). All samples include 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 6: Differentiating Candidates by Within-Party Ideology

	Democrats			Republicans		
	All (1)	Moderate (2)	Liberal (3)	All (4)	Moderate (5)	Conservative (6)
[A]: All employees						
δ_{DiD}	0.0920 (0.0634)	-0.0182 (0.0544)	0.121*** (0.0462)	-0.147** (0.0654)	-0.0686 (0.0547)	-0.153*** (0.0494)
[B]: Workers						
δ_{DiD}	0.0728** (0.0352)	0.0308 (0.0237)	0.0550* (0.0298)	-0.0502 (0.0317)	-0.0155 (0.0225)	-0.0309 (0.0257)
[C]: Managers						
δ_{DiD}	0.0735 (0.0467)	0.0129 (0.0391)	0.0896*** (0.0347)	-0.130*** (0.0490)	-0.0563 (0.0397)	-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 establishments with a pro-union vote share between 20 and 80%. $N = 33,103$ establishment-cycle observations. Standard errors clustered at the establishment level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Contributions to Political Action Committees

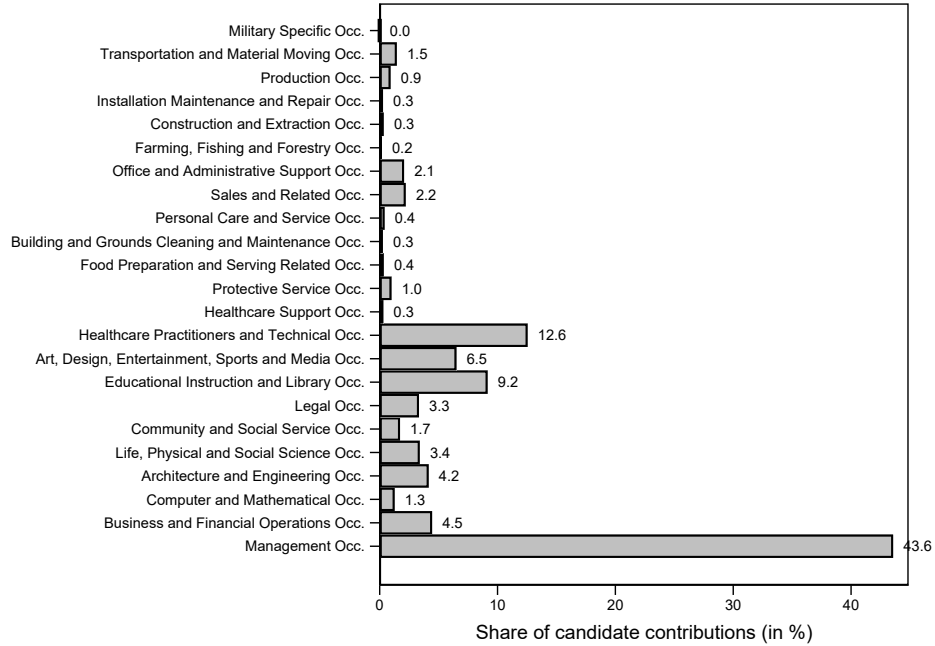
	Party/candidate PACs		Interest-group PACs					
	All	Dem – Rep	All	Corporation	Trade assoc.	Member orga.	Labor orga.	Dem – Rep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
[A]: All employees								
δ_{DiD}	-0.0255 (0.0522)	0.0968** (0.0478)	-0.0824 (0.0635)	-0.0929** (0.0409)	-0.0261 (0.0440)	-0.00886 (0.0311)	0.0168 (0.0109)	0.0599 (0.0407)
[B]: Workers								
δ_{DiD}	0.0624* (0.0320)	0.00991 (0.0275)	0.0876** (0.0347)	-0.0199 (0.0205)	0.0211 (0.0158)	0.0461** (0.0190)	0.0188*** (0.00709)	0.0239 (0.0266)
[C]: Managers								
δ_{DiD}	-0.000602 (0.0344)	0.102*** (0.0315)	-0.0931* (0.0488)	-0.0821** (0.0340)	-0.0259 (0.0331)	0.000722 (0.0179)	0.00369 (0.00684)	0.0810** (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 amounts contributed to Democratic and Republican committees. Interest-group PACs are categorized as “Democratic” (“Republican”) if more (less) than 50% of their own campaign contributions goes to Democratic candidates. The sample includes establishments with a pro-union vote share between 20 and 80%. $N = 33,103$ establishment-cycle observations. Standard errors clustered at the establishment level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Online Appendix

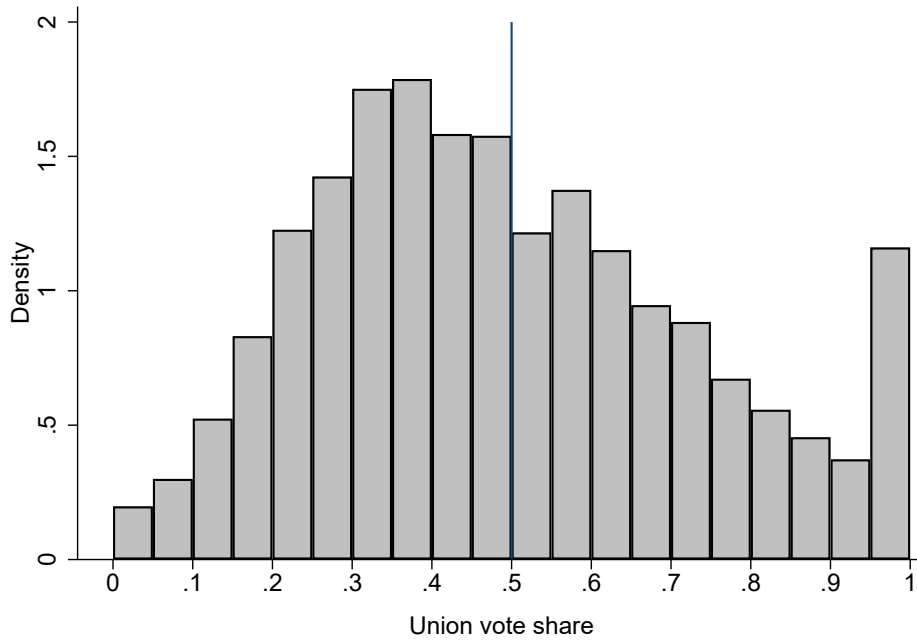
A Additional Figures and Tables

Figure A.1: Donor Occupations



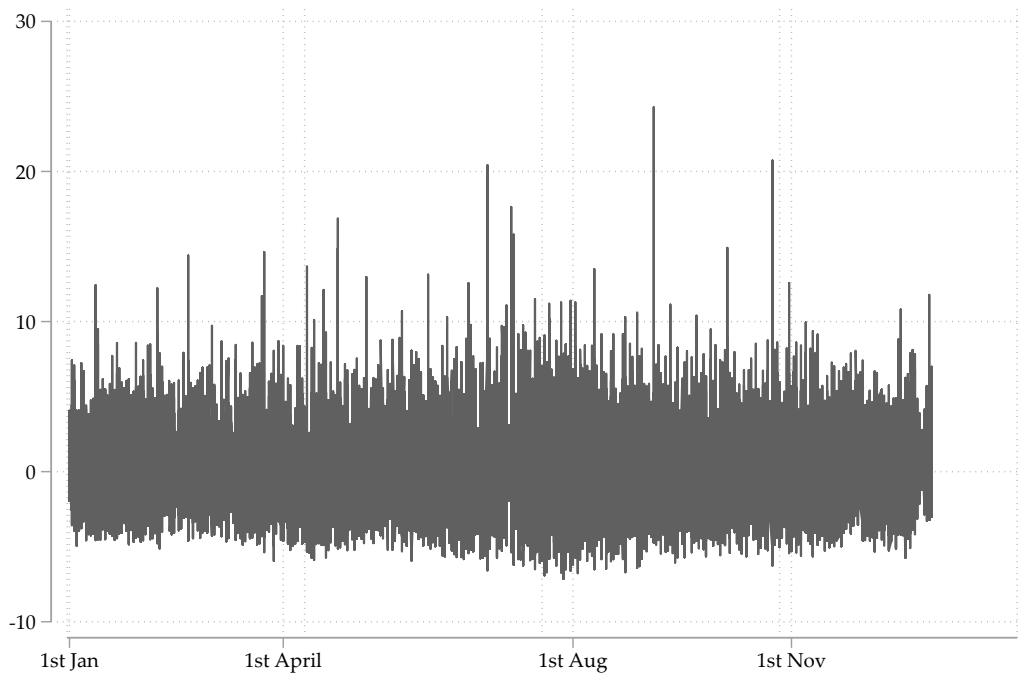
Notes: The figure shows the distribution of occupations for all candidate contributions that are included in our matched estimation sample and have a classified occupation. For 28.1% of the contributions we were not able to assign an occupation code. Occupation groups are 2-digit codes of the 2018 Standard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

Figure A.2: Vote Share Distribution



Notes: The figure plots the density of union vote shares for all 6,063 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 $k = .02$).

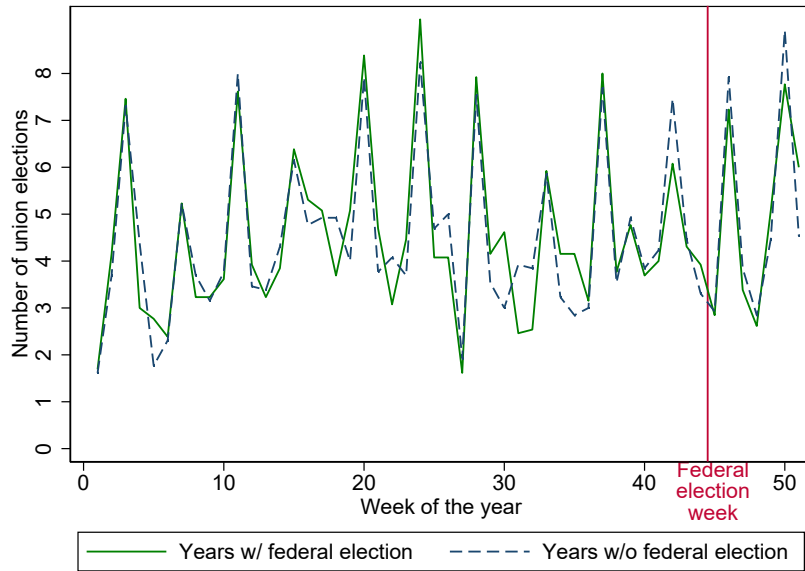
Figure A.3: Seasonally Adjusted Fatal Work Accidents, 1984-2012



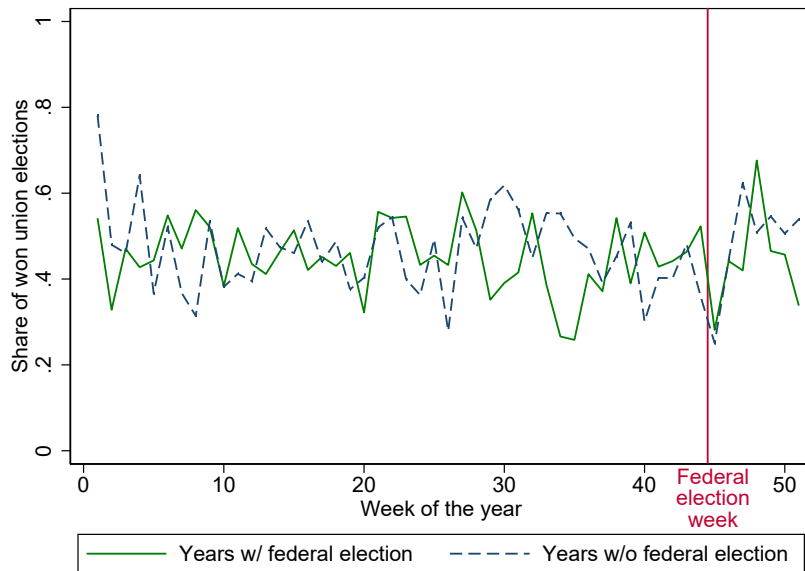
Notes: The graph shows the number of fatalities caused by work accidents on a given day of a year (e.g., January 1st) for all years in our sample period after the mean number of fatalities on that given day over our sample period (e.g., mean number of fatalities on January 1st between 1984 and 2012) is subtracted.

Figure A.4: Cyclicity of Union Elections

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

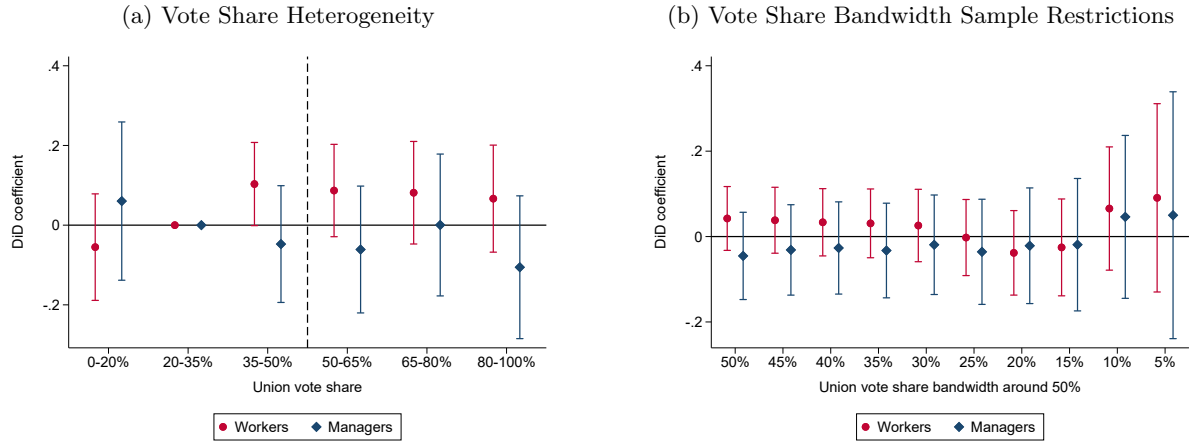


(b) Share of Won Union Elections per Week of the Year



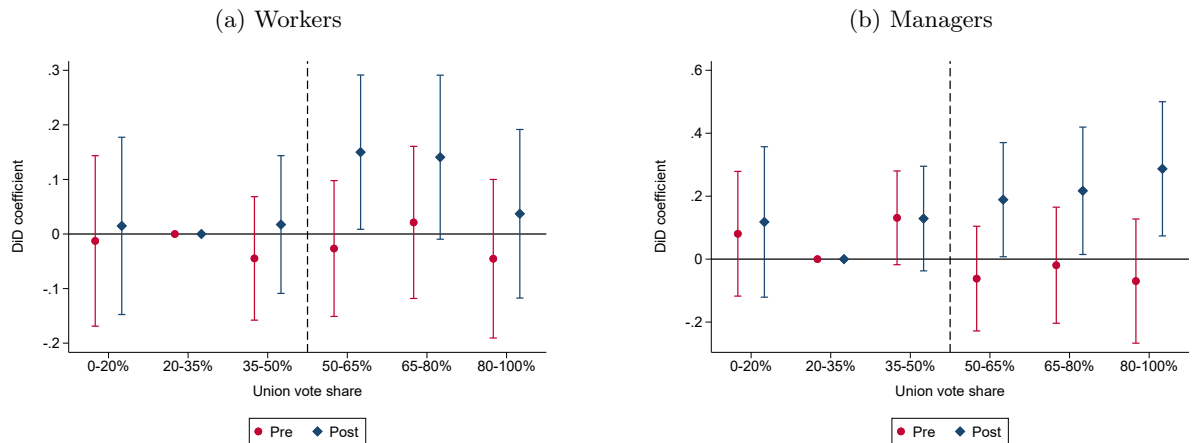
Notes: The graphs show the mean number of elections (Panel (a)) and mean share of won union elections (Panel (b)) per week of the year across all years in our period of analysis, i.e., between 1985 and 2010. The means are based on our matched estimation sample. We distinguish between years with and without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

Figure A.5: Effect of Unionization on Total Contribution Amounts - DiD-RDD Results



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 δ_g coefficients estimated in 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 from non-managerial workers (“workers”) and from managers and supervisors (“managers”). 95% confidence intervals are depicted for standard errors clustered at the establishment level.

Figure A.6: Effect of Unionization on Democratic versus Republican Support - Vote Share Heterogeneity in Pre- versus Post-Effects



Notes: The graphs report 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%. The outcome variable is the difference between the IHS-transformed amounts contributed to Democratic and Republican candidates. Results are shown separately for contributions from non-managerial workers (“workers”) and from 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	6,063	22,760
Union win (dummy)	.4397	.4405
Union vote share	.4950	.4955
Number of votes	119.37	81.92
Number of eligible voters	139.27	94.01
Industry: mining	.0397	.0388
Industry: manufacturing	.3338	.3731
Industry: transport	.1785	.1731
Industry: trade	.1397	.1251
Industry: finance	.1008	.0584
Industry: services	.1834	.2192
Years 1985-89	.1618	.2795
Years 1990-94	.1908	.2529
Years 1995-99	.2319	.2261
Years 2000-04	.2547	.1617
Years 2005-10	.1608	.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 the seven election cycles around the union election (three before, cycle of union election, three after).

Table A.2: Contributions of Union Organizations

Union organization	# of elections	% of contr. to Dem.	CF score
Teamsters Union	1605	91.0	-.655
United Steelworkers	481	98.0	-.770
United Food & Commercial Workers Union	434	97.7	-.800
Service Employees International Union	407	93.6	-.795
International Brotherhood of Electrical Workers	320	94.4	-.731
United Auto Workers	249	98.0	-.958
Machinists/Aerospace Workers Union	217	98.5	-.779
Operating Engineers Union	208	86.5	-.549
Communications Workers of America	170	95.8	-.761
UNITE HERE	136	94.0	-.706
Laborers Union	119	93.3	-.707
Carpenters & Joiners Union	110	89.6	-.650
American Federation of State/Cnty/Munic Employees	91	79.9	-.747
Office and Professional Employees International Union	51	99.3	-.816
Plumbers/Pipefitters Union	51	91.8	-.662
Amalgamated Transit Union	50	92.8	-.727
National Union of Hospital and Health Care Employees	47	96.7	-.567
Security, Police and Fire Professionals of America	44	100.0	-.793
International Longshore/Warehouse Union	43	94.2	-.920
Bakery, Confectionery, Tobacco & Grain Union	40	99.6	-.822
International Alliance Theatrical Stage Employees	40	95.0	-.742
American Nurses Association	38	83.7	-.561
Sheet Metal, Air, Rail & Transportation Union	35	92.9	-.635
United Mine Workers	33	92.2	-.640
Utility Workers Union of America	33	96.8	-.821
Transport Workers Union	27	94.1	-.663
Bridge, Structural, Ornamental and Reinforcing Iron Workers	26	94.0	-.719
Boilermakers Union	25	94.6	-.703
Painters & Allied Trades Union	25	89.1	-.714
United Electrical, Radio and Machine Workers of America	22	100.0	-1.115
American Federation of Teachers	19	96.3	-.748
Glass, Molders, Pottery, Plastics and Allied Workers	18	99.2	-.826
International Union of Journeymen and Allied Trades	18	96.8	-.698
Operative Plasterers and Cement Masons	17	91.2	-.703
Seafarers International Union	16	71.2	-.206
National Nurses United	15	98.3	-1.060
Roofers Union	14	92.7	-.765
International Guards Union of America	13	82.9	-.637
American Federation of Government Employees	12	95.9	-.791
SAG-AFTRA	9	100.0	-.933
American Postal Workers Union	9	96.5	-.735
International Union of Allied Novelty and Production Workers	8	-	-.524
Marine Engineers Beneficial Assn	7	85.2	-.606
International Association of Firefighters	6	84.2	-.504
American Federation of Musicians	6	91.8	-.562
Bricklayers Union	5	95.7	-.694
Insulators Union	4	94.3	-.815
Intl Fedn of Prof & Technical Engineers	2	87.4	-.824
International Longshoremens Assn	1	91.5	-.524
National Education Assn	1	86.3	-.519
Actors' Equity Assn	1	-	-.880
Total	5,378	93.5	-.726

Notes: The table reports characteristics of campaign contributions donated by union organizations in our sample of union elections. We consider all contributions from PACs associated with a union, including local union branches. ‘% of contr. to Dem.’ refers to the share of contributions going from a union to Democratic (as opposed to Republican) candidates. ‘CF score’ is the ideology score obtained from [Bonica \(2014\)](#) (when we match several PACs to one union organization, we average the ideology score of the different PACs, weighting each score by the number of donations). Totals in the last row give the weighted average over all union organizations, where the weights are the number of elections in our sample.

Table A.3: Robustness of Main Results

	\$ to all candidates			\$ to Dem. – \$ to Rep.		
	All	Workers	Managers	All	Workers	Managers
	(1)	(2)	(3)	(4)	(5)	(6)
[A]: Baseline						
δ_{DiD}	0.0332 (0.0794)	0.0259 (0.0433)	-0.0192 (0.0595)	0.239*** (0.0792)	0.123*** (0.0396)	0.204*** (0.0561)
[B]: Borusyak, Jaravel, and Spiess (2021)						
δ_{DiD}	0.0900 (0.0747)	0.0420 (0.0422)	0.00861 (0.0576)	0.236*** (0.0742)	0.130*** (0.0390)	0.183*** (0.0545)
[C]: Callaway and Sant’Anna (2021)						
δ_{DiD}	0.0152 (0.0827)	0.0416 (0.0444)	-0.0378 (0.0606)	0.243*** (0.0871)	0.137*** (0.0453)	0.135** (0.0619)
[D]: Log(Amount+1)						
δ_{DiD}	0.0273 (0.0727)	0.0236 (0.0393)	-0.0190 (0.0544)	0.220*** (0.0721)	0.111*** (0.0358)	0.186*** (0.0511)
[E]: Untransformed amounts						
δ_{DiD}	-27.62 (60.18)	2.414 (10.34)	-22.95 (33.02)	116.7*** (36.88)	15.58** (6.223)	65.38*** (20.13)
[F]: Only federal candidates						
δ_{DiD}	0.0476 (0.0751)	0.0257 (0.0390)	-0.0177 (0.0535)	0.207*** (0.0764)	0.0982*** (0.0364)	0.182*** (0.0519)
[G]: Only local candidates						
δ_{DiD}	-0.0472 (0.0500)	0.0241 (0.0285)	-0.0337 (0.0427)	0.158*** (0.0440)	0.0454* (0.0245)	0.130*** (0.0384)

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 amounts contributed to Democratic and Republican candidates (columns (4) - (6)). $N = 33,103$ establishment-cycle observations. Panel A shows the baseline results from the stacked DiD model (1) with IHS-transformed amounts. Panel 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.4: 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 th percentile of supervisor tasks)				
δ_{DiD}	0.0259 (0.0433)	-0.0192 (0.0595)	0.123*** (0.0396)	0.204*** (0.0561)
[B]: 90 th percentile of supervisor tasks				
δ_{DiD}	0.0430 (0.0458)	-0.0409 (0.0585)	0.140*** (0.0421)	0.201*** (0.0546)
[C]: Supervisor tasks “very important” (4 out of 5 in ranking)				
δ_{DiD}	0.0271 (0.0432)	-0.0218 (0.0597)	0.131*** (0.0394)	0.203*** (0.0561)
[D]: Non-managerial supervisors as workers				
δ_{DiD}	0.0400 (0.0481)	-0.0506 (0.0570)	0.163*** (0.0448)	0.183*** (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 amounts contributed to Democratic and Republican candidates (columns (3) and (4)). $N = 33,103$ 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 80th percentile of supervisor tasks and independent judgment. “Workers” are all remaining donors with a classified occupation. In Panel B, we increase the cutoff for supervisor tasks and independent judgment to the 90th percentile. Panel C, instead, uses an absolute cutoff for the importance of supervisor tasks and independent judgment (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 B.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.5: Effects of Losing a Union Election

	IHS(\$ to all candidates)			IHS(\$ to Dem.) – IHS(\$ to Rep).		
	All (1)	Workers (2)	Managers (3)	All (4)	Workers (5)	Managers (6)
[A]: Stacking						
δ_{DiD}	-0.0491 (0.0881)	-0.0263 (0.0396)	0.0705 (0.0529)	0.0568 (0.0966)	-0.0131 (0.0429)	0.0366 (0.0575)
N	31501	31501	31501	31501	31501	31501
[B]: Borusyak, Jaravel, and Spiess (2021)						
δ_{DiD}	-0.0481 (0.0901)	-0.0285 (0.0447)	0.0745 (0.0590)	0.0796 (0.100)	-0.00682 (0.0490)	0.0485 (0.0641)
N	16658	16658	16658	16658	16658	16658
[C]: Callaway and Sant’Anna (2021)						
δ_{DiD}	-0.0434 (0.0947)	-0.0381 (0.0469)	0.0615 (0.0637)	0.0761 (0.105)	-0.00688 (0.0515)	0.0534 (0.0698)
N	16658	16658	16658	16658	16658	16658

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-term effects of losing an election (i.e., 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 C](#) for details of the implementation. Standard errors clustered at the establishment level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Data Appendix

B.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 were originally compiled by [Farber \(2016\)](#). 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 cover 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 on 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 were 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.

B.2 Details on the Matching of Elections and Campaign Contributions

We link the campaign contributions from 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 2016.⁴⁰ We use these geocodes to match to them the 1990 commuting zones.

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.⁴¹ 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.⁴² In our review, we generally took a conservative approach and were more tolerant of possibly rejecting a true match than retaining an incorrect 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 B.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 6,063 matched establishments which form our final estimation sample. [Table A.1](#) compares the characteristics of

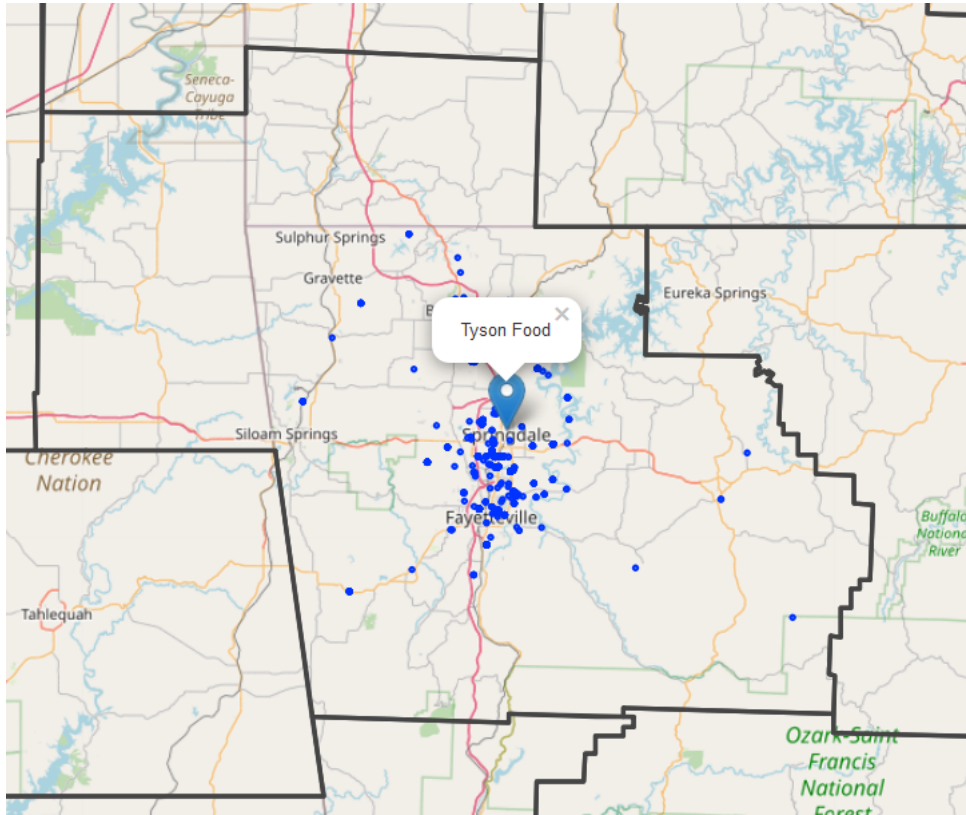
⁴⁰[Bonica \(2019\)](#) contains campaign contributions until 2018 but geocodes are only provided until 2016.

⁴¹`reclink2` builds on `reclink` written by [Blasnik \(2010\)](#).

⁴²The 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 B.1: Example of Spatial Matching Procedure



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

B.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 of 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 explicitly restricts the right to unionize 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, however, is 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: First, we harmonize occupations, and second, 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 a supervisor if *independent judgment* 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 is supported by the U.S. Department of Labor and 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 defined in the NLRA to identify occupations with *supervisor tasks*. The variables are listed in Table B.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 80th percentile of all 6-digit SOC occupations.⁴³ We then go on to evaluate whether the occupation requires *independent judgment*, the second condition that we identify in the NLRA definition of a supervisor. We evaluate whether an occupation requires *independent judgment* based on the following four variables: Independence (Work Styles), Leadership (Work Styles), Structured versus Unstructured Work (Work Context), and Freedom to Make Decisions (Work Context). Again, we classify an occupation as requiring *independent judgment* if the importance of at least one of the listed variables is equal or above the 80th percentile.⁴⁴ Finally, we classify individuals as managers or supervisors if their occupation is classified as “Management Occupation” in SOC (SOC group 11) or contains a *supervisor task* and *independent judgment* as defined above.⁴⁵ Examples of occupations in the top 95th percentile of both the *independent judgment* and *supervisor task* score are *Chief Executives*, *Human Resource Managers* and *First-Line Supervisors of Retail Sales Workers*. Non-managerial 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 non-managerial workers, and

⁴³In our robustness checks, we also use the 90th 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).

⁴⁴Again, in our robustness checks we also use the 90th percentile as the 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).

⁴⁵We 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 supervisors. 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 incorrectly coded as workers.

for 28% we are unable to obtain a classification.

Table B.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

C 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-term 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:

$$y_{ik} = \alpha_{ig} + \beta_{kg} + \delta_{\text{DiD}} \times \left(\mathbb{1}[k \geq 0] \times \mathbb{1}[g_i = g] \right) + \epsilon_{ik} \quad (7)$$

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 δ_{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.5.

In Panels B and C of Table A.5, 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.