Research Square

Preprints are preliminary reports that have not undergone peer review.
They should not be considered conclusive, used to inform clinical practice,
or referenced by the media as validated information.

Belief Updating and Collective Action Cascades in U.S. Labor Organizing

Jonne Kamphorst

jonne.kamphorst@sciencespo.fr

Sciences Po

Shirin Abrishami Kashani

Stanford University

Article

Keywords:

Posted Date: November 20th, 2026

DOI: https://doi.org/10.21203/rs.3.rs-11586805/v1

License: This work is licensed under a Creative Commons Attribution 4.0 International License.
Read Full License

Additional Declarations: There is **NO** Competing Interest.

Belief Updating and Collective Action Cascades in U.S. Labor Organizing

Shirin Abrishami Kashani[†] and Jonne Kamphorst[‡]

November 1, 2026

Abstract

Collective action often comes in waves, but it is hard to know whether waves reflect shared conditions or whether mobilization generates further mobilization. We use U.S. National Labor Relations Board representation elections, which record organizing attempts with discrete win–loss outcomes, to test whether success spreads. In a county-by-month difference-in-differences design, organizing at other workplaces rises after a successful election and stays elevated for five years, with effects that decay with distance. Lost elections also generate organizing, but less than victories: mobilization diffuses, and success adds a signal. A regression discontinuity on narrowly won and lost elections shows that success also spreads within employers, raising organizing at the winner's other worksites within a year. In a preregistered survey experiment (n = 1,091), news of recent organizing victories raises perceived feasibility of and support for unionization. Together, the results show that visible success amplifies collective-action cascades by changing what workers believe possible.

Keywords— collective action; diffusion; coordination; belief updating; labor organizing; unionization

[†]PhD Candidate, Stanford University. Email: skashani@stanford.edu

[‡]Assistant Professor, Sciences Po Paris. Email: jonne.kamphorst@sciencespo.fr

Collective action often turns on coordination under uncertainty. People may share grievances or preferences, yet still hesitate to participate when success depends on others' choices and when participation is risky. Classic work highlights why collective goods are underprovided when individuals can free ride or avoid costs, even when many would benefit (Olson 1971). Later theories formalize how participation can nonetheless shift abruptly: when individuals have thresholds for acting, small changes in expectations about others' participation can push a system past a tipping point, producing sudden waves of mobilization rather than smooth responses to slowly changing fundamentals (Granovetter 1978; Bikhchandani, Hirshleifer, and Welch 1992; Kuran 1991; Lohmann 1994; Marwell, Oliver, and Prahl 1988).

These perspectives motivate two broad explanations for collective action. One emphasizes common shocks: participation rises when the broader environment changes, be it through macroeconomic conditions, changes in institutional enforcement, or shifts in the information and coordination technologies that shape perceived risk and feasibility (Jenkins 1983; McAdam, McCarthy, and Zald 1996; Manacorda and Tesei 2020; Naidu 2022). A second emphasizes diffusion, in which observed success in one setting alters beliefs and expectations elsewhere. In this view, success can spread because it provides social proof, makes action appear feasible, and helps solve coordination problems (Kuran 1991; Lohmann 1994). Diffusion accounts also align with sociological work emphasizing that practices and repertoires travel along identifiable channels—through proximity, networks, and organizational ties—rather than simply emerging everywhere at once (Strang and Sarah A Soule 1998; Hedström, Sandell, and Stern 2000; Andrews and Biggs 2006).

Yet diffusion is much harder to test causally than common shocks. In observational data, the same clustering of behavior that looks like contagion—well documented for unionism, strike waves, and protest tactics (Hedström 1994; Conell and Cohn 1995; Biggs 2005; Andrews and Biggs 2006; D. J. Wang and Sarah A. Soule 2012)—can arise because individuals share environments or sort into groups with correlated unobservables (Manski 1993).

To our knowledge, the causal evidence on success-breeds-success dynamics in collective action comes predominantly from field experiments on internet platforms, which show that seeding new campaigns with initial support—signatures on petitions, donations to crowdfunding projects, awards to contributors—leads them to attract more support thereafter (Rijt, Kang, et al. 2014; Rijt, Akin, et al. 2016; Gonzalez Vaillant et al. 2016; Margetts et al. 2011), echoing the self-reinforcing popularity documented in online cultural markets (Salganik, Dodds, and Watts 2006). In these studies the support that accumulates accrues to the same campaign, and participating means clicking through a platform rather than acting against an employer or a

state. Separately, causal work on protest establishes that mobilization spreads between actors where participation is costly: exogenously larger Tea Party rallies raised subsequent mobilization (Madestam et al. 2013), students drew their peers into Chile's student movement (González 2020), and experimentally induced attendance at one Hong Kong protest raised attendance at later ones (Bursztyn et al. 2021). But in these designs the treatment is that action occurred, not that it achieved anything, even though a drive that fails and one that wins carry different information about what is achievable. Whether one group's victory leads others to mount campaigns of their own—the dynamic at the heart of cascade accounts—remains untested where acting is costly.

Public discourse around recent U.S. labor organizing makes this distinction vivid. Commentators described 2023 as a 'hot labor summer,' marked by highly visible organizing and strike activity. Structural accounts emphasize shared conditions—tight labor markets, post-pandemic disruptions, changes in enforcement and procedure, and cost-of-living pressures—producing parallel increases in labor conflict across many workplaces. Diffusion accounts instead argue that visible victories can be self-reinforcing: success reduces perceived risk, provides a focal point for coordination, and encourages subsequent attempts. Multi-site employers such as Starbucks became emblematic of this debate, as organizing appeared to propagate across worksites in ways that looked contagious. At the same time, these observational patterns are inherently ambiguous: the same clustering of activity could reflect shared macro conditions, campaign targeting, or endogenous transmission.

We leverage the institutional structure of U.S. labor law to provide rare leverage on success-driven diffusion. Compared to protest waves and revolutionary episodes, representation petitions and elections administered by the National Labor Relations Board generate discrete, time-stamped events with standardized outcomes, creating an unusually consistent administrative record of both organizing attempts and clear 'success.' This setting is also a stringent test for diffusion. Union drives are costly and adversarial; if success-driven cascades can be identified here, they plausibly reflect meaningful belief updating and coordination dynamics rather than low-cost imitation (McAdam 1986; Farber 2015). Related work has long suggested that unionism exhibits geographic persistence and spillovers, underscoring the plausibility of transmission mechanisms in this domain (Hedström 1994; Holmes 2006). Meanwhile, the recent rise in representation petitions highlights the contemporary relevance of understanding how organizing activity propagates (Naidu 2022).

We study diffusion in labor organizing using NLRB representation elections in a county-by-month panel. We treat a successful unionization election in a county as an exposure event

and estimate how organizing activity evolves over the five years that follow. To address the key inferential challenge—separating success-driven diffusion from time-varying common shocks and unobserved differences between early- and late-mobilizing places—we use a counterfactual difference-in-differences design with fixed effects that flexibly absorbs time-varying unobservables while estimating dynamic event-time effects.

We find evidence consistent with success-driven cascades. After a successful unionization election, organizing activity in the county rises. Elections that are lost also generate spillovers, consistent with organizing attempts being informative in themselves, but the effect of a win is substantially larger. Spillovers further exhibit spatial structure: effects attenuate with geographic distance, consistent with transmission through local informational and social pathways rather than a purely national trend—an empirical pattern that resonates with diffusion frameworks emphasizing channels and with network accounts in which reinforcement and local clustering matter for high-cost behaviors (Strang and Sarah A Soule 1998; Centola and Macy 2007). Success also spreads within organizations. Using the majority rule in NLRB elections as a regression discontinuity, we compare employers whose worksite narrowly won an election with employers whose worksite narrowly lost, and find that unionization attempts at the company's other worksites become more likely after a win. Because the outcome of a narrowly decided election is as good as random, this design rests on weaker assumptions than the county-level panel, and its agreement with the panel estimates strengthens the causal interpretation of the cascade.

Finally, we probe mechanisms linking local success to subsequent attempts. One possibility is diffusion through beliefs and coordination: visible success increases perceived feasibility, lowers perceived risk, and provides scripts for action, thereby enabling organizing in other workplaces. Studies investigating what influences support for unions have highlighted that perceptions of how many other workers support unionization are a crucial driver of support for union drives (DeCotiis and LeLouarn 1981; Youngblood et al. 1984). Moreover, seeing examples of union activity has been shown to influence perceptions of unions (Hertel-Fernandez, Naidu, and Reich 2021). Another possibility is diffusion mediated primarily by organizational capacity, if unions or organizers redeploy effort toward settings revealed to be favorable.

We assess these accounts using complementary evidence. First, we examine whether spillovers are confined to the initiating organization by focusing on elections involving the International Brotherhood of Teamsters. Following successful Teamsters campaigns, subsequent organizing increases both within the Teamsters and among other unions, a pattern that is difficult to reconcile with spillovers driven only by organization-specific resource retention (while leaving room

for broader organizer responses that affect the local organizing environment). Second, in a pre-registered survey experiment (n = 1,091), exposure to recent organizing successes in the form of reading news articles increases perceived feasibility and support for unionization, consistent with belief updating as a micro-level mechanism connecting observed success to subsequent attempts.

Together, these findings contribute to the study of collective action by providing causal evidence that one group's victory generates new attempts by other actors, in a setting where attempting is costly and contested. By combining a standardized administrative record of attempts and outcomes with a design that flexibly accounts for time-varying unobservables—and by tracing transmission through both geographic proximity and organizational ties—we show how collective action can propagate endogenously when success changes what people believe is possible and helps solve coordination problems in other settings. Mobilization is therefore path-dependent in a specific sense: which places organize depends not only on where conditions are favorable, but on where someone has already won.

The findings also contribute to research on American labor, at a moment when the prospects for union revitalization have returned to the center of debate (Farber et al. 2021; Naidu 2022). They offer an account of episodes like the hot labor summer in which structural conditions and diffusion are complements rather than rivals: favorable conditions may spark initial victories, but success itself then regenerates organizing, locally and through organizational networks, implying that each win carries positive externalities for subsequent drives. And they identify the beliefs through which this transmission operates: news of organizing success shifts precisely the perceptions of feasibility and co-worker support that underpin interest in unionizing—most strongly among Republican workers with the least favorable priors—suggesting that visible victories can broaden the coalition supporting unionization beyond workers already predisposed to it.

Results

Geographic spread of unionization attempts

We first focus on Starbucks elections. The top panel in Figure 1 shows that after the first Starbucks store unionized in Buffalo, New York, many other stores across the country followed in subsequent months. While consistent with the prevailing media narrative, these results are descriptive and do not support a causal interpretation. To assess whether successful Starbucks campaigns generated broader spillovers, we examine whether unionization activity increased in other firms within the same county after a first successful Starbucks election, following a design similar to Schaller, McCollum, and Shakya (2026). Using a county-by-month panel covering

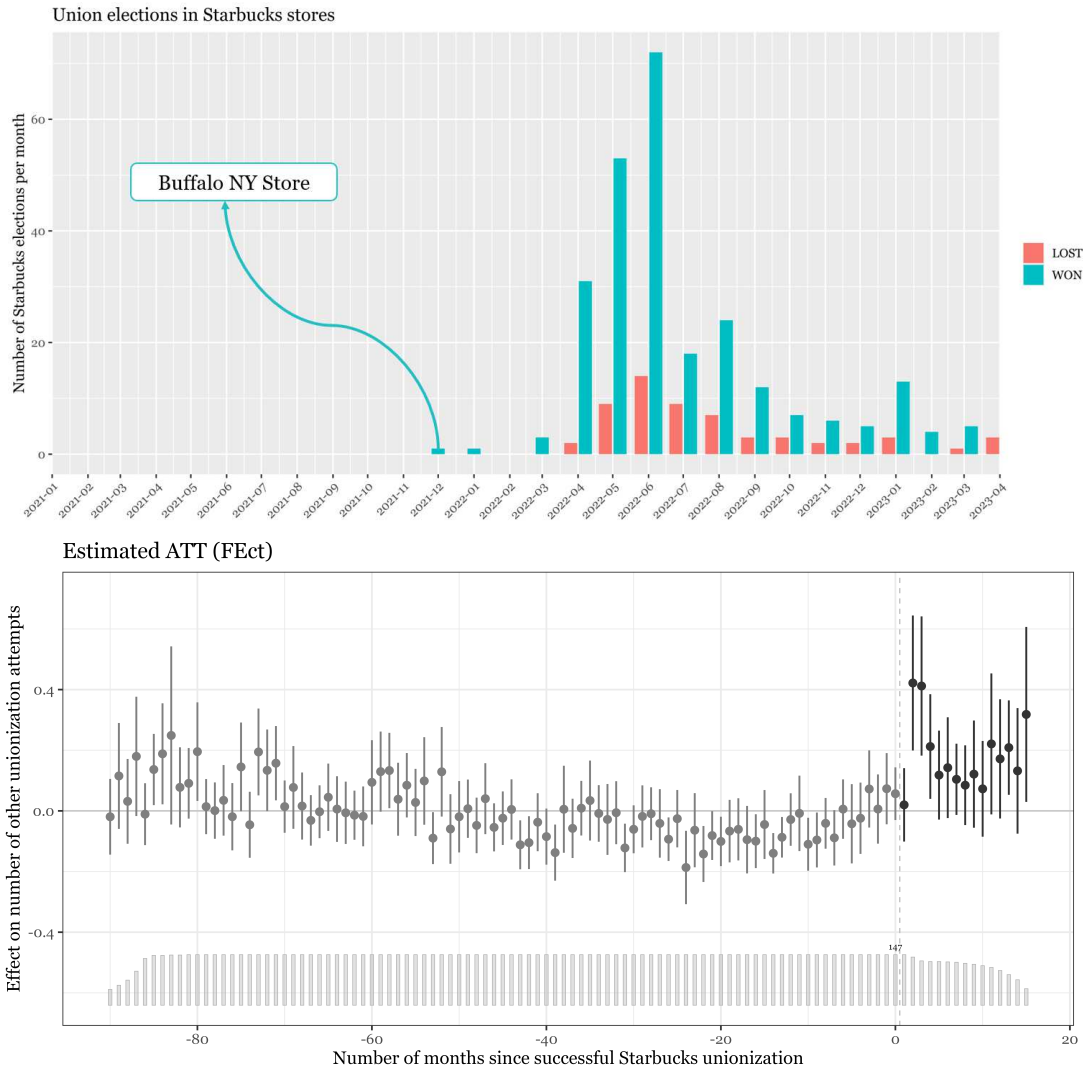
2015–2023, we estimate a staggered-adoption difference-in-differences design implemented via a fixed-effects counterfactual estimator (Liu, Y. Wang, and Xu 2024), which compares treated county-months to an imputed no-treatment trajectory constructed from untreated observations. The results are shown in the bottom panel of Figure 1. The fixed-effects counterfactual estimator yields an average post-treatment effect of 0.18 additional unionization elections per county-month after the first successful Starbucks election in that county (ATT = 0.182, 95% CI [0.109, 0.255], p<0.001), relative to an imputed no-treatment counterfactual. Substantively, this estimate implies a sustained increase in organizing activity following an initial successful Starbucks campaign. However, while the pre-trends visually seem to be centered around zero, a formal test shows the parallel trends assumption fails (SI Figure A2). This seems to be the case because treated counties enter treatment within a narrow window beginning with Buffalo in December 2021, and as a result they share essentially the same pre-treatment period—one dominated by the large pandemic-era collapse and subsequent rebound in NLRB elections.

Next, we examine whether the observed spillovers are specific to Starbucks or arise following successful unionization drives more generally. For this analysis, we use all unionization elections in the United States since 2000 and apply the same fixed-effects counterfactual estimator. Beyond supporting more general conclusions, this analysis addresses the lack of parallel trends in the Starbucks-only design. In the more general analysis, treatment events are instead spread across two decades. Counties are coded as treated in the month of a successful unionization election and for the five years that follow; further successes within that window count as outcomes and do not extend it, and a success after the window opens a new treatment period. The top panel in Figure 2 illustrates treatment history.

The results of this analysis are reported in the bottom panel of Figure 2, and Figure A1 shows parallel trends hold. We again find a positive and precisely estimated effect: the estimator yields an average post-treatment effect of 0.111 additional unionization elections per county-month following a successful drive (ATT = 0.111, 95% CI [0.089, 0.133], $p < 0.001$), relative to an imputed no-treatment counterfactual. Over the five years after treatment, this amounts to 5.54 additional unionization attempts in the same county.

Three additional patterns support a diffusion interpretation of these estimates. First, the estimated impact is small in the first month immediately following treatment. This temporal pattern is consistent with institutional constraints: a petition must first be filed with the NLRB before a vote can be held, implying a natural delay between an initial successful drive and subsequent organizing activity. Second, the design so far bundles two things: a victory occurred, and a highly visible election occurred at all. To assess whether success does work beyond

Figure 1: The effect of successful Starbucks unionization drives

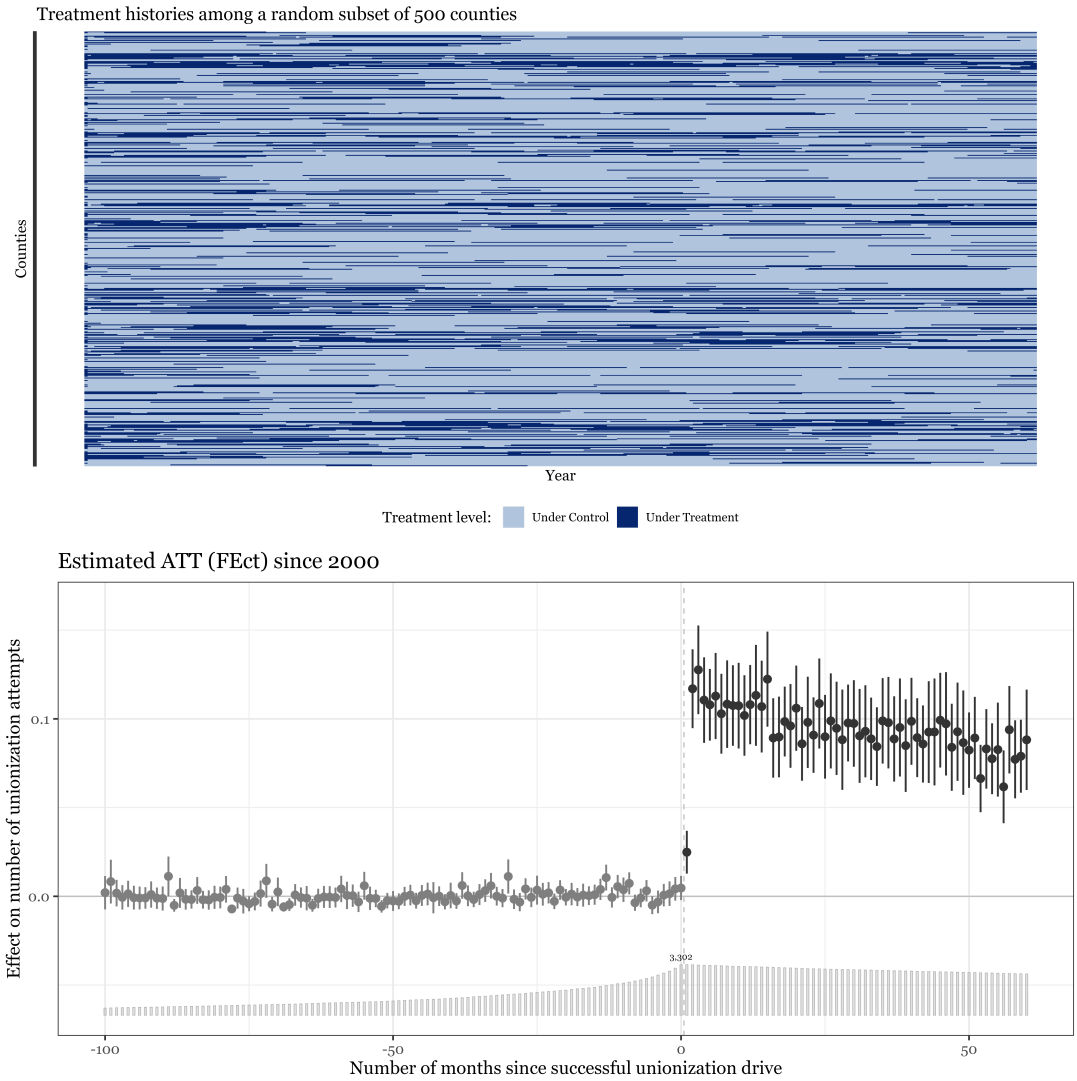


Note: The top panel shows the timing of the first successful Starbucks unionization in Buffalo, New York, and subsequent Starbucks organizing activity nationwide. The bottom panel reports average treatment effects from a fixed-effects counterfactual difference-in-differences estimator with county and month effects. Treatment (D) is an indicator that switches to one in the month of a county's first successful Starbucks election and remains one thereafter. The outcome (Y) is the number of unionization attempts reported in that county-month. Bars indicate 95% confidence intervals.

the attempt, we re-estimate the same model coding treatment as an unsuccessful unionization election, remaining active for five years. Losses also generate positive spillovers (ATT = 0.077, 95% CI [0.063, 0.090], $p<0.001$), consistent with elections themselves being salient, informative events—an unsuccessful drive still reveals that many co-workers wanted a union (see Figure A4). The effect is about a third smaller than the 0.111 for a win, and each estimate lies outside the other's confidence interval. Success, in other words, carries information beyond the attempt: it is what shifts beliefs about feasibility, not merely about latent support.

Third, spillovers are geographically localized. We measure spatial exposure as the average number of successful union elections per neighboring county over the previous five years (exclud-

Figure 2: Effect of successful unionization drives on subsequent organizing activity

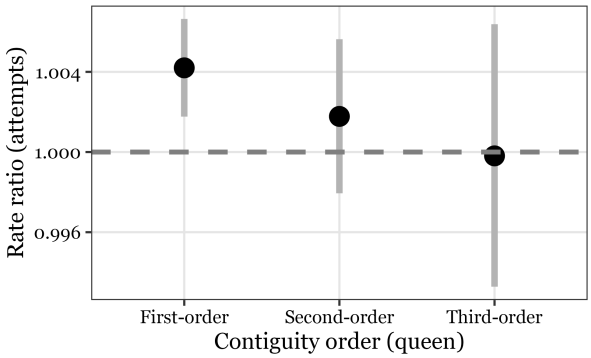


Note: The top panel visualizes treatment histories for a random subset of 500 US counties in the county-by-month panel. Treatment is an indicator that switches to one in the month of a successful unionization election and remains one for five years thereafter. The bottom panel reports estimates from a fixed-effects counterfactual difference-in-differences model. Treatment is defined as a successful unionization election in a US county (active for five years following the event). Figure A1 in the Online SI confirms trends are parallel pre-treatment. Lines indicate 95% confidence intervals.

ing the current month) and estimate a county-by-month Poisson model with county and month fixed effects. When we decompose exposure by contiguity order, the estimated association is strongest for first-order neighbors and attenuates with distance, approaching zero by third-order neighbors (Figure 3). Importantly, exposure accumulates over multiple nearby successes: counties experiencing several recent successes in adjacent counties exhibit meaningfully higher expected organizing activity than otherwise similar counties with no recent neighboring successes. This tapering pattern complements the event-time estimates by showing that spillovers are not spatially diffuse, consistent with transmission through local social and informational channels rather than a uniform national shock.

Taken together, the combination of a delayed onset and spatial decay is consistent with a geographically diffusing process of union organizing activity.

Figure 3: Spillovers taper with distance (5-year lag window)



Note: This figure plots rate ratios from a county-by-month Poisson model with county and month fixed effects. The predictors are indicators of recent successful union elections in contiguous counties, split by contiguity order (first-, second-, and third-order; queen contiguity). Each exposure is defined as the number of neighbor successes in the previous five years (months $t-1$ to $t-60$), excluding the current month. A ratio of 1 means that successes in neighboring counties leave the expected number of attempts unchanged, and a ratio of 1.004 means that one additional successful election in the average neighboring county over the previous five years raises the expected number of attempts in a county-month by 0.4 percent. Points show estimated rate ratios; lines indicate 95% confidence intervals.

Spread of unionization within companies

The county-level estimates capture spillovers between employers in the same place, but they share a limitation with any event-study design in this setting: An election, won or lost, is itself an organizing event, and the conditions that produce it - a regional campaign, organizers on the ground, a local labor-market shock - may also produce the elections that follow it. Flat pre-trends cannot rule this out, because such a shock arrives together with the election. Comparing elections that were won with elections that were lost narrows the question to the contribution of success, but it cannot settle it, since the places and employers where unions win may differ from those where they lose. Multi-site employers such as Starbucks point to a second way in which unionization may spread, in which a victory at one worksite leads workers at the same employer's other worksites to organize, wherever those worksites are, and this setting also allows a design that addresses these concerns. Namely using the majority rule in NLRB elections as a regression discontinuity. A union is certified when it obtains a majority of the valid votes cast, so among elections decided by a handful of votes, whether the union won is as good as random (DiNardo and Lee 2004; Lee and Mas 2012): employers whose worksite narrowly won and employers whose worksite narrowly lost faced the same local conditions and the same organizer interest, and differ only in the outcome of the vote. Any difference in subsequent organizing between

them is therefore an effect of success itself.

The outcome is whether a petition for a representation election was filed at another worksite of the same employer in the twelve months after the election. Because the NLRB identifies employers by free-text names, we link elections to employers in two ways: through an exact match on standardized names (shown in the SI) and through clusters of names within a small string distance of one another. Following earlier work that uses this discontinuity, we restrict the analysis to elections in which twenty or more votes were cast (DiNardo and Lee 2004; Sojourner et al. 2015). In smaller elections the vote margin bunches just above the threshold, which rules out a local comparison, whereas among larger elections its distribution is smooth through the threshold (SI Figure A12).

The results are reported in Figure 4, which shows the probability that a new petition was filed at another worksite of the same employer within a year against the vote margin. A narrow win raises it by 5.3 percentage points (RD estimate = 0.053, 95% CI [0.012, 0.107], $p = 0.014$), against a base rate of 15.9 percent. A narrow win thus raises organizing at the employer's other worksites by roughly 30 percent.

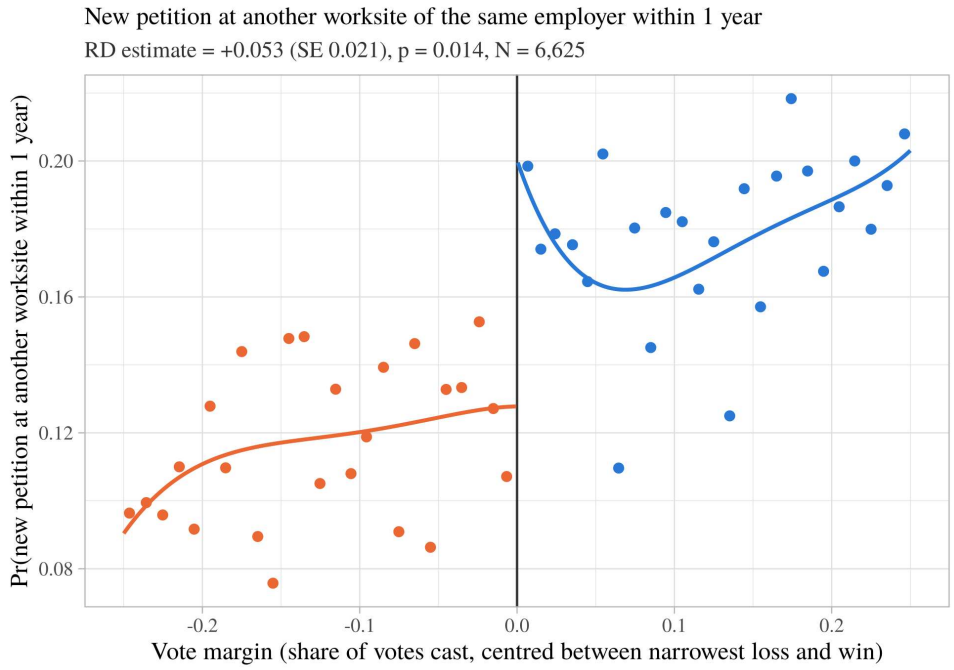
The results hold under alternative specifications (SI Figure A15), do not depend on the bandwidth chosen (SI Figure A13), do not appear on placebo outcomes and still hold when grouping companies using exact names (SI Figure A11), and there are no significant discontinuities among covariates of elections at the threshold (SI Figure A16). The effect is concentrated in the first year after the election: when the outcome window is extended to two and three years, the estimates are no longer distinguishable from zero (SI Figure A14). The within-employer estimates are less precise than the county-level ones, but they rest on a comparison that is as good as random and they point the same way: success spreads through organizational ties as well as through geographic proximity.

Mechanisms underpinning these patterns

In this section, we examine potential mechanisms underlying the effects documented above. As noted in the introduction, one possibility is that unions learn how to organize more effectively within specific geographic areas or retain organizing resources (e.g., salts) locally after a successful drive. An alternative mechanism operates through workers: successful campaigns may update workers' beliefs about the feasibility and local support for unionization, increasing perceived viability and lowering informational barriers to organizing. To explore these mechanisms, we report three analyses.

First, to examine the union-resources mechanism, we return to the analysis reported in Figure

Figure 4: Effect of a narrow union victory on organizing at other worksites of the same employer



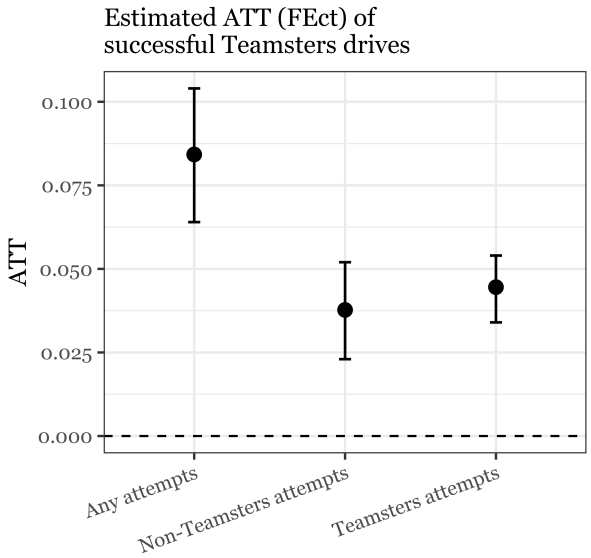
Note: Regression discontinuity estimate at the majority threshold of NLRB representation elections (n = 19,785; see Materials and Methods). The running variable is the vote margin as a share of votes cast, centred between the narrowest possible loss and win. The outcome is the probability that a petition for a representation election was filed at another worksite of the same employer within one year, with employers identified by string-distance clusters of their names. Dots are bin means (25 evenly spaced bins per side) and lines are fourth-order polynomial fits. The estimate is a local linear regression with a triangular kernel and MSE-optimal bandwidth, estimated with `rdrobust` (Calonico, Cattaneo, and Titiunik 2014; Calonico, Cattaneo, Farrell, et al. 2017); N is the number of elections within the bandwidth and the p-value is robust bias-corrected.

2 and redefine treatment to include only successful unionization drives led by the Teamsters, the most common union in our dataset. Using the same fixed-effects counterfactual estimator, we estimate the effect of a successful Teamsters drive on subsequent unionization activity within the same county. We decompose the outcome into (i) additional Teamsters drives and (ii) non-Teamsters drives. If the primary mechanism operates through union-specific resource retention (e.g., organizers or salts remaining locally), we would expect effects to be concentrated among subsequent Teamsters campaigns, with little to no spillover to other unions.

These results are reported in Figure 5. A successful Teamsters drive increases subsequent Teamsters organizing by 0.045 additional elections per county-month (ATT = 0.0445, 95% CI [0.0359, 0.0551], $p < 0.001$). At the same time, we observe a similarly sized and statistically significant increase in non-Teamsters organizing activity (ATT = 0.0377, 95% CI [0.0235, 0.0520], $p < 0.001$). The difference between the two effects is not statistically distinguishable. The cross-union spillover is difficult to explain through Teamsters-specific resource retention alone, though it is consistent with two channels our administrative data cannot fully separate: workers updating their beliefs after a visible local win, and other unions strategically redirecting effort

toward areas revealed to be favorable. The survey experiment below speaks directly to the first channel.

Figure 5: The effect of successful Teamsters unionization attempts on other attempts



Note: The figure reports estimates from a fixed-effects counterfactual difference-in-differences model. Treatment is defined as a successful Teamsters unionization election in a US county. The outcomes are illustrated on the x-axis and are any other unionization attempts, attempts by other unions than teamsters, or attempts by teamsters.

Second, we explore the possibility that workers learn about successful unionization drives. To do so, we conduct a media analysis to examine whether successful Starbucks unionization drives increase local media attention to unionization. We code a county as treated for the six months following a successful Starbucks election. As the outcome, we draw on the 3DLNews2 archive (Ariyarathne and Nwala 2024), which contains nearly one million URLs and associated HTML text from more than 14,000 local newspapers, television, and radio stations across the United States. The dataset was constructed by scraping Google and Twitter search results and subsequently cleaned and geo-coded to identify local news sources. We measure the monthly number of local news articles in the same county that mention "unionize" or "unionized." The fixed-effects counterfactual estimator yields an average increase of 0.050 union-related articles per month in that six-month post-treatment window (ATT = 0.050, 95% CI [0.012, 0.088], p = 0.010). This pattern is consistent with heightened local media salience following a successful organizing campaign, which could explain how workers may learn from unionization drives. However, pre-treatment estimates are centered on zero yet imprecise and fail a formal test for parallel trends (see Figure A3), so we treat this analysis as suggestive.

For the third analysis that examines mechanisms underlying the spread of successful unionization drives, we field a survey experiment designed to measure whether exposure to news

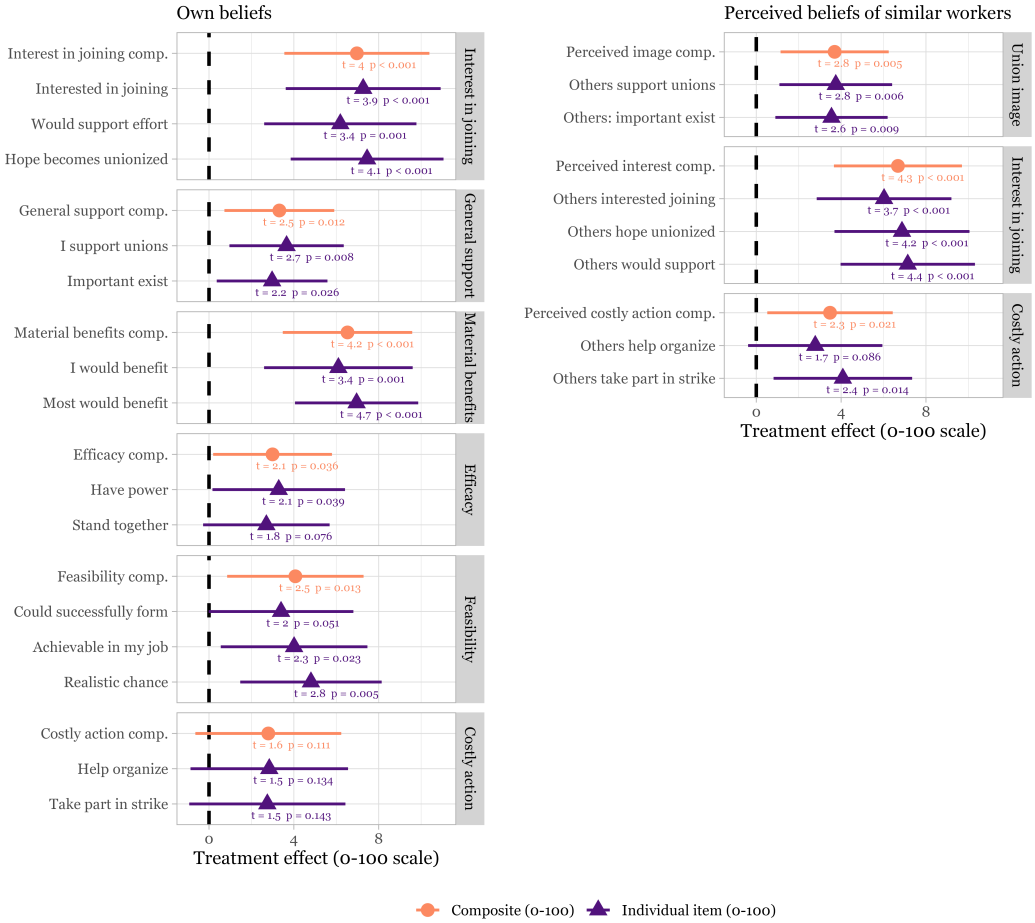
articles about recent organizing success shifts workers' beliefs and support for unions. We recruit a stratified sample of working US adults with quotas on partisanship, race, and age (n = 1,091), excluding respondents who report not currently being employed and managers, who are not eligible to join unions per the National Labor Relations Act. Participants report their occupation, which we classify into eight ISCO occupational categories. In the treatment condition, respondents read two recent, real news articles describing successful unionization drives within their own occupational category; articles are lightly edited to ensure comparable length and structure. In the control condition, respondents either proceed directly to the outcome measures or read a placebo article of similar length about unrelated content (there is no difference between these control conditions).

To measure the effects of the articles, we draw on a range of outcomes from the literature on what underpins support for unions and interest in joining them. We measure workers' own beliefs and intentions: whether they are interested in joining a union, support unions, think unions provide material benefits (Kamphorst and Willer 2024), believe workers like them have the efficacy to improve their own conditions, consider unionizing feasible in their job (Youngblood et al. 1984), and are willing to take costly actions in favor of a union (e.g., striking or helping organize). Alongside these, we ask whether they think others in a job such as theirs support unions, are interested in joining them, and are willing to take costly actions (Youngblood et al. 1984). We combine outcomes that capture the same construct into composite indexes by taking their average, as pre-registered.

The results of the survey experiment are reported in Figure 6. Respondents who read about recent unionization successes in their own field reported significantly greater interest in joining a union ($\beta = 6.97$, $t = 4.0$, $P < 0.001$), greater general support for unions ($\beta = 3.32$, $t = 2.5$, $P = 0.012$), a stronger belief that unionization would deliver material benefits ($\beta = 6.53$, $t = 4.2$, $P < 0.001$), greater perceived feasibility of unionizing in their workplace ($\beta = 4.07$, $t = 2.5$, $P = 0.013$), and a stronger belief in the efficacy of collective action ($\beta = 3.00$, $t = 2.1$, $P = 0.036$), as compared to the control group. The treatment did not significantly shift willingness to take costly action in support of a union ($\beta = 2.80$, $t = 1.6$, $P = 0.111$). These effects correspond to a 13.4% greater interest in joining, a 4.5% greater general support for unions, a 10.8% greater belief in material benefits, an 8.1% greater perceived feasibility, and a 4.7% greater belief in efficacy, as compared to the mean levels in the control group.

The treatment also shifted what respondents believed about the workers around them. Those in the treatment condition perceived a more favorable image of unions among workers in their own field ($\beta = 3.69$, $t = 2.8$, $P = 0.005$), greater interest in joining a union among those workers

Figure 6: The effect of reading about successful unionization attempts



Note: Treatment effects from the survey experiment. 95% confidence intervals are shown. Full regression models can be found in Table A4.

($\beta = 6.68$, $t = 4.3$, $P < 0.001$), and greater willingness among them to take costly action in support of a union ($\beta = 3.48$, $t = 2.3$, $P = 0.021$), as compared to the control group. These effects correspond to a 5.6% more favorable perceived union image, a 12.9% greater perceived interest in joining, and a 7.8% greater perceived willingness to take costly action, as compared to the mean levels in the control group.

Pre-registered heterogeneity analyses indicate that these effects are predominantly driven by Republican respondents. Across both their own beliefs and their perceptions of other workers, Republicans in the treatment condition updated substantially, whereas treatment effects among Democratic respondents were smaller and generally not statistically distinguishable from zero, apart from interest in joining and perceived material benefits (Figure A17). This pattern is consistent with the belief-updating account: Democratic respondents already hold comparatively favorable beliefs about unions, leaving limited room to update, whereas news of successful organizing in one's own field is most informative for those with less favorable priors. It also

suggests that visible organizing successes may broaden the coalition supporting unionization beyond workers already predisposed to it, rather than merely reinforcing existing support.

Discussion

Our findings suggest that successful collective action can generate further collective action. Across U.S. union elections, organizing activity increases following successful local drives, remains elevated for several years, and is geographically concentrated rather than uniformly distributed. Within employers, a regression discontinuity design shows that a narrowly won election raises organizing at the employer's other worksites in the following year, where a narrowly lost one does not. These patterns are difficult to reconcile with a purely national shock. Instead, they are consistent with a diffusion process in which local success causes further action. The survey experiment provides complementary evidence for such a mechanism: exposure to successful organizing increases perceived feasibility, expected material benefits, and interest in joining a union. Taken together, the results suggest that collective-action waves can be partly self-reinforcing, with successful episodes becoming inputs into subsequent mobilization.

This interpretation connects to classic theories of collective action in which participation depends on expectations about what others will do and whether coordinated action can succeed. Threshold and informational-cascade accounts explain how relatively small changes in beliefs can produce abrupt shifts in participation. Our findings suggest that the realized outcomes of earlier collective action may provide one source of such belief change. Observing others act reveals willingness to participate; observing them succeed additionally provides information about feasibility, likely returns, and the degree of support that collective action can command. This distinction matters because it creates scope for path dependence. Favorable structural conditions may generate an initial organizing victory, but that victory can then increase the probability of further attempts, amplifying the original shock and producing a larger wave than underlying conditions alone would imply.

The mechanism evidence is consistent with this account, while also placing limits on it. Successful Teamsters campaigns are followed by increased organizing by both the Teamsters and other unions, suggesting that the pattern is not easily explained solely by organization-specific resources being redeployed locally. In the experiment, exposure to successful campaigns shifts beliefs about feasibility, material benefits, and the attitudes of similar workers, but does not clearly increase willingness to undertake costly pro-union action itself. This pattern is consistent with a staged process in which success first changes perceptions of what is possible and socially supported, thereby creating conditions under which subsequent organizing becomes

more likely. The larger experimental effects among Republican respondents are also consistent with a belief-updating interpretation: information about successful organizing appears most consequential among workers whose prior views leave greater scope for updating.

Several limitations qualify these conclusions. We find geographic spillover effects of successful union drives, but these results do not directly reveal the interpersonal, workplace, or organizational networks through which information travels. While the local media analysis provides suggestive evidence that successful campaigns increase information exposure, the parallel trends assumption is unlikely to hold. In all, the analysis presented here cannot determine how much of the observed diffusion operates through local media rather than through interpersonal communication, workplace networks, or union organizations. Future research could more directly identify the causal effect of exposure to successful collective action on outcomes such as these.

Despite these limitations, our findings also help clarify how structural and diffusion-based accounts of recent U.S. labor mobilization can coexist. Conditions such as labor-market tightness, changes in enforcement, and post-pandemic disruption may have increased the likelihood of initial organizing successes. If those early successes encourage others to organize, favorable conditions can trigger a much larger wave of collective action. Episodes such as the recent "hot labor summer" may therefore reflect neither a common shock nor contagion alone, but an interaction between the two. More broadly, understanding waves of collective action requires attention not only to the conditions that make initial mobilization possible, but also to the informational consequences of what happens once people act.

Material and Methods

Geographic spillover of unionization

Data on unionization attempts come from the administrative records of the National Labor Relations Board (NLRB), the agency that administers union certification elections in the U.S. private sector. We collect these records through a series of Freedom of Information Act requests to the NLRB and by scraping the NLRB's public case records and election reports, and supplement them with digitized historical files from the National Archives and earlier compilations of NLRB election data. Because the agency has repeatedly changed its case-handling system—the Case Activity Tracking System (CATS) covers cases closed between fiscal years 2000 and 2010, and the NxGen system has been used since—the released files differ in structure and in the fields they contain, and we harmonize them into a single election-level dataset covering 1961 through

2023. In the analysis presented here, we focus on elections after 2000 because the data quality is higher. The harmonized data, along with full documentation, are published separately (Schaller, Young, and Kamphorst 2026).

The location information the NLRB releases is uneven across eras, so we geocode each election on the most precise address the agency released for it. In recent decades this is usually the street address of the bargaining unit or polling location; in earlier years the records contain only a ZIP code, or just a city and state. We retain a flag for the precision of the resulting match: an exact street-level match, an approximate street-level match, or a city centroid when no address was available. Among elections held since 2000, 75.3% are matched at the street level, compared with 19.2% before 2000, where city centroids predominate (78.7%). Each election is assigned to a county (2020 vintage FIPS codes). The county-by-month panels use only this county assignment; the point coordinates enter only the distance-based spillover models, which require within-county location.

We treat the individual bargaining unit, rather than the NLRB case, as the unit of observation. A single case can cover several units at the same employer—for instance office staff and production workers—and those units vote separately and can vote differently, so we identify elections by the combination of case number and unit identifier and count each unit's vote as a distinct election. Units appear separately only where the NLRB reported a separate tally for each; where the agency aggregated the votes of several groups of workers, the record contains a single unit and we treat it as a single election. This distinction matters for little of the data: only 2.7% of elections since 2000 cover more than one unit.

We keep representation petitions filed by employees (RC) and by employers (RM) and drop decertification petitions (RD), so that observations are elections over establishing representation rather than removing an existing union. An election is coded as won where the NLRB recorded the case as closing with a certification of representative and, for the earlier records in which no closing reason is reported, where votes for the union exceeded votes against. The resulting data contain 41,177 elections held between 2000 and 2023 in 1,916 counties, of which 26,168 were won by the union.

For the analyses we aggregate these elections into a county-by-month panel, counting in each county-month the number of elections held (our measure of organizing attempts) and the number won (our measure of success), with both set to zero in county-months without elections. The county-level analyses are restricted to 2000 onward, leaving 41,177 elections in 1,916 counties, 26,168 of which were successful; the Starbucks analyses cover 2015–2023 and identify the 440 Starbucks elections in that window from the employer name. Wherever treatment is defined by

a given employer's success, we exclude that employer's own elections from the outcome, so the estimates reflect organizing at other workplaces.

Robustness checks for the DiD

First, we present the results from parallel-trends diagnostics implemented in the `R fect` package (Liu, Y. Wang, and Xu 2024). The equivalence test assesses whether estimated pre-treatment prediction errors are sufficiently small to be considered substantively equivalent to zero. The equivalence range is the cutoff for what counts as a negligible pre-trend, while the minimum range shows how precisely the data can establish that pre-trends are small. When the minimum range falls within the equivalence range, the observed pre-treatment deviations are small enough to pass the equivalence test. Unlike a conventional significance test, the null hypothesis is that at least one pre-treatment deviation falls outside the equivalence range. Simply put, a lower p-value on the equivalence test provides stronger support for the parallel trends assumption, with a p-value below the conventional significance threshold indicating that the equivalence test is passed. For the effect of any successful unionization election on subsequent organizing since 2000 (Figure 2), the equivalence test yields $p = 0.002$ (Figure A1). The Starbucks analysis does not pass ($p = 0.977$; Figure A2). The pre-treatment path is not flat: estimates are significantly positive roughly six to seven years before treatment and significantly negative in the two years immediately preceding it, the largest deviation being about -0.14 at fourteen months before treatment. Because treatment is concentrated in December 2021, these negative deviations fall in 2019–2020, and the most plausible reading is that treated counties, which are urban and Starbucks-dense, experienced a sharper pandemic-era decline in NLRB elections than their imputed counterfactual. We therefore read the Starbucks estimate as consistent with, but not independent confirmation of, the county-level result. The media analysis likewise does not pass ($p = 0.594$; Figure A3). Here the pre-treatment estimates are centered on zero without visible trend, but they are imprecise, leading them to fail a formal equivalence test that rules out pre-trends. Finally, concerning the main results reported in Figure 2, estimates from a longer panel that handles treatment reversal in different ways range from 0.02 to 0.08 per county-month for the post-2000 period (SI Figure A10); all are positive and significant.

Within-company spillover of unionization

To estimate whether a successful election leads to organizing at other worksites of the same employer, we use the majority rule in NLRB representation elections as a regression discontinuity. The analysis starts from the harmonized election data described above, cleaned with the same

rules as the county panel, which gives 41,024 elections held between 2000 and 2023, and applies three further restrictions. First, we keep elections in which twenty or more votes were cast, following the studies that use this discontinuity: DiNardo and Lee (2004) analyze elections with twenty or more votes cast and Sojourner et al. (2015) require that at least twenty individuals voted, on the grounds that this minimizes the scope for either side to control the exact outcome. The restriction matters in our data because small elections bunch just above the threshold: among elections with fewer than twenty votes cast, 3,080 were won by a single vote against 1,484 lost by a single vote, and a density test rejects continuity at the threshold ($p < 0.001$), whereas among elections with twenty or more votes cast the distribution of the margin is smooth (SI Figure A12). Second, among these we drop the 577 elections in which the number of challenged ballots was at least as large as the margin, because their recorded tally did not determine the outcome. Third, because the outcome is measured over the twelve months after the election and petitions are observed through March 2024, we keep elections held by March 2023, whose one-year window is fully observed. The estimation sample contains 19,785 elections, 57 percent of which the union won.

Treatment is the outcome of the vote: the union wins when the votes in its favor exceed the votes against, and a tie is a loss. Let T be the number of valid votes cast and F the votes for the union. The union needs $N = \lfloor T/2 \rfloor + 1$ votes to win, so $F - N = 0$ is the narrowest possible win and $F - N = -1$ the narrowest possible loss. The running variable is $r = (F - N + \frac{1}{2})/T$, the vote margin as a share of votes cast, centered midway between the narrowest loss and the narrowest win, with the union winning when $r > 0$. Expressing the margin as a share makes closeness comparable across units of different size, and the half-vote shift places the cut-off halfway between the two nearest attainable values of the margin for every unit size, which matters because the running variable is discrete (Sojourner et al. 2015). In the estimation sample the density of r is continuous at the cut-off ($p = 0.94$), and predetermined characteristics are balanced (SI Section B).

The NLRB identifies employers by free-text names that vary across filings in spelling, punctuation, legal form, and the inclusion of store or plant numbers, so we link elections to employers in two ways and report results under both. The first is an exact match on standardized names: we convert names to lower case, remove punctuation, drop legal designations and common filler words, and strip store, plant, unit, and division numbers, so that "Safeway Stores Inc" and "Safeway Store 60" resolve to the same employer. This identifier is conservative, since a misspelled name leaves an employer split across identifiers, which works against finding within-employer spillovers. The second allows for spelling variation: we compute Jaro–Winkler distances between

standardized names that share a first letter, apply hierarchical clustering with complete linkage, and cut the tree so that every pair of names within a cluster has a similarity of at least 0.85. The exact identifier yields 25,118 employers among elections held between 2000 and 2023 and the clustered identifier 23,076; 34 and 40 percent of elections, respectively, belong to an employer observed in more than one county. Neither identifier distinguishes separately owned franchisees that operate under a shared trade name, nor follows employers through name changes.

The outcome is an indicator equal to one if a petition for a representation election was filed at another worksite of the same employer within twelve months after the election. A worksite counts as another worksite when it lies in a different county, in a different city, or at a different street address where both addresses are recorded; elections under the same NLRB case number never count, so that reruns and multi-unit cases at the same site do not enter the outcome. We date the outcome by the petition's filing date rather than the election date and count only petitions filed after the focal election, because in multi-site campaigns several worksites often petition within weeks of one another, and an election held a month after the focal election may have been petitioned before it. The placebo outcome records, for the same employer, whether an election was held at another worksite in the twelve months before the focal election.

Estimates are local linear regressions on each side of the cut-off with a triangular kernel, the mean-squared-error-optimal bandwidth of Calonico, Cattaneo, and Titiunik (2014), heteroskedasticity-robust standard errors, and the adjustment for mass points in the running variable implemented in `rdrobust` (Calonico, Cattaneo, Farrell, et al. 2017); confidence intervals and p-values are the robust bias-corrected ones. Density tests use the local polynomial estimator of Cattaneo, Jansson, and Ma (2020). Because the running variable is discrete, we also report local randomization comparisons of the elections at the nearest attainable margins on each side of the cut-off (Cattaneo, Frandsen, and Titiunik 2015).

Original survey and experiment

We recruited participants via Prolific to take part in a between-subjects experiment, using pre-registered quotas representative of the US working population across gender, race, age, and party ID. We pre-registered that we would exclude respondents who are not eligible to join a union because they are unemployed or hold a managerial position. To identify respondents in managerial positions, we relied on the definition of a supervisor under the National Labor Relations Act, which covers employees with independent authority to hire, fire, set pay, or promote other employees. Accordingly, we then filtered out such respondents if they indicated they could do so, or fell into ISCO-08 major group 1 (Managers). After applying these exclusion criteria

and two pre-registered attention checks, our final sample comprised 1,091 participants. Participants in the treatment group read two short news articles about successful unionization efforts by workers in their own sector, identified by matching respondents' self-reported occupation to ISCO-08 major groups (2–9). Participants in the control group were either randomized into a pure control condition, moving directly to the outcome measures, or read two placebo articles of similar length and format that covered positive local news unrelated to work or unions (the opening of a community sports facility and of a new zoo). Treatment articles were real and lightly edited to be of similar length, an example can be found in the SI. SI Figure A18 shows no difference in outcomes between the two control conditions, which we therefore pool, as pre-registered. We estimate treatment effects using OLS with pre-registered covariates to increase precision.

Data and code availability

Code and data used to run these analyses will be made available before publication on OSF.

Author contributions

Both authors contributed equally.

Acknowledgments

We thank Jens Hainmueller and Robb Willer for their helpful suggestions. This research has been supported by the TRANSNATIONAL ERC project (Grant No: 885026) and the Politics and Social Change Lab at Stanford University (PASCL).

Ethics

Respondents in the survey experiment provided informed consent. The study received approval from Stanford University's IRB (protocol 83225).

References

Andrews, Kenneth T. and Michael Biggs (2006). "The Dynamics of Protest Diffusion: Movement Organizations, Social Networks, and News Media in the 1960 Sit-Ins". In: *American Sociological Review* 71.5, pp. 752–777. DOI: 10.1177/000312240607100503.

Ariyarathne, Gangani and Alexander C. Nwala (2024). "3DLNews: A Three-decade Dataset of US Local News Articles". In: *Proceedings of the 33rd ACM International Conference on Information and Knowledge Management (CIKM '24)*. Boise, ID, USA: ACM, pp. 1–5. DOI: 10.1145/3627673.3679165. URL: https://doi.org/10.1145/3627673.3679165.

Biggs, Michael (2005). "Strikes as Forest Fires: Chicago and Paris in the Late Nineteenth Century". In: *American Journal of Sociology* 110.6, pp. 1684–1714. DOI: 10.1086/427675.

Bikhchandani, Sushil, David Hirshleifer, and Ivo Welch (1992). "A Theory of Fads, Fashion, Custom, and Cultural Change as Informational Cascades". In: *Journal of Political Economy* 100.5, pp. 992–1026.

Bursztyn, Leonardo et al. (2021). "Persistent Political Engagement: Social Interactions and the Dynamics of Protest Movements". In: *American Economic Review: Insights* 3.2, pp. 233–250. DOI: 10.1257/aeri.20200261.

Calonico, Sebastian, Matias D. Cattaneo, Max H. Farrell, et al. (2017). "rdrobust: Software for Regression-Discontinuity Designs". In: *The Stata Journal* 17.2, pp. 372–404.

Calonico, Sebastian, Matias D. Cattaneo, and Rocío Titiunik (2014). "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs". In: *Econometrica* 82.6, pp. 2295–2326.

Cattaneo, Matias D., Brigham R. Frandsen, and Rocío Titiunik (2015). "Randomization Inference in the Regression Discontinuity Design: An Application to Party Advantages in the U.S. Senate". In: *Journal of Causal Inference* 3.1, pp. 1–24.

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma (2020). "Simple Local Polynomial Density Estimators". In: *Journal of the American Statistical Association* 115.531, pp. 1449–1455.

Centola, Damon and Michael Macy (2007). "Complex contagions and the weakness of long ties". In: *American journal of Sociology* 113.3, pp. 702–734.

Conell, Carol and Samuel Cohn (1995). "Learning from Other People's Actions: Environmental Variation and Diffusion in French Coal Mining Strikes, 1890–1935". In: *American Journal of Sociology* 101.2, pp. 366–403. DOI: 10.1086/230728.

DeCotiis, Thomas A and Jean-Yves LeLouarn (1981). "A predictive study of voting behavior in a representation election using union instrumentality and work perceptions". In: *Organizational Behavior and Human Performance* 27.1, pp. 103–118.

DiNardo, John and David S. Lee (2004). "Economic Impacts of New Unionization on Private Sector Employers: 1984–2001". In: *The Quarterly Journal of Economics* 119.4, pp. 1383–1441.

Farber, Henry S. (2015). "Union Organizing Decisions in a Deteriorating Environment: The Composition of Representation Elections and the Decline in Turnout". In: *ILR Review* 68.5, pp. 1126–1156. DOI: 10.1177/0019793915592623.

Farber, Henry S. et al. (2021). "Unions and Inequality over the Twentieth Century: New Evidence from Survey Data". In: *The Quarterly Journal of Economics* 136.3, pp. 1325–1385. DOI: 10.1093/qje/qjab012.

González, Felipe (2020). "Collective Action in Networks: Evidence from the Chilean Student Movement". In: *Journal of Public Economics* 188, p. 104220. DOI: 10.1016/j.jpubeco.2020.104220.

Gonzalez Vaillant, Gabriela et al. (2016). "A Field-Experimental Study of Emergent Mobilization in Online Collective Action". In: *Mobilization* 21.3, pp. 281–303.

Granovetter, Mark (1978). "Threshold models of collective behavior". In: *American journal of sociology* 83.6, pp. 1420–1443.

Hedström, Peter (1994). "Contagious Collectivities: On the Spatial Diffusion of Swedish Trade Unions, 1890–1940". In: *American Journal of Sociology* 99.5, pp. 1157–1179. DOI: 10.1086/230408.

Hedström, Peter, Rickard Sandell, and Charlotta Stern (2000). "Mesolevel Networks and the Diffusion of Social Movements: The Case of the Swedish Social Democratic Party". In: *American Journal of Sociology* 106.1, pp. 145–172. DOI: 10.1086/303109.

Hertel-Fernandez, Alexander, Suresh Naidu, and Adam Reich (2021). "Schooled by strikes? The effects of large-scale labor unrest on mass attitudes toward the labor movement". In: *Perspectives on Politics* 19.1, pp. 73–91.

Holmes, Thomas J (2006). *Geographic spillover of unionism*.

Jenkins, J. Craig (1983). "Resource Mobilization Theory and the Study of Social Movements". In: *Annual Review of Sociology* 9, pp. 527–553. DOI: 10.1146/annurev.so.09.080183.002523.

Kamphorst, Jonne and Robb Willer (2024). "Correcting misperceptions of the material benefits associated with union membership increases Americans' interest in joining unions". In: *Proceedings of the National Academy of Sciences* 121.19, e2321025121.

Kuran, Timur (1991). "Now Out of Never: The Element of Surprise in the East European Revolution of 1989". In: *World Politics* 44.1, pp. 7–48.

Lee, David S. and Alexandre Mas (2012). "Long-Run Impacts of Unions on Firms: New Evidence from Financial Markets, 1961–1999". In: *The Quarterly Journal of Economics* 127.1, pp. 333–378.

Liu, Licheng, Ye Wang, and Yiqing Xu (2024). "A practical guide to counterfactual estimators for causal inference with time-series cross-sectional data". In: *American Journal of Political Science* 68.1, pp. 160–176.

Lohmann, Susanne (1994). "The Dynamics of Informational Cascades: The Monday Demonstrations in Leipzig, East Germany, 1989–91". In: *World Politics* 47.1, pp. 42–101.

Madestam, Andreas et al. (2013). "Do Political Protests Matter? Evidence from the Tea Party Movement". In: *The Quarterly Journal of Economics* 128.4, pp. 1633–1685. DOI: 10.1093/qje/qjt021.

Manacorda, Marco and Andrea Tesei (2020). "Liberation technology: Mobile phones and political mobilization in Africa". In: *Econometrica* 88.2, pp. 533–567.

Manski, Charles F (1993). "Identification of endogenous social effects: The reflection problem". In: *The review of economic studies* 60.3, pp. 531–542.

Margetts, Helen et al. (2011). "Social Information and Political Participation on the Internet: An Experiment". In: *European Political Science Review* 3.3, pp. 321–344. DOI: 10.1017/S1755773911000129.

Marwell, Gerald, Pamela E Oliver, and Ralph Prahl (1988). "Social networks and collective action: A theory of the critical mass. III". In: *American journal of Sociology* 94.3, pp. 502–534.

McAdam, Doug (1986). "Recruitment to high-risk activism: The case of freedom summer". In: *American journal of sociology* 92.1, pp. 64–90.

McAdam, Doug, John D. McCarthy, and Mayer N. Zald, eds. (1996). *Comparative Perspectives on Social Movements: Political Opportunities, Mobilizing Structures, and Cultural Framings*. Cambridge: Cambridge University Press. DOI: 10.1017/CBO9780511803987.

Naidu, Suresh (2022). "Is There Any Future for a US Labor Movement?" In: *Journal of Economic Perspectives* 36.4, pp. 3–28. DOI: 10.1257/jep.36.4.3.

Olson, Mancur (1971). *The Logic of Collective Action: Public Goods and the Theory of Groups*. Vol. 124. Harvard University Press.

Rijt, Arnout van de, Idil Afife Akin, et al. (2016). "Success-Breeds-Success in Collective Political Behavior: Evidence from a Field Experiment". In: *Sociological Science* 3, pp. 940–950. DOI: 10.15195/v3.a41.

Rijt, Arnout van de, Soong Moon Kang, et al. (2014). "Field Experiments of Success-Breeds-Success Dynamics". In: *Proceedings of the National Academy of Sciences* 111.19, pp. 6934–6939. DOI: 10.1073/pnas.1316836111.

Salganik, Matthew J., Peter Sheridan Dodds, and Duncan J. Watts (2006). "Experimental Study of Inequality and Unpredictability in an Artificial Cultural Market". In: *Science* 311.5762, pp. 854–856. DOI: 10.1126/science.1121066.

Schaller, Zachary, Sal McCollum, and Prasiddha Shakya (2026). "A double-shot of organizing: how Starbucks workers have inspired the labour movement". In: *Applied Economics Letters*, pp. 1–6.

Schaller, Zachary, S. Young, and Jonne Kamphorst (2026). *Union Election Stats*. Self published. Retrieved from unionelectionstats.com. URL: https://unionelectionstats.com.

Sojourner, Aaron J. et al. (2015). "Impacts of Unionization on Quality and Productivity: Regression Discontinuity Evidence from Nursing Homes". In: *ILR Review* 68.4, pp. 771–806.

Strang, David and Sarah A Soule (1998). "Diffusion in organizations and social movements: From hybrid corn to poison pills". In: *Annual review of sociology* 24.1, pp. 265–290.

Wang, Dan J. and Sarah A. Soule (2012). "Social Movement Organizational Collaboration: Networks of Learning and the Diffusion of Protest Tactics, 1960–1995". In: *American Journal of Sociology* 117.6, pp. 1674–1722. DOI: 10.1086/664685.

Youngblood, Stuart A et al. (1984). "The impact of work environment, instrumentality beliefs, perceived labor union image, and subjective norms on union voting intentions". In: *Academy of Management Journal* 27.3, pp. 576–590.

Supplementary Information

Contents

A Additional results for the DID **1**

B Further details about the RDD **7**

B.1 Further coding details 7

B.2 Robustness analysis 7

C Additional results from the survey experiment **13**

D Example of treatment article **18**

E Survey outcome measures **19**

A Additional results for the DID

Figure A6 shows the main results plot for the media analysis, which was not reported in the main paper due to space reasons. Figures A1, A2, and A3 test parallel trends for the results reported in the paper. As discussed in the main paper, parallel trends hold for our main specification, but not for the media analysis or Starbucks only results: both these analyses are influenced more strongly by the covid-era delay and then increase in unionization attempts. Furthermore, figure A9 reproduces the main set of results as shown in Figure 2 with different estimators. The results hold up with the unbiased estimators. The biased TWFE estimator has a significant yet smaller result.

Figure A7 repeats the contiguity model of Figure 3 with state-by-month in place of month fixed effects, so that state-level shocks to organizing are absorbed along with national ones. The gradient is unchanged: the first-order estimate is of the same size as in the main specification and marginally significant ($p = 0.055$), and the second- and third-order estimates remain close to one.

To further address the concern that the county-level estimates pick up elections that are part of the same campaign rather than new organizing, we re-estimate the main specification with a narrower outcome. We remove from the outcome the elections most characteristic of an organizing spell: elections under the same NLRB case as a previous win (a second bargaining unit voting after the first has won), and elections held by an employer that had a successful election in the same county in the previous five years (the winning employer's other worksites). To do so,

we identify employers by the standardized names following the RDD procedure as described in Materials and Methods. This removes 10.9 percent of the elections held between 2000 and 2023 from the outcome. Treatment is unchanged, so the difference between this estimate and the main one is the part of the effect that comes from the treating case or employer. The estimate is 0.095 (95% CI [0.077, 0.114], $p < 0.001$), against 0.111 in the main specification (Figure A8). About 14 percent of the main effect thus reflects the winning employer's own later elections; the rest is organizing at other employers.

A remaining concern is treatment reversal. The baseline specification codes a successful drive as on for five years and then off, and FEct uses all untreated months—including months after switch-off—to fit the counterfactual. If treatment effects persist beyond that window, those post-reversal months are contaminated controls and the ATT is biased downward (because control units then already have an upward trend). Dropping post-reversal observations from the post-2000 panel is not informative because most counties treated after 2000 were already treated at the start of that window, meaning that the remaining sample would be small and peculiar (only those mostly rural and remote places that had not before seen any unionization). To nonetheless speak to this issue, Figure A10 re-estimates the same FEct model on a panel that begins in 1975, which gives counties treated in the 2000s and 1990s a longer pre-period to use in the estimation of the counterfactual control trends. From that single fit we report the overall ATT and the calendar-restricted ATTs on treated months before and after 2000. We then re-estimate the model after excluding from the control pool the first 1, 3, 5, or 8 years after switch-off, and after dropping all post-reversal untreated months. Point estimates differ across these specifications because they average over different treated county-months and use different control pools, yet all remain positive and statistically significant.

Figure A1: Parallel trends test for Figure 2

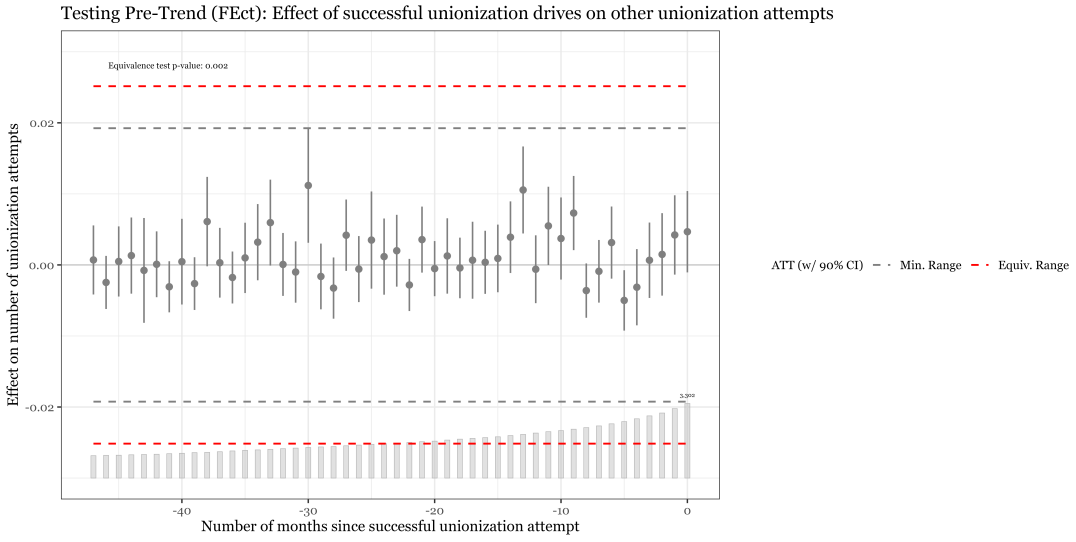


Figure A2: Parallel trends test for Figure 1

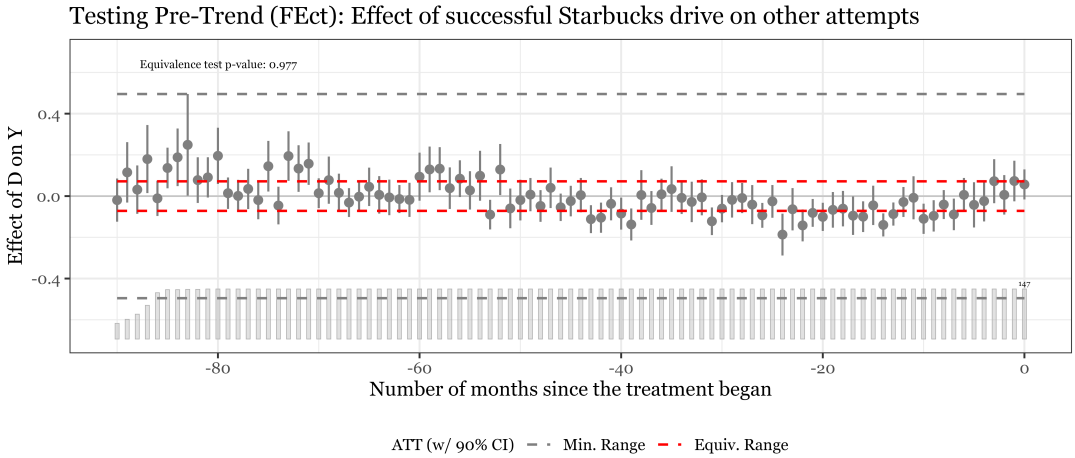


Figure A3: Parallel trends test for Figure A6

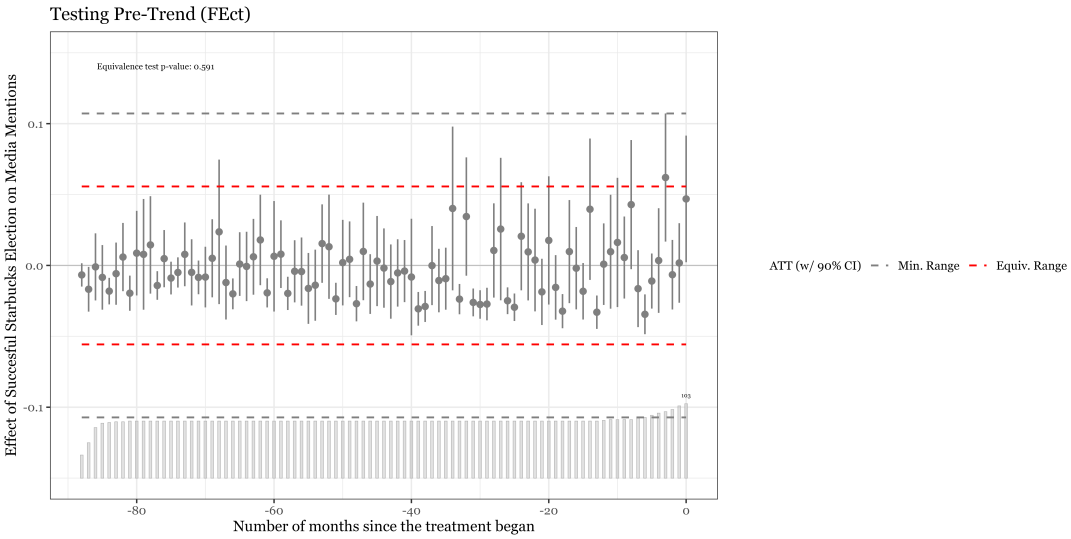


Figure A4: Visualization of the analysis where treatment is coded as a failed election

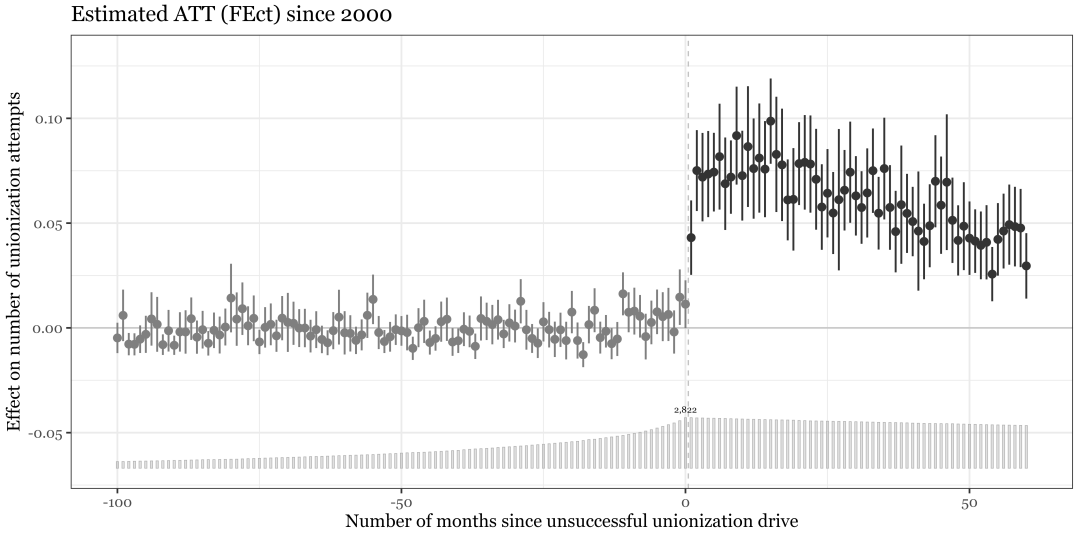


Figure A5: Parallel trends test for Figure A4

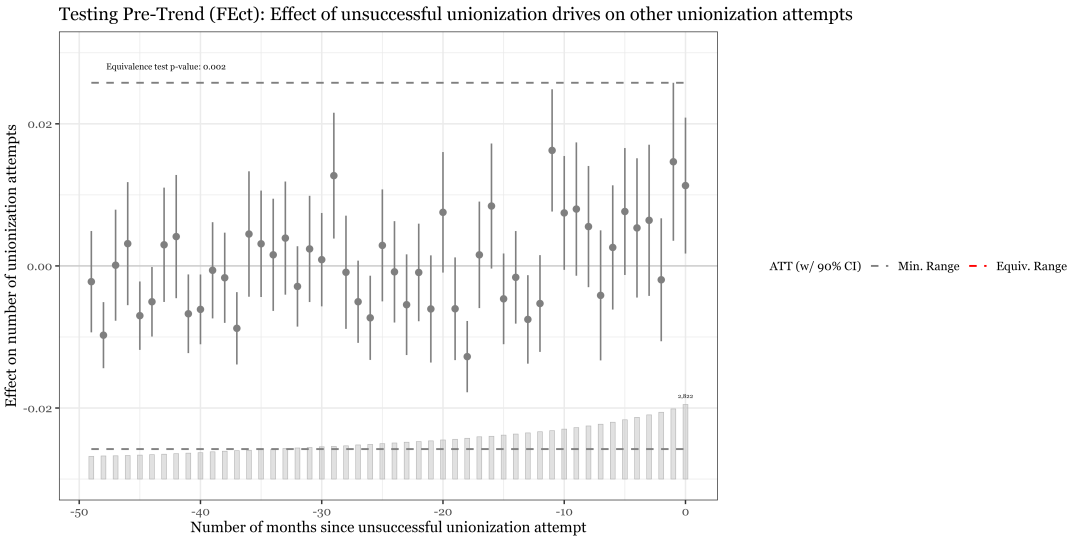


Figure A6: Visualization of the media analysis

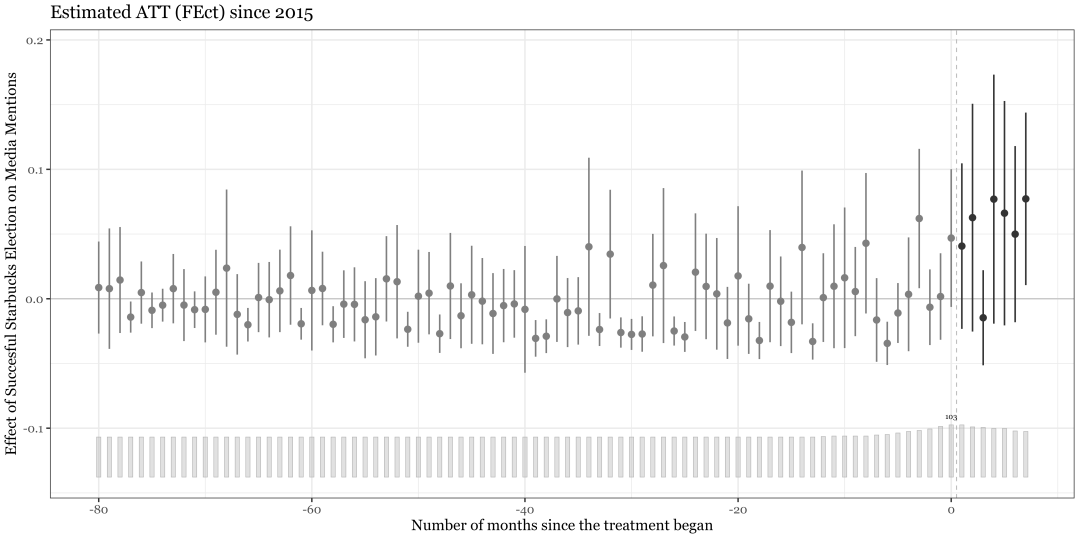


Figure A7: Spillovers taper with distance, including state-by-month fixed effects

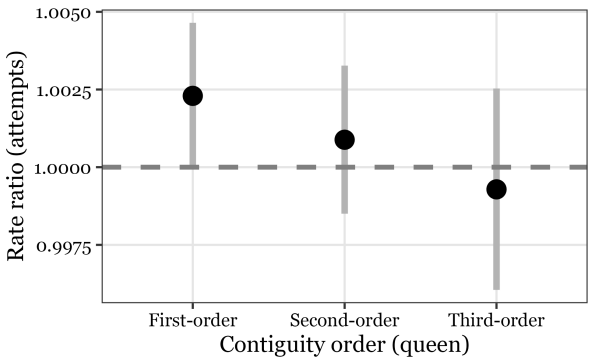


Figure A8: DiD analysis with outcome that excludes elections that are most likely to be part of an organizing spell

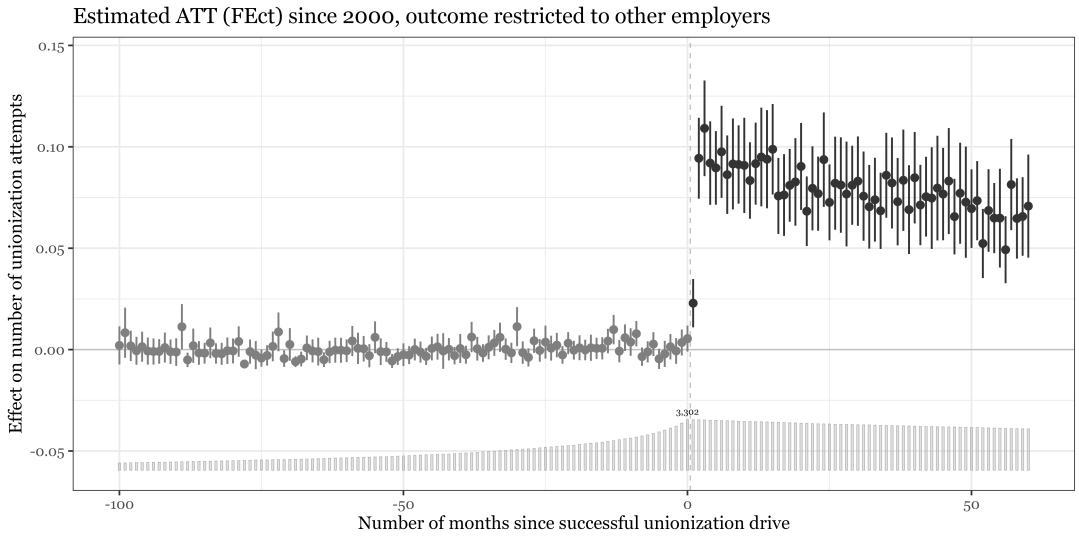


Figure A9: Main results from Figure 2 with different estimators

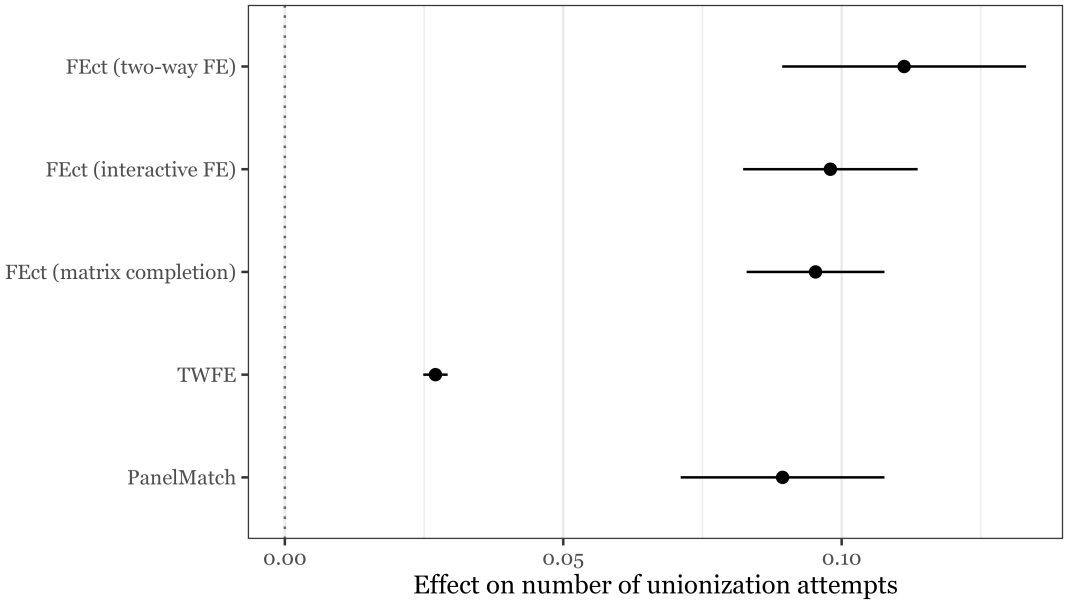
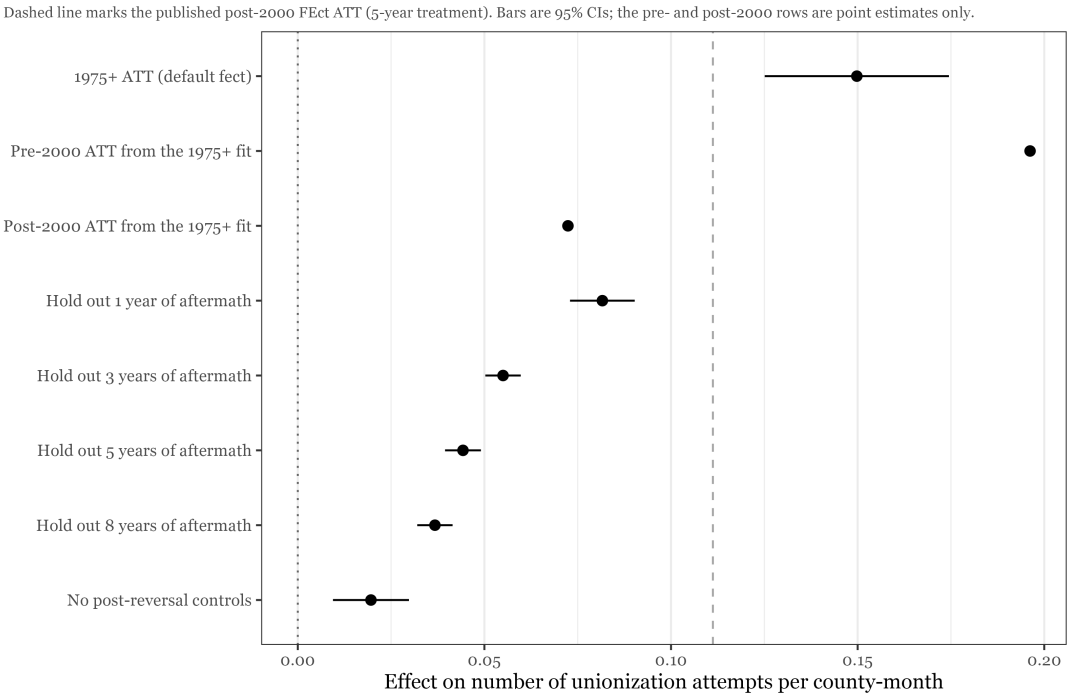


Figure A10: Tests using data from before 2000



B Further details about the RDD

B.1 Further coding details

Treatment in the regression discontinuity design is the outcome of the vote rather than the certified outcome of the case. In 1.8 percent of elections held between 2000 and 2023 (755 of 41,024) the case was later certified with the opposite outcome. One fifth of these occur in cases covering several bargaining units, where the closing reason is recorded once per case, so that a unit that lost inside a case in which another unit won carries that unit's certification. The rest reflect post-election proceedings, mainly objections that led to the election being set aside and challenged ballots resolved after the tally, and a small number of recording errors. Excluding the 188 such elections in the estimation sample leaves the estimates unchanged.

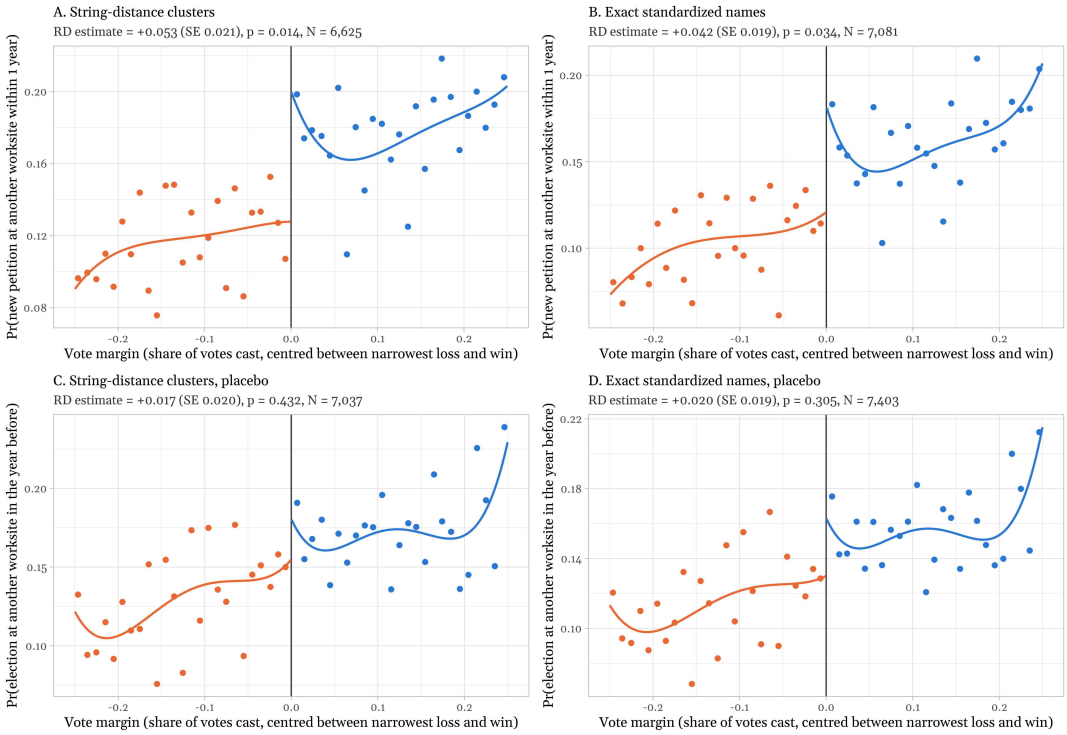
Filing dates are missing for 982 elections held in 2000 or later, almost all of them held from August 2023 onwards. For these we impute the filing date as the election date minus 28 days, the median lag between petition and election since 2015. The last observed election was held on 22 April 2024, so petitions can be observed through 25 March 2024, and the estimation sample keeps elections held by 25 March 2023. Excluding these cases does not change the estimates (see below).

B.2 Robustness analysis

This section reports the diagnostics and robustness checks for the within-employer regression discontinuity design in Figure 4. Figure A11 reports two robustness tests for the within-employer estimate. First, panel B re-estimates the effect with a second way of determining which elections belong to the same company: an exact match on standardized employer names. We prefer the string-distance clusters used in the main text because small typos and variant spellings of company names are common in the NLRB records ("Allied Waste Transportation" and "Allied Waste Trnsportation," for instance, or "Starbucks" and "Starbucks C."). The main result holds under the exact match (RD estimate = 0.042, 95% CI [0.003, 0.091], $p = 0.034$). As expected, the estimate is somewhat smaller than under the string-distance clusters (0.042 against 0.053), since some elections at the same company are no longer linked to one another. Second, panels C and D report a placebo test under both measures, in which the outcome is whether an election was held at another worksite of the same employer in the twelve months before the focal election. A win cannot affect what happened before it, so a discontinuity in this outcome would indicate that narrow winners were already employers in the middle of multi-site organizing campaigns, and that the jumps in panels A and B reflect this difference rather than the win itself. Neither

measure shows such a discontinuity (RD estimates of 0.017, 95% CI [−0.029, 0.067], $p = 0.43$, and 0.020, 95% CI [−0.021, 0.067], $p = 0.31$).

Figure A11: Within-employer estimates under both employer identifiers, with the placebo outcome

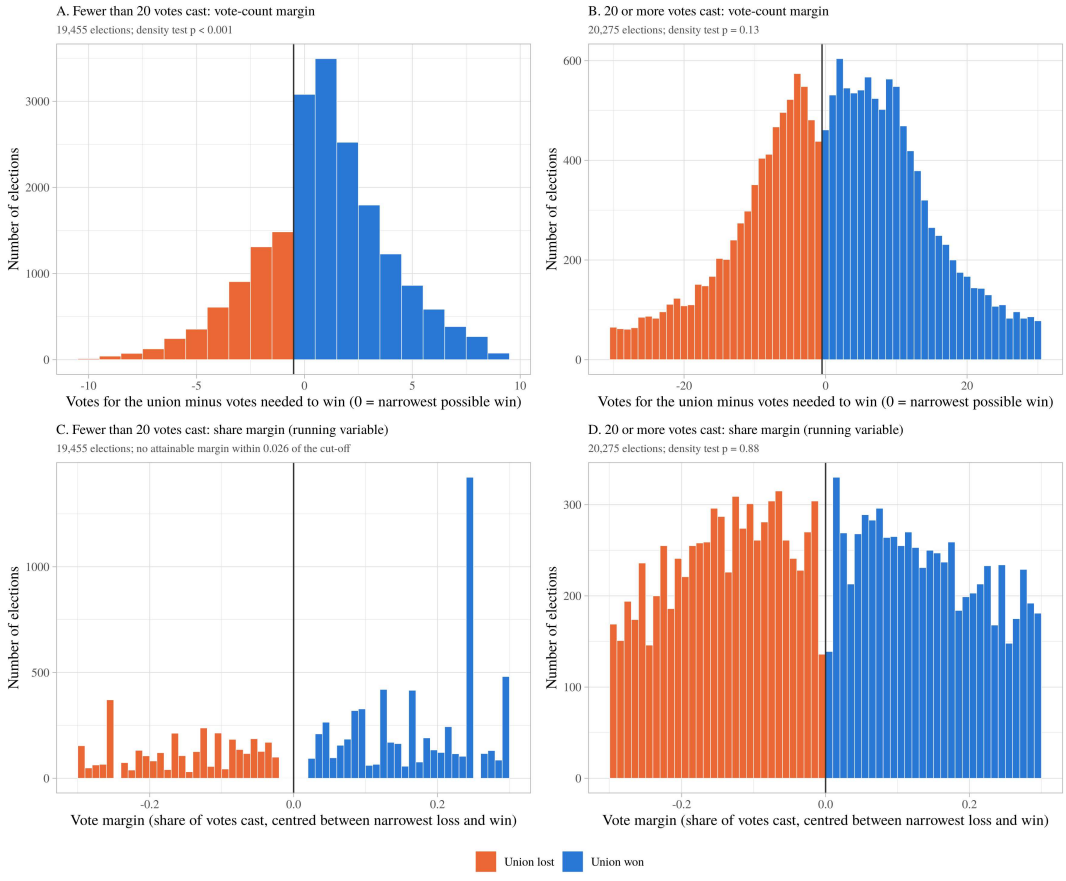


Note: Panel A is the estimate in Figure 4. Panel B shows the same outcome with employers identified by an exact match on standardized names. Panels C and D show, for the same two ways of identifying employers, the probability of an election at another worksite of the same employer in the year before the focal election. Sample, running variable, binning and estimation as in Figure 4.

Figure A12 motivates two choices in the design: the restriction to elections with twenty or more votes cast, and the definition of the running variable. It plots the distribution of the vote margin, colored by the outcome of the vote, for elections with fewer than twenty votes cast (left) and twenty or more (right), in votes (top row) and as the share margin used as the running variable (bottom row). Among the 19,455 small elections the mass piles up just above the threshold: 3,080 were won by a single vote against 1,484 lost by a single vote, and the density test of Cattaneo, Jansson, and Ma (2020) rejects continuity ($p < 0.001$). This pattern likely reflects unions' ability to gauge support in very small units before filing rather than manipulation of ballots, but either way it rules out a local comparison. Moreover, in these elections the running variable takes only a handful of values and has no attainable value close to the cut-off (panel C). Among the 20,275 elections with twenty or more votes cast, the distribution is smooth through the threshold both in votes (panel B, $p = 0.13$) and in the share margin (panel D, $p = 0.88$). The threshold of twenty votes is the one used by DiNardo and Lee (2004) and Sojourner et al. (2015).

We use the share margin rather than the vote-count margin as the running variable because it makes closeness comparable across units of different size: within the estimation bandwidth of 0.13, a worksite with 25 voters is included only if it was decided by three votes or fewer, whereas a worksite with 200 voters is included up to a margin of 26 votes. We construct the share margin as DiNardo and Lee (2004) and Sojourner et al. (2015) do. Because the values the vote share can take change mechanically with the number of votes cast, they subtract half a vote ($0.5/T$) from the union's vote share in elections with an even number of votes cast, which places the cut-off midway between the narrowest possible loss and the narrowest possible win in every election. Without this adjustment, the union's vote share minus one half places every tie exactly at the cut-off on the losing side while no win lies closer to it than half a vote, and a local density estimator reads this asymmetry as excess mass on the losing side ($p < 0.001$ among elections with twenty or more votes cast, against $p = 0.88$ for the adjusted margin).

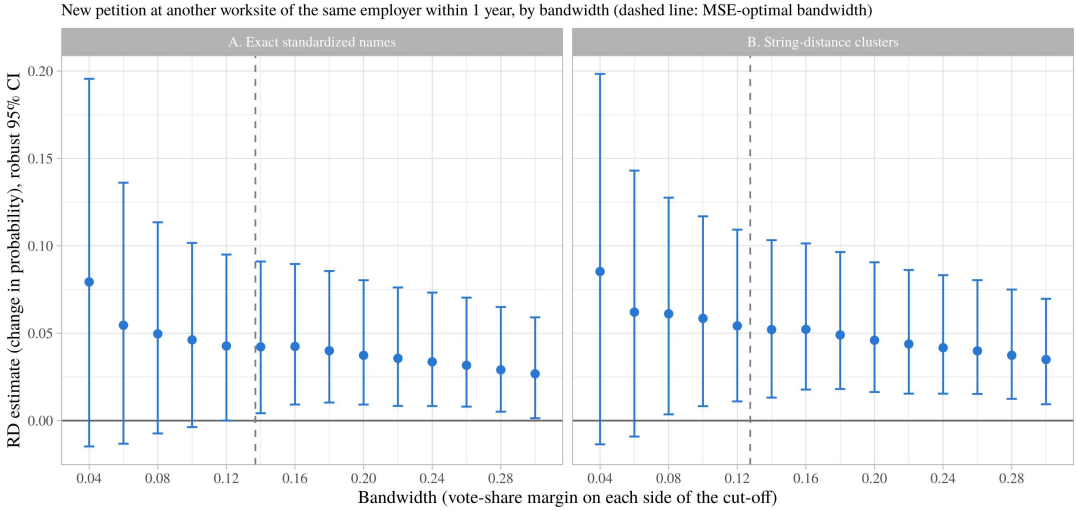
Figure A12: Distribution of the vote margin below and above twenty votes cast



Note: NLRB representation elections held between 2000 and 2023, excluding elections in which challenged ballots could have changed the outcome, for elections with fewer than twenty votes cast (left) and twenty or more (right). The top row shows the vote-count margin, votes for the union minus the votes needed to win, with the cut-off between the narrowest loss (-1) and the narrowest win (0); the bottom row shows the share margin used as the running variable. Colour indicates the outcome of the vote. Density tests are the Cattaneo, Jansson, and Ma (2020) tests at the cut-off.

Figure A13 re-estimates the one-year effect at bandwidths set by hand from 0.04 to 0.30 for both employer identifiers; the dashed line marks the MSE-optimal bandwidth used in Figure 4. The estimate is positive throughout, lies between 4 and 6 percentage points at bandwidths from 0.06 to 0.18, and declines slowly to about 3 percentage points at 0.30 as the linear fit absorbs more of the curvature in the outcome.

Figure A13: Sensitivity of the within-employer estimate to the bandwidth

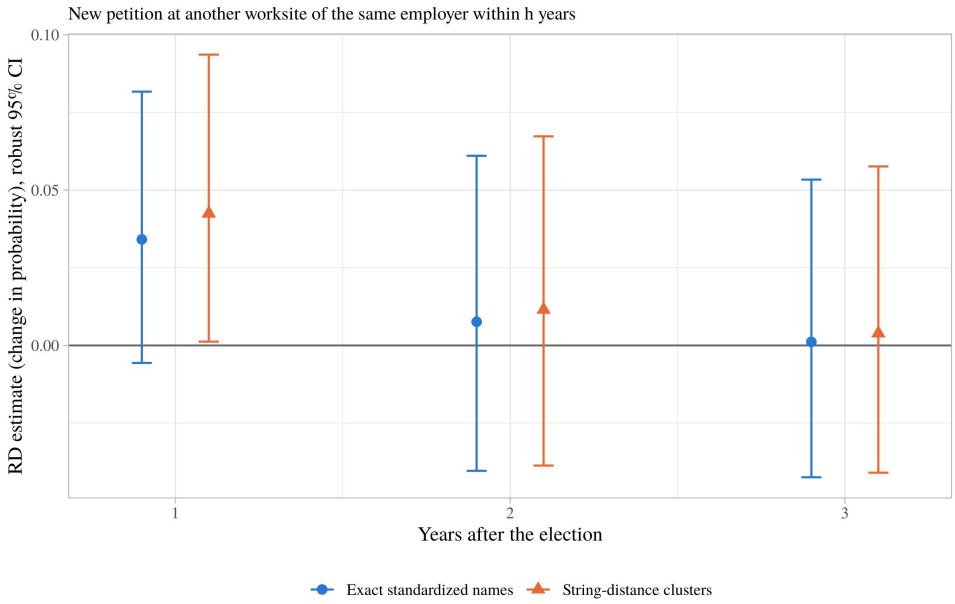


Note: The one-year estimate from Figure 4 at bandwidths set by hand from 0.04 to 0.30, for employers identified by exact standardized names (A) and by string-distance clusters (B). The dashed line marks the MSE-optimal bandwidth. Bars are robust bias-corrected 95% confidence intervals.

Figure A14 extends the outcome window to two and three years. Because later windows are observed only for earlier elections, the figure uses the 18,851 elections held by March 2021, whose three-year window is fully observed, so that the estimates at different horizons are computed on the same elections; the one-year estimate is therefore somewhat smaller than in Figure 4, which also includes the elections held between March 2021 and March 2023. The outcomes are cumulative, so the three-year indicator counts any petition filed within three years. On this sample the one-year estimate is 3.4 percentage points under exact names (95% CI [−0.006, 0.082], $p = 0.088$) and 4.3 under the clusters (95% CI [0.001, 0.094], $p = 0.044$); at two years it is 0.8 and 1.2 percentage points and at three years 0.1 and 0.4, none of them distinguishable from zero.

Figure A15 reports the one-year estimate under alternative specifications: half and twice the optimal bandwidth, a local quadratic fit, a uniform kernel, the vote-count margin in place of the share margin as the running variable, a donut specification that drops the two attainable margins nearest the cut-off, the exclusion of Starbucks, the exclusion of the 188 elections whose certified outcome differs from the recorded tally, covariate adjustment for the log number of eligible voters and the election year, standard errors clustered by employer, the restriction to

Figure A14: Within-employer estimates over one, two, and three years

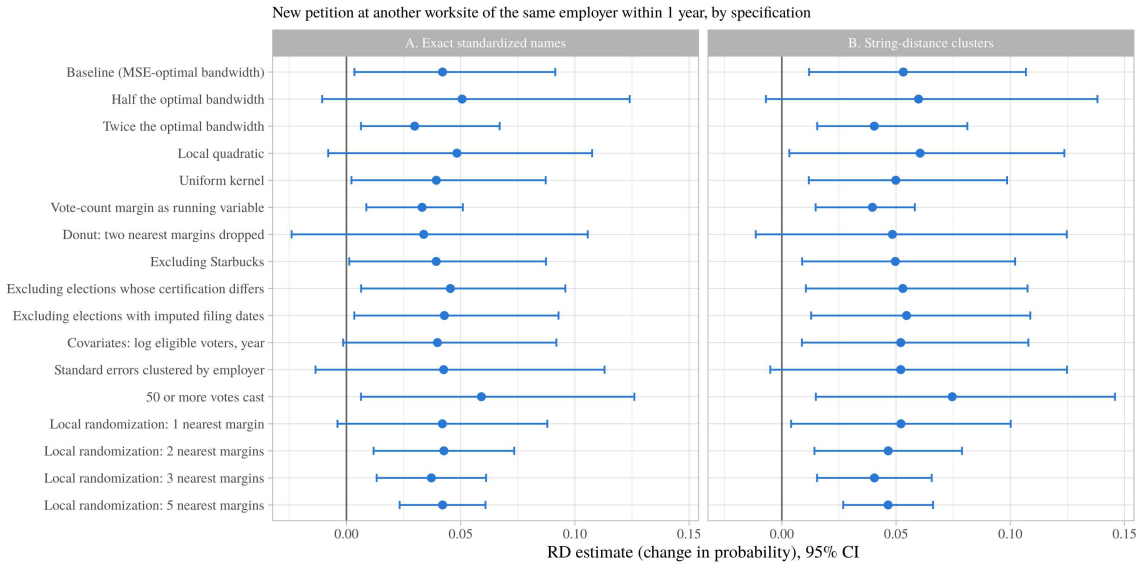


Note: Regression discontinuity estimates of the effect of a narrow union victory on the probability of a new petition at another worksite of the same employer within one, two, and three years, on the elections whose three-year window is fully observed (held between January 2000 and March 2021, twenty or more votes cast, no determinative challenged ballots; n = 18,851). Outcomes are cumulative. Bars are robust bias-corrected 95% confidence intervals.

elections with fifty or more votes cast, and local randomization comparisons of the elections at the one, two, three, and five nearest attainable margins on each side of the cut-off (Cattaneo, Frandsen, and Titiunik 2015). The point estimate lies between 3 and 6 percentage points in every specification under both identifiers. The confidence interval includes zero in the least precise specifications under exact names (half the optimal bandwidth, the local quadratic, the donut, and employer-clustered standard errors) and in the donut and clustered specifications under the string-distance clusters.

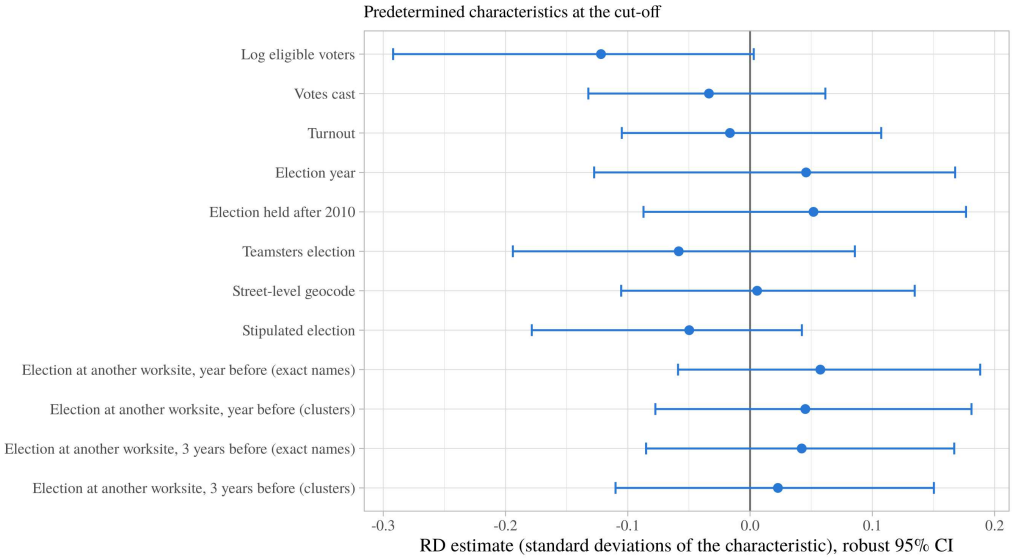
Figure A16 estimates the discontinuity at the cut-off in predetermined characteristics of the elections, expressed in standard deviations of each characteristic: the log number of eligible voters, votes cast, turnout, the election year, whether the election was held after 2010, whether the Teamsters were the petitioning union, whether the worksite is geocoded at the street level, whether the election was held under a stipulated election agreement, and whether the employer had an election at another worksite in the one and three years before the focal election, under both identifiers. None of the discontinuities is distinguishable from zero at the 5 percent level; the largest is in the log number of eligible voters (-0.12 standard deviations, $p = 0.055$), and the estimate is unchanged when this variable is included as a covariate (Figure A15).

Figure A15: The within-employer estimate across specifications



Note: The one-year estimate from Figure 4 under the specifications listed on the left, for employers identified by exact standardized names (A) and by string-distance clusters (B). Bars are robust bias-corrected 95% confidence intervals; for the local randomization comparisons, which are differences in means between the elections at the nearest attainable margins on each side of the cut-off, they are conventional 95% confidence intervals.

Figure A16: Discontinuities in predetermined characteristics at the cut-off



Note: Regression discontinuity estimates at the majority threshold for predetermined characteristics of the elections in the estimation sample of Figure 4, each divided by the standard deviation of the characteristic. Estimates are local linear with a triangular kernel and MSE-optimal bandwidth. Bars are robust bias-corrected 95% confidence intervals.

Table A1: Difference between the treatment and the control arm among Republicans, in points on the 0-100 scale. Each row comes from the interaction model for that outcome, with Republicans as the reference category.

Outcome	Difference	SE	t	p
Interest in joining	13.32	3.51	3.80	<0.001
General support	10.43	2.65	3.94	<0.001
Material benefits	13.47	3.12	4.31	<0.001
Efficacy	4.73	2.87	1.64	0.100
Feasibility	8.69	3.30	2.64	0.008
Costly action	12.26	3.51	3.49	<0.001
Perceived image	12.06	2.61	4.63	<0.001
Perceived interest	16.42	3.08	5.33	<0.001
Perceived costly action	11.82	3.02	3.91	<0.001

C Additional results from the survey experiment

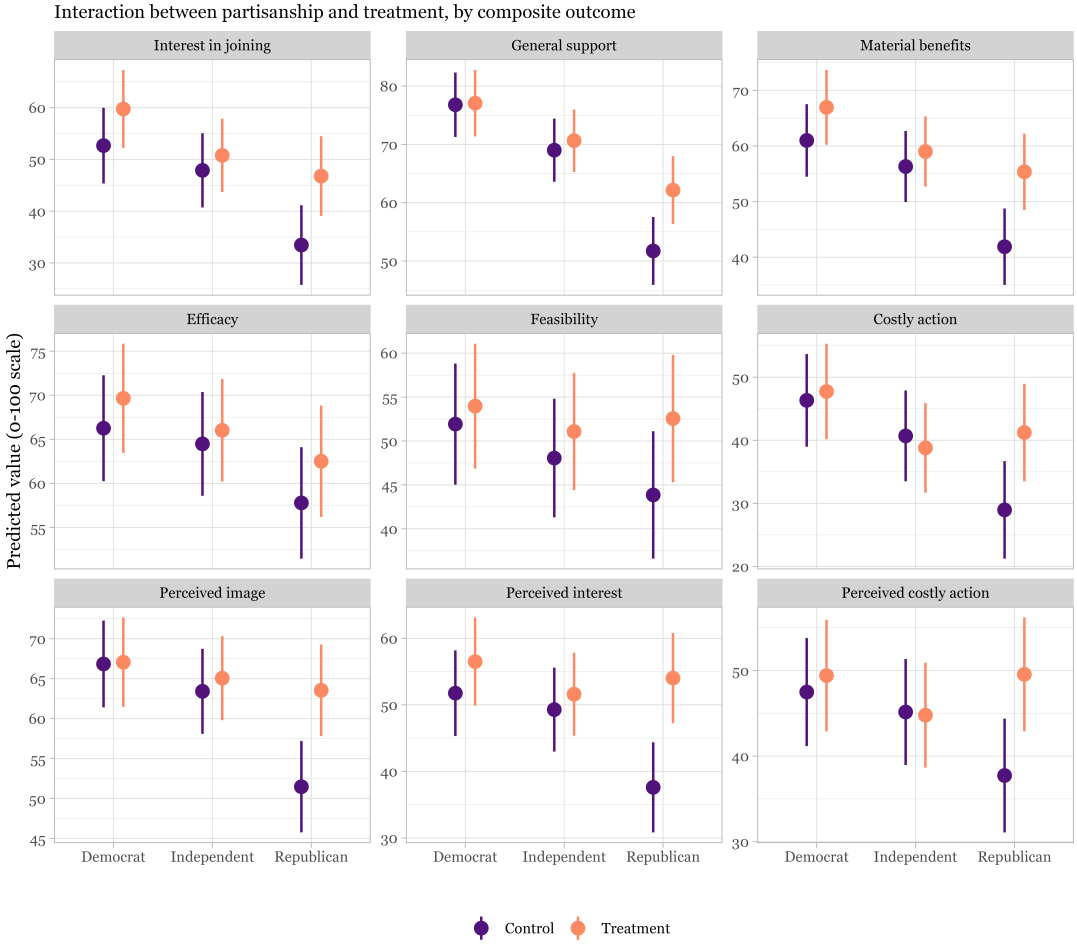
In figure A17 we show the results of the treatment interacted with partisanship (leaners are coded as independents) for all nine outcomes. Table A1 provides a formal test for Republicans, comparing differences in treatment and control among them. The results indicate that the treatment effects are substantially larger among Republicans.

In Figure A18 we present the results of a test where we show treatment effects on the interest in joining composite outcome broken down by the individual control conditions: treatment vs both controls pooled, vs the pure control, or vs the placebo articles; we also compare placebo articles vs the pure control. The results of this test indicate that there are no differences between the two control conditions, as expected.

Table A2 shows the population quotas and the sample proportions. As can be seen, the sample is reasonably close to the targets, with the exception of gender: the sample is more female than the population quota.

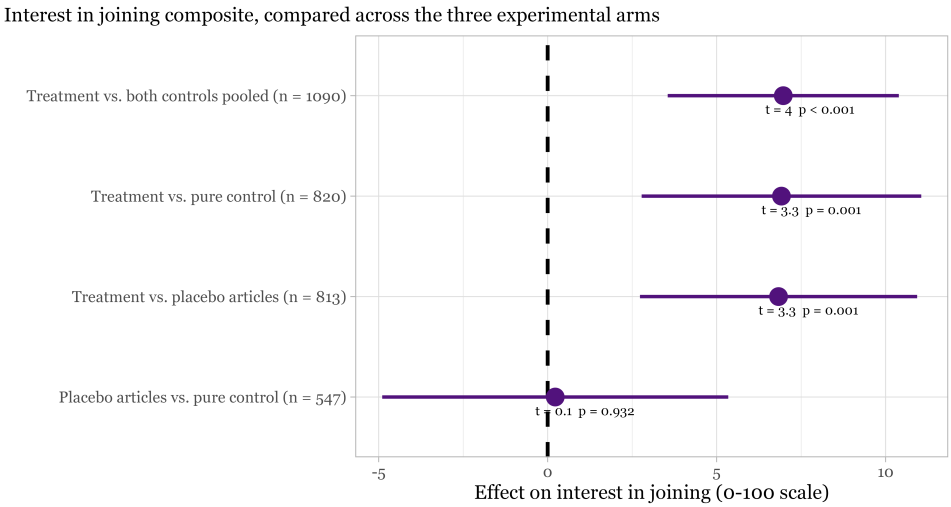
Finally, Table A4 shows the full regression equations for all the outcomes as reported in Figure 6.

Figure A17: Survey treatment interacted with partisanship



Note: Treatment effects by partisanship. Independent leaners are coded as independents. 95% confidence intervals are shown. See SI Table A1 for statistical tests on the difference in treatment and control for Republican respondents, and Table A3 for a regression with Democrats as the reference value.

Figure A18: Test of different control conditions



Note: Treatment effects on the interest in joining composite outcome broken down by control condition (placebo articles, pure control, or both combined). The n indicates the number of respondents in each regression.

Table A2: Sample characteristics of the union experiment. The target column gives the shares the survey wave was recruited against, the sample column the composition of the 1091 respondents the analysis runs on.

Dimension	Group	Target %	Sample %
Sex (CPS 2025, employed 18+)	Male	53.0	44.9
	Female	47.0	54.1
	Other gender	–	1.0
Age (CPS 2025, employed 18+)	18-24	11.0	12.4
	25-34	22.5	25.3
	35-44	23.0	22.5
	45-54	20.0	18.3
	55+	23.5	21.4
Race (ACS 2024, employed)	White	60.7	68.6
	Mixed	12.3	7.3
	Black	11.4	12.9
	Other	8.7	5.2
	Asian	6.9	6.0
Party Affiliation (Prolific)	Independent	42.6	39.5
	Democrat	29.5	36.1
	Republican	27.9	24.4

Table A3: Full regression output for the nine composites with the treatment interacted with partisanship, Democrats as the reference category.

	Interest in joining	General support	Material benefits	Efficacy	Feasibility	Costly action	Perc. image	Perc. interest	Perc. costly action
Intercept	44.30***	60.35***	48.52***	54.48***	49.12***	38.21***	61.90***	49.29***	46.02***
	(6.30)	(4.76)	(5.62)	(5.17)	(5.93)	(6.32)	(4.68)	(5.54)	(5.43)
Treatment (Democrats)	7.08*	0.27	5.95*	3.39	2.05	1.41	0.21	4.74	1.93
	(2.90)	(2.19)	(2.58)	(2.38)	(2.73)	(2.91)	(2.15)	(2.55)	(2.50)
Age	−0.56***	−0.33***	−0.36***	−0.25***	−0.38***	−0.44***	−0.27***	−0.35***	−0.32***
	(0.07)	(0.05)	(0.06)	(0.05)	(0.06)	(0.07)	(0.05)	(0.06)	(0.06)
Female	1.23	1.13	1.80	0.35	3.31	0.14	2.17	2.56	1.48
	(1.82)	(1.38)	(1.63)	(1.50)	(1.72)	(1.83)	(1.35)	(1.60)	(1.57)
Other gender	1.92	4.02	4.56	6.25	2.66	10.87	−2.23	−3.83	−0.68
	(8.88)	(6.71)	(7.92)	(7.29)	(8.36)	(8.91)	(6.59)	(7.81)	(7.66)
Asian	5.32	0.93	4.22	0.19	−1.48	2.71	4.54	6.77*	3.45
	(3.82)	(2.89)	(3.40)	(3.13)	(3.60)	(3.83)	(2.83)	(3.36)	(3.29)
Black	7.75**	2.75	7.32**	9.87***	11.88***	5.93*	6.27**	10.27***	8.15***
	(2.69)	(2.03)	(2.40)	(2.21)	(2.53)	(2.70)	(2.00)	(2.36)	(2.33)
Latino	1.93	0.82	3.51	7.80*	9.16*	2.72	4.51	5.67	4.78
	(4.50)	(3.40)	(4.01)	(3.69)	(4.23)	(4.51)	(3.34)	(3.95)	(3.88)
Multiple races	2.30	1.79	4.00	1.23	2.67	3.34	0.27	1.51	3.66
	(3.44)	(2.60)	(3.06)	(2.82)	(3.24)	(3.45)	(2.55)	(3.02)	(2.97)
Other race	13.37	5.06	6.85	−2.64	−3.71	7.52	10.29	11.03	9.90
	(8.35)	(6.32)	(7.45)	(6.86)	(7.87)	(8.39)	(6.20)	(7.35)	(7.21)
BA or higher	3.19	6.08**	3.62	−0.62	−0.76	3.19	2.18	0.69	−2.47
	(3.04)	(2.30)	(2.71)	(2.49)	(2.86)	(3.05)	(2.25)	(2.67)	(2.63)
Some college	4.61	5.54*	3.82	−1.56	−0.93	4.22	1.70	2.12	−0.05
	(3.03)	(2.29)	(2.70)	(2.49)	(2.85)	(3.04)	(2.25)	(2.66)	(2.62)
Income	−0.14	−0.22	−0.29	0.13	−0.14	−0.33	−0.19	−0.31	−0.07
	(0.27)	(0.20)	(0.24)	(0.22)	(0.25)	(0.27)	(0.20)	(0.24)	(0.23)
Independent	−4.79	−7.79***	−4.68	−1.78	−3.87	−5.64	−3.42	−2.47	−2.33
	(2.89)	(2.18)	(2.58)	(2.37)	(2.72)	(2.90)	(2.14)	(2.54)	(2.49)
Republican	−19.22***	−25.07***	−19.11***	−8.47**	−8.06**	−17.36***	−15.36***	−14.16***	−9.75***
	(3.30)	(2.49)	(2.94)	(2.71)	(3.10)	(3.31)	(2.45)	(2.90)	(2.84)
Treatment × Independent	−4.18	1.37	−3.26	−1.85	0.98	−3.30	1.43	−2.43	−2.30
	(4.00)	(3.03)	(3.57)	(3.29)	(3.77)	(4.02)	(2.97)	(3.52)	(3.46)
Treatment × Republican	6.25	10.17**	7.52	1.33	6.65	10.85*	11.85***	11.68**	9.89*
	(4.55)	(3.44)	(4.06)	(3.73)	(4.28)	(4.57)	(3.38)	(4.00)	(3.92)
Empathy	0.36***	0.37***	0.33***	0.23***	0.19***	0.32***	0.19***	0.21***	0.17***
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Union member	20.18***	11.89***	15.76***	12.25***	21.61***	16.48***	10.45***	12.94***	9.38***
	(2.11)	(1.60)	(1.88)	(1.73)	(1.99)	(2.12)	(1.57)	(1.86)	(1.82)
R^2	0.27	0.32	0.26	0.17	0.19	0.20	0.18	0.19	0.13
Adj. R^2	0.25	0.31	0.24	0.15	0.17	0.18	0.16	0.17	0.11
Num. obs.	1090	1091	1091	1091	1091	1091	1090	1091	1090

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A4: Full regression output for the nine composites of the treatment effects figure, with the pre-registered controls. All models include occupation fixed effects, which are not shown. The last three columns are the metaperceptions, what respondents think similar workers believe.

	Interest in joining	General support	Material benefits	Efficacy	Feasibility	Costly action	Perc. image	Perc. interest	Perc. costly action
Intercept	44.42***	58.71***	48.27***	54.72***	48.03***	37.54***	60.01***	48.32***	45.26***
	(6.17)	(4.67)	(5.51)	(5.06)	(5.81)	(6.21)	(4.60)	(5.45)	(5.34)
Treatment	6.97***	3.32*	6.53***	3.00*	4.07*	2.80	3.69**	6.68***	3.48*
	(1.74)	(1.32)	(1.56)	(1.43)	(1.64)	(1.75)	(1.30)	(1.54)	(1.51)
Age	−0.55***	−0.33***	−0.36***	−0.25***	−0.37***	−0.44***	−0.26***	−0.35***	−0.31***
	(0.07)	(0.05)	(0.06)	(0.05)	(0.06)	(0.07)	(0.05)	(0.06)	(0.06)
Female	1.20	1.03	1.75	0.35	3.24	0.06	2.07	2.46	1.40
	(1.83)	(1.38)	(1.63)	(1.50)	(1.72)	(1.84)	(1.36)	(1.61)	(1.58)
Other gender	2.68	4.17	5.23	6.54	2.75	11.66	−2.03	−3.13	−0.06
	(8.88)	(6.72)	(7.93)	(7.27)	(8.35)	(8.93)	(6.62)	(7.84)	(7.68)
Asian	5.13	0.46	3.96	0.17	−1.79	2.31	4.01	6.31	3.07
	(3.82)	(2.89)	(3.41)	(3.13)	(3.59)	(3.84)	(2.84)	(3.37)	(3.30)
Black	7.97**	2.91	7.54**	9.94***	11.98***	6.20*	6.46**	10.54***	8.41***
	(2.69)	(2.04)	(2.40)	(2.21)	(2.53)	(2.71)	(2.01)	(2.38)	(2.33)
Latino	2.09	0.94	3.67	7.85*	9.24*	2.92	4.66	5.86	4.95
	(4.50)	(3.41)	(4.02)	(3.69)	(4.24)	(4.53)	(3.36)	(3.98)	(3.89)
Multiple races	1.93	1.43	3.61	1.12	2.43	2.81	−0.14	0.97	3.20
	(3.44)	(2.60)	(3.07)	(2.82)	(3.23)	(3.46)	(2.56)	(3.04)	(2.97)
Other race	13.94	5.40	7.41	−2.45	−3.50	8.23	10.70	11.71	10.49
	(8.36)	(6.34)	(7.47)	(6.85)	(7.87)	(8.42)	(6.24)	(7.39)	(7.24)
BA or higher	3.20	5.96**	3.60	−0.60	−0.84	3.14	2.05	0.62	−2.55
	(3.04)	(2.30)	(2.71)	(2.49)	(2.86)	(3.06)	(2.27)	(2.68)	(2.64)
Some college	4.52	5.26*	3.69	−1.56	−1.11	4.01	1.37	1.88	−0.27
	(3.03)	(2.30)	(2.71)	(2.48)	(2.85)	(3.05)	(2.26)	(2.68)	(2.63)
Income	−0.14	−0.21	−0.28	0.13	−0.14	−0.32	−0.19	−0.30	−0.06
	(0.27)	(0.20)	(0.24)	(0.22)	(0.25)	(0.27)	(0.20)	(0.24)	(0.23)
Independent	−6.90***	−7.10***	−6.33***	−2.72	−3.37	−7.31***	−2.70	−3.70*	−3.49
	(2.06)	(1.56)	(1.84)	(1.69)	(1.94)	(2.07)	(1.54)	(1.82)	(1.78)
Republican	−16.18***	−20.04***	−15.42***	−7.83***	−4.77*	−12.03***	−9.51***	−8.41***	−4.89*
	(2.40)	(1.82)	(2.15)	(1.97)	(2.26)	(2.42)	(1.79)	(2.12)	(2.08)
Empathy	0.36***	0.37***	0.33***	0.23***	0.19***	0.32***	0.20***	0.21***	0.17***
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Union member	20.06***	11.89***	15.67***	12.21***	21.62***	16.38***	10.43***	12.86***	9.30***
	(2.12)	(1.60)	(1.89)	(1.73)	(1.99)	(2.13)	(1.58)	(1.87)	(1.83)
R^2	0.26	0.32	0.25	0.17	0.19	0.19	0.17	0.18	0.12
Adj. R^2	0.25	0.30	0.23	0.15	0.17	0.18	0.15	0.16	0.10
Num. obs.	1090	1091	1091	1091	1091	1091	1090	1091	1090

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

D Example of treatment article

THE CORNELL DAILY SUN

Cayuga Medical Center Nurses Approve Union Representation in Landslide Vote



In an overwhelming 82% vote, registered nurses at Cayuga Medical Center won union representation on Thursday afternoon, according to a Communications Workers of American Union press release.

The victory comes amid a surge in labor organizations surrounding healthcare workers safety and patient concerns. In New York City, nearly 15,000 nurses participated in a strike, demanding adequate staffing and increased pay. The strike impacted some of New York City's leading medical institutions including Mount Sinai Hospital.

"We've been delivering quality patient care for years while feeling unsupported and burnt out, and I'm excited to see what we can do with our union behind us," said Shane Snyder, an in-patient wound registered nurse in the CWA press release. "We are tenacious, we are strong, and we are ready to start bargaining our contract."

Cayuga United-CWA is fighting for safe staffing levels to support lower patient to nurse ratios and bargaining for a union contract. With union representation secured, the union plans to begin negotiations regarding their concerns.

"**We are laying the groundwork for the generations of nurses to come after us, and we're already hearing from other medical professionals inspired to join us**," said Morgan Downing, a cardiac catheterization RN in the press release. "I am so excited to have my seat at the table alongside my fellow nurses to start bargaining for our first contract."

The nurses have spent the last ten years attempting to unionize.

"CMC nurses have overwhelmingly and repeatedly voiced their urgent desire for a union contract and it is time for management to join us at the bargaining table and get to work improving conditions for nurses and their patients," Trainor said.

E Survey outcome measures

Own beliefs. Respondents rated agreement on a 0–100 slider ("On a scale from 0 to 100, do you strongly disagree (0), neither agree nor disagree (50), or strongly agree (100) with the following statements:").

Interest in joining

- I am interested in joining a labor union.
- If there was an effort to unionize at my workplace, I would support it.
- I hope that my workplace becomes unionized.

General support

- I support labor unions.
- I would personally like labor unions in the U.S. to have more influence.
- It is important that labor unions exist.

Material benefits

- I would benefit from joining a union.
- Most people would benefit from joining a union.
- Most people would be better off if they were a member of a union.

Efficacy

- Workers like me have the power to improve their own working conditions.
- If workers like me can stand together, we can improve our working conditions.

Feasibility

- Workers in my type of job could successfully form a union if they wanted to.
- Unionizing is achievable for people who do the kind of work I do.
- If workers in my field tried to organize a union, they would have a realistic chance of succeeding.

Costly action

- If there was an effort to unionize at my workplace, I would help organize it.
- If there was a strike at my workplace, I would take part.

Perceived beliefs of similar workers. Respondents were asked to guess how an average person in a job like their own would respond ("We'd like you to guess how an average person in a job like your own would respond to the following questions. Please give your best guess. On a scale from 0 to 100, do you think the average person in a job like yours would strongly disagree (0), neither agree nor disagree (50), or strongly agree (100) with the following statements:").

Union image

- "I support labor unions."
- "I would personally like labor unions in the U.S. to have more influence."
- "It is important that labor unions exist."

Interest in joining

- "I am interested in joining a labor union."
- "I hope that my workplace becomes unionized."
- "If there was an effort to unionize at my workplace, I would support it."

Costly action

- "If there was an effort to unionize at my workplace, I would help organize it."
- "If there was a strike at my workplace, I would take part."