Winning is Contagious: Social Learning and the Dynamics of Union Support*

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Abstract

Union growth has historically happened in spurts. We combine union election data from 1961–2025 with a large-scale information experiment involving 9,000 low-wage workers to study the role of social learning in union growth. Observationally, close election wins cause future wins within state-industries. Experimentally, information about recent union wins increases union support and beliefs about co-worker support, with effects concentrated among workers with weak information environments and low baseline coworker support. In contrast, information about labor market tightness has little effect. Our results highlight social learning as a mechanism for union growth.

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1 Introduction

While private sector union decline in the United States has been virtually continuous since the mid-twentieth century, most of its growth occurred in a single, decade-long "spurt" following the Wagner Act (Farber et al., 2021). Similar bursts of union growth can be observed in other OECD countries as well (Freeman, 1997). This pattern suggests that union growth is sudden and episodic rather than a steady, linear accumulation of organizing successes, resembling rapid diffusion processes seen in many other contexts.[1]

Although economists have long posited that economic conditions like unemployment may explain aggregate variation in union density (Ashenfelter and Pencavel, 1969; Dinlersoz and Greenwood, 2016; Farber and Western, 2016), a competing explanation is social learning—individuals updating their beliefs about the payoffs to an action after observing others' behavior and outcomes. Both economics and sociology have emphasized social learning as a mechanism that can generate sharp, self-reinforcing increases in participation (Young, 2009; Givan et al., 2010). In the union context, this explanation suggests that observing successful organizing efforts at other workplaces may increase workers' own willingness to support unionization, potentially triggering waves of activity.

This paper provides new evidence on the role of social learning in driving union growth. We begin with a descriptive fact: using elections data from the recent spurt of Starbucks unionization, we show that election wins rise sharply with initial wins. This autocorrelation is consistent with social learning, but it could also reflect other forces, such as sustained organizing effort following an initial win.

To establish a causal effect of wins on future wins, we leverage close elections in the universe of NLRB elections from 1961 to 2025, instrumenting past union wins with close wins within state-industry cells. This aggregate regression discontinuity design, following Borusyak and Kolerman-Shemer (2024), shows that one additional win generates between 0.28 and 0.69 additional wins over the subsequent three years.

While this design establishes that union wins are *contagious*, it cannot distinguish between demand-side channels—shifts in workers' beliefs about union success or the benefits or costs from unionization—and supply-side channels, such as unions reallocating organizing effort or firms adjusting opposition tactics. To isolate the role of social learning as a mechanism, we design and run an information experiment with 9,089 employed retail and service-sector workers, a group at the center of the recent wave of union organizing in the U.S. The experiment compares the effects of information about recent union successes with information about local labor market conditions. We show that information about recent union activity and wins at other workplaces increase workers' willingness to support unionization for themselves. In contrast, information about economic conditions generates significantly smaller, more heterogeneous impacts on union support.

Participants are recruited online and randomly assigned to one of three treatments or to a control condition. The *Tightness* treatment shows respondents the average time it takes an unemployed worker to find a new job in the retail/service sector in their state; the *Union*

[1] See e.g., Conley and Udry (2010); García-Jimeno et al. (2018); Cantoni et al. (2024); Munshi (2004); Banerjee et al. (2013); Breza and Chandrasekhar (2019).

treatment highlights a recent increase in union formation in the retail and service sectors; and the *Combined* treatment provides both. The control group receives information on national employment levels. Cross-randomizing information about labor market conditions lets us disentangle social learning from purely economic considerations.

Using beliefs data collected before and after treatment, we first establish that the treatments shifted beliefs about union activity and labor market conditions. The vast majority of respondents (84%) underestimated union activity in the retail and service sectors in 2022–23. The *Union* treatment increases the estimated number of elections in the following year by 90% ($p<0.01$), with gains concentrated among those who underestimated activity at baseline. The *Tightness* treatment shifts beliefs about the ease of finding a new job by respondents' baseline priors: those who underestimated unemployment duration update negatively, while those who overestimated it update positively.

We then examine stated union support, focusing on the respondents who underestimated union activity at baseline. Information about recent union activity significantly increases workers' willingness to support unionization, while information about labor market tightness has little effect. The *Union* and *Combined* treatments raise an index of union support by 0.09–0.13 standard deviations ($p<0.01$), whereas the *Tightness* treatment alone generates smaller and statistically weaker effects (0.04 s.d., $p=0.16$). There are striking differences in how the treatments shape perceptions of coworker support for unionization and the private economic returns to unionization. The union information significantly increases perceptions of coworker support as well as the perceived benefits from forming a union, while the tightness information only increases the latter. Consistent with social learning, the effect of union information is concentrated among respondents previously unaware of organizing at their workplace and those expecting low coworker support, providing direct evidence that information about others' union activity shifts beliefs and increases willingness to support unions.

This paper contributes to a recent literature examining the post-COVID uptick in union interest in the United States, which joins a large literature on determinants of unionization, surveyed in Jäger et al. (2025). Our findings directly complement Pezold et al. (2023), who document a limited empirical relationship between labor market tightness and union organizing. By cross-randomizing tightness and union activity information, we show experimentally that social learning shifts union support considerably more than information about local economic conditions. We also build on recent evidence of localized organizing spillovers by Schaller et al. (2026), who find that each successful Starbucks union election leads to five additional union elections in the same county.[2] Studying a recent, relatively high-wage sector's strike, Biasi et al. (2025) show that wage inequality negatively affects high-wage workers' willingness to support unions. Our paper both identifies a causal effect of wins on future wins using six decades of NLRB election data, and our experiment isolates the social learning channel, directly testing a mechanism that observational spillover patterns cannot distinguish.

In focusing on social learning, we also contribute to a broader literature on the role of information in collective action (Cantoni et al., 2024; Deryugina, 2013; Deryugina and

[2] Holmes (2006) provides related evidence of geographic spillovers, showing that establishments in health care and retail are more likely to be unionized in the 1990s if they are located near coal mines and steel mills that were heavily unionized in the 1950s.

Shurchkov, 2016). Several studies have examined the effects of information diffusion on protest attendance using variation in access to communications or transportation infrastructure (García-Jimeno et al., 2018; Enikolopov et al., 2020). These papers find that greater exposure to information leads to more protest activity, suggesting that learning about others protesting increases one's willingness to engage. We similarly find that information about unionization increases both the perceived benefits of unionizing and beliefs about others' support for unions. If a critical mass is needed to unionize, information may push groups over the threshold.[3]

2 Observational Evidence on Union Growth Dynamics

2.1 Context: The Recent Spurt of Union Activity

Following the COVID pandemic, there was a highly publicized wave of worker organizing across the U.S.[4] Workers at prominent non-unionized retailers and service establishments—from Starbucks to Amazon to Apple—held union elections for the first time, reflecting a broader shift: Figure 1a shows a sharp increase in NLRB-held union representation elections in the retail and service sectors in 2022 and 2023, relative to both the prior five years and to trends in other sectors.

News reports from this surge often highlight how workers were motivated by seeing others organize successfully. For instance, a Starbucks worker in California reported that his interest in organizing began after seeing a TikTok video about the Buffalo union effort (Betts, 2022). Appendix A provides several similar examples. Although anecdotal, these narratives suggest that non-economic factors may play an important role in determining union support.

Alongside this organizing wave, labor market tightness increased dramatically in the post-COVID recovery (Autor et al., 2023), which could affect organizing in two ways. A tight labor market may have supported unionization by strengthening workers' bargaining power. However, tighter labor markets may dampen organizing if workers can simply switch to a better job rather than invest in collective action.[5]

2.2 Descriptive Evidence from Starbucks NLRB elections

A particularly salient case is Starbucks, a large firm with thousands of establishments where initial organizing in Buffalo (Brisack, 2025) quickly propagated nationwide. Figure 1b shows the rapid growth of NLRB election wins, with a sharp increase in early 2022, after which the pace decelerates. Figure 1c directly estimates the relationship between current and lagged wins, controlling for a cubic polynomial in time. The estimated coefficient on lagged wins is 0.75 (s.e. 0.11), indicating a strong positive association.

[3]One theoretical basis for this is the "global games" literature, where public signals determine equilibrium selection in coordination games (Morris and Shin, 2002; De Mesquita, 2010).

[4]Appendix D1 shows mentions of "Union election," "Union win," or "National Labor Relations Board" in top national newspapers, 2018–2025 (Roberts et al., 2021), with spikes beginning in early 2021 and peaking in 2022.

[5]See Appendix A for contemporaneous accounts of both narratives.

This time-series evidence is suggestive but not causal, since increased union organizing efforts can induce auto-correlation in elections. We therefore now turn to a strategy that leverages close elections across six decades of NLRB elections.

2.3 Aggregate Regression Discontinuity Evidence

We use a regression discontinuity design to estimate the effect of a union win on future wins within state-industry cells from 1961 to 2025. We follow Borusyak and Kolerman-Shemer (2024), hereafter BKS, who extend the shift-share instruments literature to aggregate regression discontinuities. The close wins act as a quasi-random "shift," while the employment share of a union election in the cell gives the "shares," combined to generate an instrument for union density. While BKS consider the effect of aggregate union density on wage inequality, we instead look at the effect of a union victory on future union wins in a state-industry cell, instrumenting union wins with close wins. Relative to a standard RD design that would examine the effect of a (close) union win on an establishment's own outcomes, this aggregation estimates whether a win at one workplace causes more wins at *other* workplaces in the same sector and geography. Under the standard RD assumptions, this LATE recovers the causal effect of an additional union win on future wins.

Our estimating equation is

$$Wins_{i,t+k} = \beta^{k}_{OLS} Wins_{i,t} + \delta_i + \delta_t + \epsilon_{i,t} \tag{1}$$

where i denotes state $\times$ industry and t denotes year.[6] $Wins_{i,t+k}$ is the count of union election wins in cell i over the k years after t.

As we are interested in worker collective action, we weight these regressions by the employment shares of each election in a labor market cell. Let $s_{j,i,t}$ be establishment j's share of cell i,t's employment. Employment at the industry-sector-year level is obtained from County Business Patterns (1959–1974) and the QCEW (1975 onward).[7] We estimate equation 1 using employment share weighted wins, defined as $Wins^{sw}_{i,t} = \sum_{j \in i,t} s_{j,i,t} Wins_{j,t}$, rather than raw win counts. The interpretation of the OLS regression is the effect of an employment-share weighted win on subsequent employment-share weighted wins.

To establish a causal relationship, we instrument the employment-share weighted $Wins^{sw}_{i,t}$ with $Z_{i,t}$, which is a weighted sum of close wins at the cell level:

$$Z_{i,t} = \sum_{j \in i,t} s_{j,i,t} CloseWin_j \tag{2}$$

where $CloseWin_j$ indicates a win within a 15% vote margin of the 50% threshold required to win a union. Following BKS, we also control for $S_{it} = \sum_j s_{j,i,t} Close_j$; $SV_{it} = \sum_j s_{j,i,t} Close_j \times VoteShare_j$; and $SV^{+}_{it} = \sum_j s_{j,i,t} CloseWin_j \times VotesShare_j$, which are weighted sums of close

[6] We use coarse, 1-digit industry codes by state to achieve reasonable cell sizes over time and avoid measurement error induced by industry re-classification over time.

[7] Time-varying weights ensure that the contribution of each establishment to the cell-level instrument accurately reflects its size in that state and industry cell in the years prior to the election. However, we also probe robustness to using fixed weights for the entire period.

elections, of vote shares in close elections, and of vote shares in close wins respectively. These terms are the analogs of the RD running variable controls at the aggregate cell level, ensuring that the $CloseWin_j$ variable is conditionally independent of the error term. Overall, our IV specification is:

$$\begin{aligned} Wins^{sw}_{i,t+k} &= \beta^k_{IV} Wins^{sw}_{i,t} + \alpha_1 S_{it} + \alpha_2 SV^+_{it} + \alpha_3 SV_{it} + \delta_i + \delta_t + \epsilon_{i,t} \\ Wins^{sw}_{i,t} &= \gamma Z_{i,t} + \omega_1 S_{it} + \omega_2 SV^+_{it} + \omega_3 SV_{it} + \delta_i + \delta_t + \epsilon_{i,t} \end{aligned}$$

Standard errors are clustered at the state-industry level.[8] We validate the design by looking at negative values of k, testing whether our instrumented close wins predict past organizing activity. Figure 2 shows that pre-period coefficients are statistically indistinguishable from zero across a range of specifications varying bandwidth choices and weighting schemes.

Results: We begin by graphically showing the coefficients from dynamic event-study regressions in Figure 2, plotting $\sum_{k=0}^{K} \beta^k_{IV}$ for $K \in \{-3, ..., 5\}$. Our preferred specification uses a 0.15 bandwidth and time-varying weights constructed as a 5-year trailing average of employment in the five years preceding each election, ensuring that weights are predetermined relative to the election's outcome. We also show a set of other specifications, varying the bandwidth from .10 to .20, and using fixed weights, either the 1975 QCEW or the 1959-1962 CBP from the beginning of our sample. The qualitative pattern is consistent across specifications: 3 years after a close win, we see an increase in share-weighted wins.

The lagged effect is consistent with the time it takes for an increased demand for unions to manifest as an increased number of elections and victories, given the lengthy process of organizing, petition, and election. Table E5 shows the coefficients at $K = 3$ for all of our outcomes and specifications. We find that one additional win leads to between 0.284 and 0.689 extra wins over the next three years. Table E5 and Appendix Figure D2 also present suggestive evidence, varying weighting schemes and bandwidths, that past wins lead to an increase in elections held and a higher vote share for the union, with little detectable effect on the share of elections won.

This evidence points to a causal effect of current union election wins on future wins. However, it cannot shed light on the precise mechanism. The effect may be driven by supply-side channels where unions or firms react to wins by reallocating effort to pro- or anti-union activities. In contrast, demand-side channels, like the social learning channel we are interested in, would require evidence that workers update their beliefs and become more willing to support unions in response to others' wins. We provide direct evidence on this channel through an experiment described in the next section.

[8] Appendix B provides details on variable construction for RDA implementation, while Tables E3 and E4 show summary statistics at the election and state-industry level, respectively.

3 Experimental Design and Data

3.1 Design

We recruited 9,089 employed hourly workers in the retail and service sectors from two platforms: YouGov (n=6,000), and Prolific (n=3,089).[9] These participants were randomly assigned to one of four experimental groups—*Control*, *Tightness*, *Union*, or *Combined*—each involving a short informational video, with content varying across arms as follows (see Appendix C.2 for video scripts).

Control: Participants received information on national employment levels in the United States.

***Tightness*:** Participants received information on their local labor market conditions—namely, the number of months it would take an unemployed person to find a new job in the retail or services sectors in their state of residence.[10]

***Union*:** Participants receive information on the number of union representation elections held from 2017–2023 in the retail and services sector and the overall success rate of these elections. This information shows a large increase in activity in 2022–23 compared to previous years.

***Combined*:** Participants receive both videos from the *Union and Tightness* treatments. The video on recent union activity is shown first, followed by information regarding their local labor market conditions.

At the end of the survey, 65–72% of respondents across groups rated the information as either credible or very credible; only 1–2% considered it not credible at all.

We report randomization checks in Appendix Table E6. Out of 72 pairwise comparisons of baseline covariates across treatment and control groups, only 4 yield statistically significant differences, and joint tests show no systematic imbalance across covariates.

3.2 Framework

To guide our empirical analysis, we provide a simple framework of an individual's decision to support a union. An individual i is willing to support a union if the expected marginal benefit of their support exceeds the marginal cost:

$$[Pr(win|S_i = 1, \widehat{s_{-i}}) - Pr(win|S_i = 0, \widehat{s_{-i}})] \times b_i > c_i \tag{3}$$

[9] YouGov is a commercial survey company and Prolific is an online survey platform.

[10] We computed the average length of time it took an unemployed person to find a job in the retail or services sectors at the state level using Current Population Survey data from the recent past (see Appendix C.1 for details).

where $S_i = \{0, 1\}$ denotes individual support, b_i the perceived benefit from a successful union effort, and c_i the perceived cost. $Pr(win)$ depends on perceptions of average co-worker support, which we denote $\widehat{s_{-i}}$, and an individual's support marginally increases the probability of a win, so that $Pr(win|S_i = 1) > Pr(win|S_i = 0)$. This decision rule highlights that union support is strategic and conditional on expectations about others' behavior.

Our experimental variation targets these determinants directly. The *Tightness* treatment targets perceived private returns (b_i and c_i) by informing workers about local job-finding conditions. The effect on union support is ambiguous: learning the market is worse than perceived could increase c_i (harder to find a new job if laid off while organizing) or reduce b_i if poor conditions weaken bargaining power, while learning it is better could either encourage organizing or simply induce workers to switch jobs instead.

The *Union* treatment targets the probability of winning a union by revealing the recent surge in union activity and success rates, which may update beliefs about coworker support $\widehat{s_{-i}}$. The *Combined* treatment delivers both, allowing us to assess how economic conditions and social learning interact. While the *Tightness* and *Union* treatments primarily target specific parameters, cross-effects are possible: for instance, union information could also affect perceived costs, and tightness information could affect beliefs about coworker support if workers assume others share their information set. We examine these empirically in Section 4.

Finally, how individuals respond to each treatment depends on their prior beliefs. We expect the union information to have the largest effects among those who most underestimate union activity, and the tightness treatment to have the largest effects among those who most misperceive local labor market conditions.

3.3 Survey Data and Outcomes

Before receiving the treatments described in section 3.1, participants completed a baseline survey. This included modules on demographics, social media usage, employment, and beliefs about unionization and labor market conditions.

We specifically elicited prior beliefs directly related to our treatments. For union activity, respondents were asked to estimate the average number of union elections per year in 2022 and 2023 in the retail and services sector.[11] We also asked them to predict the success rate of union elections in the past year. To measure perceptions about labor market tightness, we elicited beliefs about the number of months it would take the respondent personally to find a comparable job if they became unemployed, as well as the same measure for someone similar to them.[12] We incentivized accuracy for beliefs about the number of union elections and the unemployment duration of someone similar to the respondent, with the 10% of respondents

[11] The exact question was: "Between 2017 and 2021, there were 660 union elections on average every year in the service and retail sectors. What do you think is the average number of union elections in a year in the last two years (2022 and 2023) in the service and retail sectors?"

[12] We also collected beliefs about job-finding ease on a 5-point scale (very difficult to very easy). Responses were recorded in 0.5-month intervals and top-coded at 10 months. In the analysis we use respondents' own prior, since this should more directly shape how they update in response to the treatment. Appendix D7 shows that these measures are correlated, though respondents on average expect it would be easier for them than for others to find a new job.

closest to the truth receiving a $1 bonus.

After treatment, we measured belief updating and union support. To capture belief updating, we asked respondents to estimate union elections in the retail and service sectors in 2024 and to rate the ease of finding a comparable job if unemployed.

Our primary outcome is an index of union support combining three measures: interest in discussing unionization with a coworker, likelihood they would do so in the next month, and whether they would vote yes for a union, constructed following Anderson (2008). We also measured perceived coworker support, and indices of perceived benefits and costs of unionizing.

Finally, we implemented behavioral measures of varying intensity—from willingness to pay for union information videos to connecting with a workplace organizer—to assess whether stated belief changes translate into costly action. Figure D9 shows these are strongly correlated with the union support index in the control group.

3.4 Sample Characteristics and Descriptive Evidence

Appendix Table E1 shows sample characteristics. Our sample is broadly representative of retail and service workers, though slightly more educated and lower-income than national figures from the 2023 American Community Survey (ACS).

Baseline beliefs: The vast majority of respondents underestimate union activity: 84% estimate fewer elections than actually occurred in 2022–23 (Appendix Figure D4a). We refer to these individuals as 'union pessimists.' For labor market conditions, 64% of respondents expect shorter unemployment durations than the CPS average for retail and service workers in their state (Appendix Figure D4b), suggesting most believe finding a new job would be easier than it actually is ('labor market optimists').[13] With the cross-randomized design, union pessimists form the focus of our main analysis, where cell sizes are large (Appendix Table E2).[14]

Survey correlates of union support: Consistent with the anecdotal evidence in Section 2.1, willingness to support unions varies systematically with perceived support among coworkers, knowledge of union activity, and social media use. Among control respondents, Appendix Figure D5a shows a strong positive correlation between beliefs about coworker support and own union support. Conditional on perceived coworker support, Panels (b) and (c) show that union interest is significantly higher when a respondent has experienced or heard about organizing activity, further reinforcing the influence of social exposure. Greater social media engagement is also associated with stronger union support (panel d).[15]

[13] About 7% of respondents report a value equal to the CPS average. The median difference between respondents' priors and the CPS measure is -1 month.

[14] Results for the 16% of respondents who overestimate union activity are reported in Appendix Table E12; small cell sizes limit inference for this group.

[15] Respondents could report using Facebook, X/Twitter, Instagram, TikTok, Reddit, Discord, or "Other" platforms.

In contrast, Appendix Figure D6 shows little relationship between labor market perceptions and union support. Neither perceived unemployment duration nor wages predict union support (panels a and b). Strikingly, beliefs about costs such as store closures or layoffs are also uncorrelated with union support (panel d). Only beliefs about the benefits of unionizing, which include non-monetary improvements such as more respect in the workplace, are correlated with union support (panel c).

4 Experimental Results

4.1 First-stage

Following the delivery of the information treatments, respondents were asked to estimate the expected number of union elections in 2024 and the perceived ease of securing a job comparable to their current position in the event of unemployment on a five-point scale.

Our simplest intent-to-treat (ITT) equation is:

$$y_i = \alpha + \beta Tightness_i + \gamma Union_i + \delta Combined_i + \psi_s + \epsilon_i \tag{4}$$

where *Tightness*, *Union*, and *Combined* are (mutually exclusive) binary indicators for the information treatments and ψ_s is an indicator for the sampling source (Prolific or YouGov).

Since treatment responses may vary by prior beliefs, we interact treatment indicators with p_i, which can either be an indicator for being pessimistic about union activity (p_i^U, underestimating union elections) or labor market tightness (p_i^{LM}, overestimating unemployment duration), and estimate:

$$\begin{aligned} y_i &= \alpha + \beta Tightness_i + \gamma Union_i + \delta Combined_i + \phi p_i \\ &+ \beta'(Tightness_i \times p_i) + \gamma'(Union_i \times p_i) + \delta'(Combined_i \times p_i) \\ &+ \psi_s + \epsilon_i \end{aligned} \tag{5}$$

Table 1 presents results from both specifications.[16] Across treatments, individuals update their beliefs in the expected direction. Respondents assigned to the *Union* and *Combined* treatment predicted substantially more union elections in 2024 than the control group—an 85–90% increase (p<0.01; column 1). This effect is driven by respondents who were pessimistic about union activity at baseline (column 2), while those who initially overestimated union elections revised their expectations downwards.

Turning to the *Tightness* treatment, we find heterogeneous effects on beliefs about the ease of finding a job if unemployed. On average, this information has a negative, but not statistically significant, impact (column 3). However, individuals who initially underestimated unemployment duration update negatively about finding a job by 0.125–0.14 points—a 4–4.5% effect (p<0.01, column 4). Conversely, those who overestimated unemployment duration update differentially by 0.23–0.26 points relative to optimists (7.4–8.4% effect, p<0.01).

Importantly, we find no cross-treatment effects between the individual treatments: the

[16] Appendix Figure D8 shows the first-stage across the full variation in beliefs.

Tightness treatment does not alter expectations about the number of union elections, and the *Union* treatment does not affect beliefs about the ease of finding a job.

4.2 Willingness to Support a Union

Having established belief updating from the information treatments, we now estimate treatment effects on union support in Table 2. We focus on union pessimists—the 84% of respondents who underestimated union activity at baseline—for the remaining analysis.[17] We present ITT estimates (specification 4) alongside estimates interacting treatment indicators with p_i^{LM} (specification 5) to understand how baseline labor market perceptions moderate effects.

We find a striking contrast between the two types of information for our primary outcome, an index of union support. The *Union* and *Combined* treatments raise the support index by 0.09–0.13 standard deviations (column 1, p<0.01), while the *Tightness* treatment has a small, statistically insignificant effect. We reject equality of coefficients between *Tightness* and the other two treatments. Appendix Table E7 shows that this pattern is broadly consistent across each of the three index components.

As expected, effects vary by baseline labor market perceptions. Among labor market optimists—who learn that conditions are worse than expected—the *Tightness* treatment modestly increases union support by 0.056 s.d. (p=0.07, column 2). On the other hand, for labor market pessimists—who learn that conditions are better than expected—the effect dissipates entirely. The interaction term, $T\times$ Pessimist LM, is negative, yielding a small, insignificant net effect. Information about economic conditions thus only moves workers toward unions when it delivers bad news about outside options.

The *Union* and *Combined* treatments show a different pattern: effects are positive across both labor market optimists and pessimists, though attenuated among the latter. Among labor market optimists, the index rises by 0.10-0.15 s.d. (p<0.01, column 2), nearly double the effect of *Tightness*. The *Combined* treatment is somewhat larger than the *Union* treatment alone, though equality cannot be rejected (p=0.13). The net effect remains positive even for labor market pessimists, though it is noisier and only statistically significant for the *Combined* group. Across all comparisons, we can reject equality of coefficients between *Tightness* and the other two treatments (p=0.002–0.125).

These results establish that learning about union wins shifts union support more than information about economic conditions.

4.3 Determinants of Union Support

Our framework highlights three channels through which information might affect union support: perceived coworker support, perceived benefits, and perceived costs. Table 2, columns 3–8, examine treatment effects on each of these determinants to understand *why* union in-

[17] Results for union optimists are reported in Appendix Table E12. Effects are generally opposite in sign to those for union pessimists: the *Tightness* treatment significantly lowers union support by 0.13 s.d. (p<0.01), driven by labor market optimists who learn conditions are worse than expected. Effects of the *Union* and *Combined* treatments are negative but insignificant.

formation shifts support more than tightness information. Each determinant is measured as an index. The coworker support index combines beliefs about the share of coworkers willing to sign union cards and vote to form a union.[18] The benefits index combines agreement with whether unionization would improve pay/benefits and respect at work. The costs index combines agreement with whether unionization would result in reduced hours, dismissal, or workplace closure.

There are striking differences across the treatment arms. The *Tightness* treatment increases perceived benefits (0.06 s.d., p=0.04, column 5), but does not change perceived coworker support or costs (columns 3 and 7). In contrast, the *Union* and *Combined* treatments increase perceived coworker support by 0.076–0.103 s.d. (p<0.01, column 3), and these effects are statistically different from those of the *Tightness* treatment (p<0.01). Information on union activity also raises perceived benefits by a magnitude similar to the *Tightness* treatment, though—like *Tightness*—it does not affect perceived costs. [19] That is, *Tightness* information shifts only *private* returns, while union information shifts both private returns and beliefs about coworker support. Because unionization requires coordination, this lack of movement in coworker support may explain the more muted effects of the *Tightness* treatment on support.

These average effects vary with baseline labor market perceptions. Among labor market optimists, the *Tightness* treatment raises perceived benefits (0.07 s.d., p=0.05, column 6), whereas pessimists show no significant response, consistent with the asymmetric pattern in Section 4.2.

For the *Union* and *Combined* treatments, labor market optimists have significantly higher perceived coworker support (0.12 s.d., p<0.01, column 4) *and* benefits (0.07–0.10 s.d., p<0.05, column 6). Among pessimists, however, the *Union* treatment does not increase coworker support, suggesting that workers who learn the labor market is better than expected may discount the value of collective action. Correcting labor market perceptions via the *Combined* treatment mitigates this dampening effect: pessimists show a smaller decline in coworker support and no significant difference in perceived benefits relative to the *Union* treatment (see $C\times$ Pessimist LM in columns 4 and 6).

The movement in coworker support points to social learning as a central mechanism: workers appear to update their beliefs about others, not just their own returns.

4.4 Heterogeneity in Union Support by Baseline Information

We also consider whether effects are larger in environments where social learning should matter most. Respondents in low union-information environments should be more responsive to union information. Moreover, responses should be stronger where baseline perceptions of coworker support for unionization are weaker and respondents' marginal support is more likely to matter (see section 3.2). Table 3 tests these hypotheses directly by examining whether treatment effects vary by (i) respondents' awareness of organizing activity at their

[18] About 20% of respondents answered "don't know" to these questions, which we recode as the sample mean of the control group. Results are qualitatively similar if we treat them instead as missing values.

[19] Appendix Table E8 shows treatment impacts on each individual component of the three indices, with results following a similar pattern.

own employer (columns 1–2), and (ii) their perceived coworker support at baseline (columns 3–4).[20]

Among labor market optimists unaware of organizing activity at their employer, the *Union* treatment increases support by 0.11 s.d. (p<0.01, column 1), whereas the effect is close to zero for those already aware at baseline (column 2), although we cannot reject equality (p =0.22). Likewise, the *Union* treatment raises support by 0.13 s.d. among respondents who believed less than 75% of their coworkers supported unions at baseline (p<0.01; column 3), compared to a small and insignificant effect for those who believed support was 75% or higher (equality is rejected at p=0.03). The *Combined* treatment shows a similar pattern to the *Union* treatment. These results are consistent with social learning being the key mechanism through which information shapes willingness to unionize.[21]

4.5 Alternative Explanations

Length and style of treatment videos: Given difference in the length and style of the videos (see Appendix C.2), one might be concerned that the effects reflect greater salience/attention in the union video rather than genuine learning. We offer three pieces of evidence to mitigate these concerns.

First, a recall check immediately after viewing found that 92% of respondents in the *Tightness* group correctly recalled the information presented, compared to 81% in the *Union* group.[22] Second, belief updating about union activity is largest among respondents farthest from the truth at baseline, a pattern consistent with information-based learning (Appendix D8). Third, salience or persuasion would predict uniformly larger effects across subgroups—instead, effects are heterogeneous by baseline labor market perceptions (Table 2), inconsistent with either explanation.

Experimenter demand effects: Another concern is that our results reflect experimenter demand or social desirability bias. Two pieces of evidence suggest this cannot fully explain our findings. First, the heterogeneity by baseline beliefs and environments (see Tables 2 and 3) is inconsistent with a simple demand-based explanation, which would imply more uniform effects. Second, we implement a willingness-to-pay behavioral measure asking respondents whether they would pay about $1 of their survey earnings to access the videos they saw. The *Union* treatment increases willingness to pay for union information by 3 p.p. (p = 0.01), with effects concentrated among labor market optimists (Appendix Table E9, columns 1–2). That individuals are willing to take a costly private action suggests that our results cannot be driven by demand effects alone.[23]

[20] Simple ITT estimates are shown in Appendix Table E10.

[21] One confounding source of heterogeneity may be job quality, where workers in better or worse jobs have differential preferences for or information about unions. Appendix Table E11 reports effects above and below the sample median of hourly wages (columns 1–2) and an index of non-wage job amenities (columns 3–4). For both the *Union* and *Combined* treatments, effects are positive across job quality levels, with no statistically significant heterogeneity.

[22] Those who received tightness information were asked to recall unemployment duration in their state; those who received union information were asked to report the average number of union elections in 2022–23.

[23] Appendix Table E9 reports two additional behavioral outcomes: interest in receiving an informational newsletter about workplace organizing and sharing their contact information to connect with a volunteer work-

5 Conclusion

Our findings contribute to understanding why union growth has historically happened in sudden waves rather than gradual, linear increases. We show that past union victories lead to future wins, with social learning serving as a key mechanism: information about recent union successes raises workers' willingness to support unionization by shifting their beliefs about coworker support. Consistent with this mechanism, we document that actual local NLRB wins are positively associated with survey respondents' beliefs about national union activity and union organizing in their own workplace (see Figure D3).

Together, this evidence suggests that economic fundamentals alone cannot explain sharp bursts of organizing, and instead points to a self-reinforcing dynamic in which each win makes the next more likely. Given the systematic underestimation of union activity we document at baseline, high-profile visible wins that reach workers through media and social networks may be especially powerful in shaping union support. But union support is only a necessary, not sufficient, condition for unionization. Understanding how the dynamics of union demand among non-union workers that we document interact with the organizational behavior of existing unions and firms is an important direction for future work.

place organizer. We do not find significant impacts of the *Union* treatment on these two measures, consistent with the treatment shift being insufficient to move actions that may require stronger signals of coworker support. The *Combined* treatment also does not replicate the WTP effect, suggesting that tightness information may dampen willingness to seek out union information even when it increases stated support.

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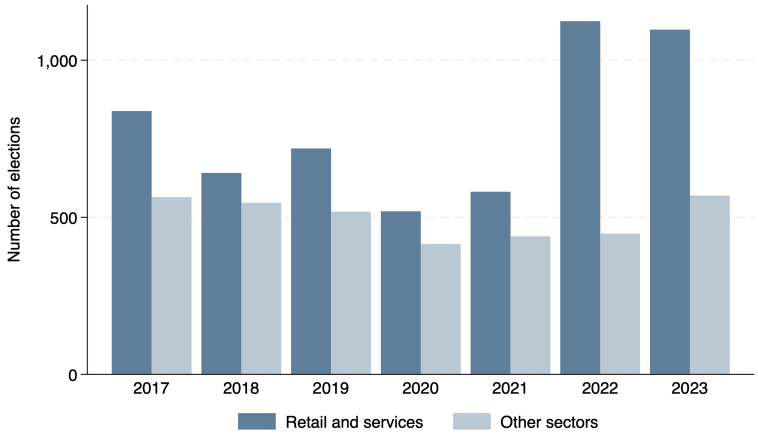
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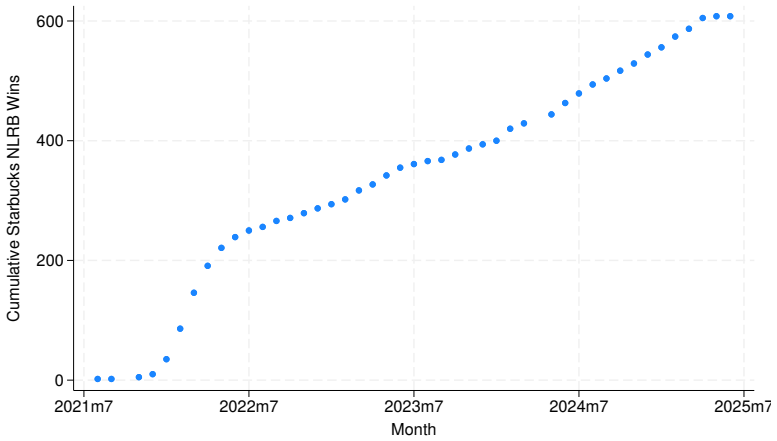
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Figure 1: The Recent Surge in Union Activity

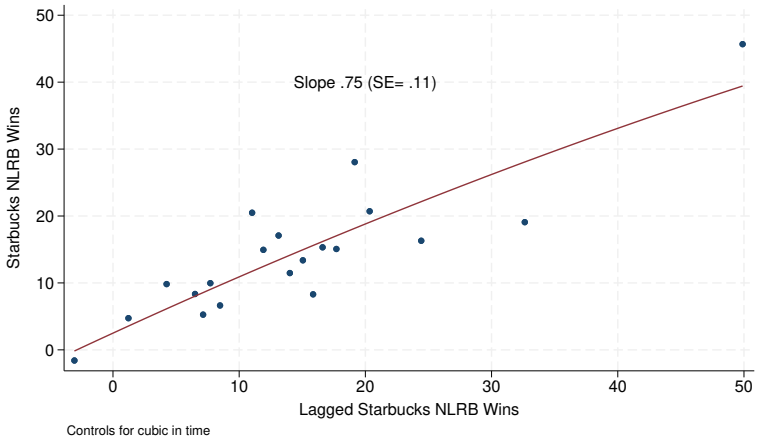
(a) NLRB union elections, 2017–23



(b) Cumulative Starbucks union election wins, 2021–25

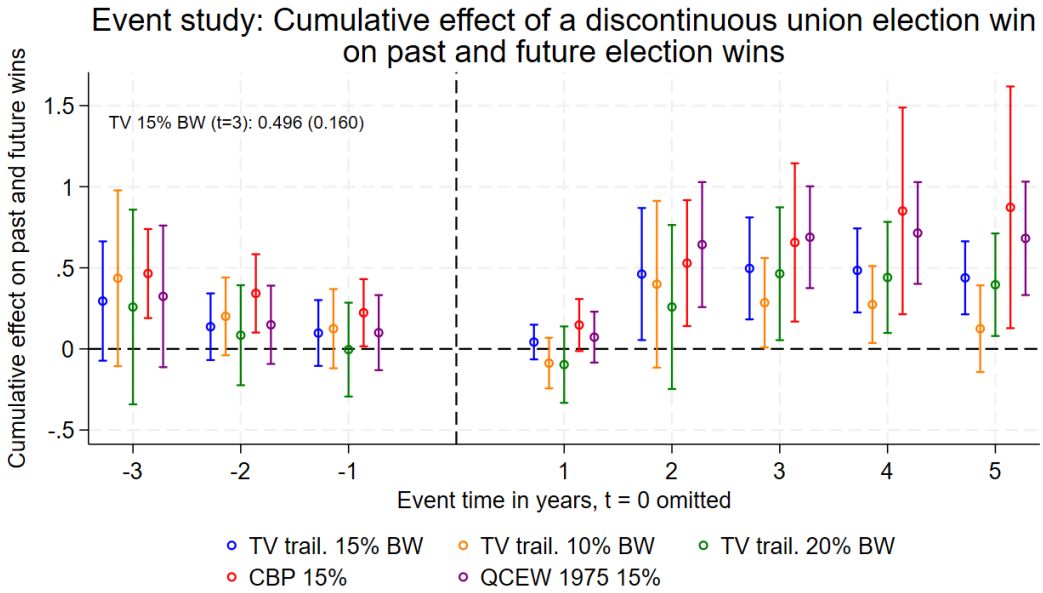


(c) Correlation between wins and lagged wins at Starbucks



Notes: Panel (a) shows the number of NLRB union elections held each year from 2017 to 2023 using data from Bloomberg Law. Panel (b) shows the cumulative number of NLRB union election wins at Starbucks since July 2021. Panel (c) shows the conditional correlation between Starbucks NLRB wins and last month's number of Starbucks NLRB wins, controlling for a cubic polynomial in time.

Figure 2: Aggregate Regression-Discontinuity Estimates of The Effect of Lagged Wins on Future Wins



Notes: This figure presents event-study estimates of the cumulative effect of a union election win on past and future election wins within state×industry cells. The dependent variable is cumulative share-weighted union representation election wins in the state×industry cell in years $t = 0$ to $t + k$ where $k \in [-3, 5]$ (0 is omitted) (see Section 2.3 for details). Each coefficient represents the effect of one additional share-weighted win at $t = 0$ on the number of cumulative wins at $t + k$. We show estimates that vary both the weights used to aggregate election-level variables to the state×industry level and the vote-margin bandwidth considered. Our preferred specification uses a 15% vote-margin bandwidth and time-varying employment weights constructed using a 5-year trailing average (employment from the five years before each election). We also show results using time-varying employment weights for 10% and 20% vote-margin bandwidths. "CBP 15%" uses fixed County Business Patterns weights averaging over 1959-1962 and a 15% vote-margin bandwidth, and "QCEW 1975 15%" uses fixed QCEW weights from 1975 and a 15% vote-margin bandwidth. 95% confidence intervals are shown.
Source: NLRB elections data from 1961-2025 merged to County Business Patterns (CBP) and Quarterly Census of Employment and Wages (QCEW).

Table 1: First-stage effects

	Union elections in 2024 (in 100s)		Ease of finding a job	
	(1)	(2)	(3)	(4)
Tightness (T)	-0.020	-0.501	-0.050	-0.139***
	(0.199)	(0.667)	(0.037)	(0.040)
Union (U)	5.663***	-1.626***	0.002	-0.007
	(0.189)	(0.585)	(0.037)	(0.040)
Combined (C)	5.381***	-1.828***	-0.058	-0.125***
	(0.187)	(0.588)	(0.037)	(0.040)
T × Pessimist Union		0.591		
		(0.682)		
U × Pessimist Union		8.713***		
		(0.608)		
C × Pessimist Union		8.605***		
		(0.610)		
T × Pessimist LM				0.256***
				(0.076)
U × Pessimist LM				-0.029
				(0.075)
C × Pessimist LM				0.233***
				(0.075)
Pessimist Union		-10.999***		
		(0.519)		
Pessimist LM				-1.141***
				(0.053)
p: T=U	0.000	0.031	0.158	0.001
p: T=C	0.000	0.012	0.821	0.737
p: U=C	0.091	0.626	0.101	0.003
p: T+TxP=U+UxP		0.000		0.017
p: T+TxP=C+CxP		0.000		0.885
p: U+UxP=C+CxP		0.082		0.023
N	9031	9028	9046	8898
Control Mean	6.314	6.314	3.095	3.095

Notes: This table shows treatment effects on posterior beliefs. The dependent variables are: the estimated number of union elections in the retail and services sectors in 2024, measured in 100s (columns 1–2); and the ease of finding a new job similar to the current one in the case of unemployment, measured on a 5-point scale where 1 is very difficult and 5 is very easy (columns 3–4). Columns 1 and 3 report simple intent-to-treat estimates, as per specification 4. Columns 2 and 4 fully interact the base specification with an indicator for whether the respondent was a baseline pessimist with regard to (i) union activity in 2022–23 and (ii) labor market conditions (see specification 5). A respondent is considered to be 'Pessimist Union' if their prior belief about union elections in 2022–23 was lower than the truth. A respondent is a 'Pessimist LM' if they believed that the number of months it would take to find a new job similar to their current one under unemployment was greater than the truth. The number of observations vary due to item non-response. The bottom panel reports *p*-values from tests of equality of coefficients between treatment arms.Robust standard errors are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 2: Union Support and its Determinants

			Determinants of Support					
	Union Support Index		Perceived Coworker support		Perceived Benefits		Perceived Costs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tightness (T)	0.035	0.056*	-0.004	0.011	0.060**	0.069**	0.001	0.005
	(0.025)	(0.031)	(0.028)	(0.034)	(0.029)	(0.035)	(0.027)	(0.032)
Union (U)	0.091***	0.103***	0.076***	0.124***	0.081***	0.101***	-0.003	-0.002
	(0.025)	(0.030)	(0.028)	(0.034)	(0.029)	(0.034)	(0.027)	(0.032)
Combined (C)	0.131***	0.148***	0.103***	0.122***	0.076***	0.070**	0.013	0.013
	(0.025)	(0.030)	(0.028)	(0.034)	(0.029)	(0.035)	(0.027)	(0.033)
T × Pessimist LM		-0.076		-0.059		-0.039		0.007
		(0.056)		(0.062)		(0.066)		(0.061)
U × Pessimist LM		-0.039		-0.164***		-0.080		0.007
		(0.055)		(0.062)		(0.065)		(0.062)
C × Pessimist LM		-0.064		-0.070		0.018		0.011
		(0.056)		(0.062)		(0.064)		(0.061)
Pessimist LM		0.066*		0.087**		-0.027		0.042
		(0.039)		(0.044)		(0.045)		(0.044)
p: T=U	0.028	0.125	0.004	0.001	0.460	0.351	0.867	0.828
p: T=C	0.000	0.002	0.000	0.001	0.581	0.982	0.659	0.807
p: U=C	0.110	0.131	0.339	0.945	0.851	0.363	0.545	0.647
p: T+TxP=U+UxP		0.075		0.874		0.879		0.889
p: T+TxP=C+CxP		0.029		0.051		0.302		0.808
p: U+UxP=C+CxP		0.660		0.076		0.232		0.709
N	7588	7458	7635	7499	7587	7457	7586	7456
Control Mean	-0.062	-0.062	-0.041	-0.041	-0.046	-0.046	-0.009	-0.009

Notes: This table reports treatment effects on union support and its determinants—perceived coworker support and perceived benefits and costs of unionization. The sample is restricted to baseline union pessimists. All dependent variables are indices: the union support index is based on three components measuring interest in talking to a coworker who wants to form a union, likelihood of discussing unions with coworkers in the next month, and willingness to vote for a union (columns 1–2); the coworker support index is formed from two variables asking about the share of coworkers that would sign cards for a chance to have a union election and that would vote to form a union, where missing values are recoded to the control mean (columns 3–4); the perceived benefits index is constructed from two variables measuring agreement on a 5-point scale with whether a union would increase pay and benefits and respect at work (columns 5–6); and the perceived costs index is constructed from three variables measuring agreement on a 5-point scale with whether a union would reduce work hours, increase the chance of firing, or shut down the worksite (columns 7–8). Odd-numbered columns report simple ITT estimates, while even-numbered columns report heterogeneous effects by respondents' labor market perceptions. A respondent is classified as a "Pessimist LM" if they overestimated the number of months it would take to find a job comparable to their current one if unemployed. The bottom panel reports *p*-values from tests of equality of coefficients between treatment arms. Robust standard errors are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 3: Effect heterogeneity by baseline information environment

	Dependent variable: Union Support Index			
	Organizing activity at employer		Perceived coworker support	
	(1) No	(2) Yes	(3) Bottom 75%	(4) Top 25%
Tightness (T)	0.042	0.038	0.058*	0.010
	(0.031)	(0.072)	(0.035)	(0.059)
T × Pessimist LM	-0.041	-0.079	-0.081	0.141
	(0.057)	(0.142)	(0.065)	(0.102)
Union (U)	0.108***	0.007	0.129***	-0.018
	(0.030)	(0.076)	(0.035)	(0.056)
U × Pessimist LM	-0.016	0.042	-0.016	0.149
	(0.056)	(0.143)	(0.065)	(0.106)
Combined (C)	0.153***	0.088	0.152***	0.050
	(0.031)	(0.072)	(0.035)	(0.056)
C × Pessimist LM	-0.082	0.064	-0.046	0.112
	(0.057)	(0.133)	(0.067)	(0.102)
Pessimist LM	0.065	-0.076	0.048	-0.128*
	(0.040)	(0.096)	(0.047)	(0.069)
p: T=U	0.035	0.667	0.044	0.625
p: T=C	0.000	0.474	0.007	0.499
p: U=C	0.137	0.267	0.528	0.221
p: T+TxP=U+UxP	0.056	0.485	0.010	0.827
p: T+TxP=C+CxP	0.149	0.108	0.020	0.905
p: U+UxP=C+CxP	0.669	0.387	0.887	0.744
N	6324	1134	4906	1553
Control Mean	-0.178	0.617	-0.174	0.541

Notes: This table reports effect heterogeneity by respondents' baseline information environment. Sample is restricted to baseline union pessimists. The dependent variable is the union support index. Heterogeneity is shown along two pre-treatment covariates: (i) awareness of organizing activity at the respondent's employer (columns 1–2); and (ii) perceived coworker support for a union, split into the bottom 75% and top 25% of the distribution (columns 3–4). A respondent is classified as a "Pessimist LM" if they overestimated the number of months it would take to find a job comparable to their current one if unemployed. The bottom panel reports *p*-values from tests of equality of coefficients between treatment arms. Robust standard errors are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

ONLINE APPENDIX

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A Qualitative Reports About Drivers of Union Activity

We provide a list of quotes from news reports and press releases that highlight the role of (i) social spillovers in union activity and (ii) labor market conditions in potentially shaping organizing and union activity.

Union activity

> (1) "When we see young people leading this movement, a lot of other young people say, 'Hey, this person is my age... and if this person could lead a labor movement at their store and get an election at their store, why can't I at my store?"' (Betts, 2022)

> (2) "After seeing Verizon workers in Washington win a union, we felt inspired and motivated to demand a voice and fight for the respect and dignity we deserve." (NABET-CWA Local 44, 2022)

> (3) "Starbucks employees around the country say they're seeing successful union votes at other locations and thinking they could improve conditions at their own stores by doing the same." (Molla, 2022)

> (4) "We definitely have seen what Starbucks has done—it's been incredibly inspiring—and Amazon as well." (Gibson, 2022)

Labor market tightness

> (5) "'OK, so what if Chipotle closes down?' They were like, 'Oh I'd just get a job at Qdoba, they're hiring for $14 an hour.' I think it's a tight labor market that is giving people a lot of power right now." (Furman, 2022)

> (6) "One is that we have historically a very tight labor market right now. [...] And this means that workers have a lot of leverage with their employers right now, which makes it more likely that they feel willing at this moment to sort of take risks, to demand more, because they know, if they get fired, that they can go out and get another job." (PBS NewsHour, 2023)

B RDA: Data Construction and Estimation

B.1 Election sample

Union election records combine two sources. The core sample is the set of NLRB representation elections from 1962–2009 from Henry Farber and John-Paul Ferguson.[24] We extend coverage through 2025 using representation-election records from Bloomberg Law, keeping cases coded as representation elections with non-missing votes for and against the union. We drop elections outside the fifty states and DC (Guam, Puerto Rico, the U.S. Virgin Islands, and Panama) and records with a missing state. Because a subset of cases appear in both sources, we identify cross-source duplicates by case number and election year and retain a single record per election.

We then impose the restrictions that define the estimation sample. We keep representation (new-organizing) elections and drop those with an incumbent union; we drop elections that cannot be assigned a 1-digit industry, that have missing vote counts, or that have fewer than six eligible voters; and we drop internally inconsistent records in which votes for the union exceed the number of eligible voters. Elections in the nonclassifiable SIC division (division K) and those that cannot be matched to any SIC division are also dropped. These represent less than a tenth of a percent of all elections. Finally, we drop all elections in the public sector (SIC division J) as these were not under the jurisdiction of the NLRA. These elections represent 0.3% of all elections.

For each surviving election j we define the running variable (the win margin) $r_j = \frac{VotesFor_j}{VotesFor_j + VotesAgainst_j} - \frac{1}{2}$, the vote share $VoteShare_j = r_j + \frac{1}{2}$, and a win indicator $Win_j = \mathbf{1}\{VotesFor_j > VotesAgainst_j\}$. An election is close at bandwidth h if $|r_j| < h$; our preferred bandwidth is $h = 0.15$, and we report $h = 0.10$ and $h = 0.20$ as robustness. The close-win instrument at each bandwidth is $CloseWin_j = Win_j \times \mathbf{1}\{|r_j| < h\}$.

B.2 Cells and industry classification

Cells i are defined as state-by-1-digit-industry, and t is the election year. We use 1-digit SIC divisions (A–I) to maintain adequate cell sizes over the six-decade sample and to avoid measurement error induced by changes in industry classification schemes. The elections data contain SIC codes. We used a NAICS-to-SIC-division crosswalk to map NAICS codes available in QCEW after 2000 to SIC divisions.

B.3 State-by-industry employment measures

Each election is weighted by the associated bargaining unit's share of employment in its state-by-industry cell: $s_{j,i,t} = Eligible_j / Emp_{i,t}$, where $Eligible_j$ is the number of eligible voters in election j and $Emp_{i,t}$ is total employment in the establishment's state-by-industry cell in year t. To measure state-by-industry employment each year, we use data from County Business Patterns for the years up to 1974 and from the Quarterly Census of Employment and Wages for the years 1975–2025. To assess sensitivity to the weighting scheme, we construct several employment denominators: time-varying ("TV") weights using employment averaged over the years leading up to an election; average employment from the beginning of the window (1959-1962); and employment from the first year of the QCEW in 1975. We provide more detail on the construction of each of these weights below:

- *Trailing 5-year average employment ("TV trailing").* Our baseline specification uses trailing averages of state-by-industry employment to construct $s_{j,i,t}$ over the five years strictly preceding the election ($t = -5$ to $t = -1$).

[24] The data files were directly provided by Henry Farber.

- *1958-1962 Average Employment ("CBP").* Employment from CBP averaged over 1959–1962.
- *1975 Employment ("QCEW 1975").* 1975 employment in each state-by-1-digit-industry cell from the QCEW.

Time-varying weights allow each establishment's contribution to reflect its size relative to employment in that state and industry cell around the time of the election. We average employment over the five years prior to the election to reduce noise.

B.4 Cell-level aggregation and estimation

Within each cell-year we construct the share-weighted objects used in the analysis: the share-weighted win count $Wins^{sw}_{i,t} = \sum_{j \in i,t} s_{j,i,t} Win_j$; the instrument, share-weighted close wins: $Z_{i,t} = \sum_j s_{j,i,t} CloseWin_j$; and the aggregate running-variable controls, including share-weighted number of close wins $S_{it} = \sum_j s_{j,i,t} Close_j$, share-weighted vote share SV_{it}, and share-weighted win margin SV^+_{it}. We aggregate these share-weighted variables up to the cell $\times$ year level by summing the weighted variables across elections. Cumulative outcomes at horizon k are constructed as running sums of the share-weighted objects over the k years following t.

We estimate the IV specification by two-stage least squares with cell (state-by-industry) and year fixed effects, clustering standard errors at the cell level and instrumenting $Wins^{sw}_{i,t}$ with $Z_{i,t}$, whereas the OLS analog omits the instrument.

C Experimental Details

C.1 Construction of *Tightness* information

We use monthly CPS microdata from October 2023–September 2024 to construct a measure of labor market tightness, which we define as the expected duration of unemployment spells. The construction proceeds in several steps. First, we identify individuals aged 16–64 who are unemployed at the start of the CPS reference period and follow them across months to observe transitions into employment. From this, we construct an indicator for moving from unemployment to employment between adjacent months (the UE transition). Restricting the sample to workers in retail and service industries, we calculate the average UE transition probability at the state level over the 12-month window. The inverse of this probability provides our measure of expected unemployment duration, i.e., the expected number of months to find a job in the retail and services sectors if unemployed. To reduce noise, we round the resulting duration measure to half-month increments. The table below shows the variation in the tightness measure across U.S. states in the experiment.

UE duration (in months)	States
2	Maine
2.5	Delaware, Minnesota, Nebraska, South Dakota, Vermont
3	Arizona, Colorado, Florida, Idaho, Indiana, Michigan, Montana, New Hampshire, Oklahoma, Tennessee, Utah
3.5	Alabama, Alaska, Arkansas, Georgia, Illinois, Kansas, Maryland, Missouri, New Mexico, New York, North Dakota, Ohio, Oregon, Rhode Island, South Carolina, Virginia, Wyoming
4	Iowa, Louisiana, Massachusetts, Nevada, New Jersey, Texas, Washington
4.5	California, Connecticut, Kentucky, Pennsylvania, Wisconsin
5	Mississippi
5.5	North Carolina, West Virginia
6.5	Hawaii
10	District Of Columbia

C.2 Scripts from the information videos

Control: This video had a duration of 12 seconds and followed the script below:

> Did you know? There are currently roughly 158.5 million employed individuals in the U.S."

Information on labor market tightness: This video had a duration of about 16 seconds and followed the script below:

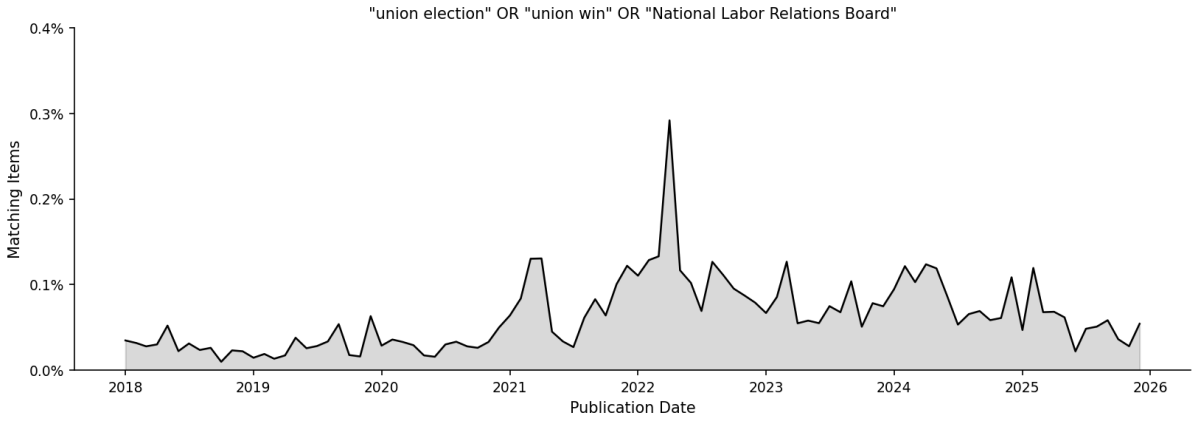
> Did you know? In your state, it usually takes an unemployed person about X months to find a new retail or service sector job that they are willing to work.

Information on union activity: This video had a duration of 1 minute and 3 seconds and followed the script below:

Recently, retail and service workers around the country have been taking action to improve their workplaces—walking out, going on strike, and forming unions. To understand how this moment looks different from the recent past, we looked at the number of union elections followed by workers in the retail and service sectors over the last several years. Between 2017 and 2021, there were 660 union elections each year on average. But in the last two years, the yearly average jumped to 1,110 elections, an increase of nearly 70%. Workers are also winning about 80% of these elections. This dramatic recent increase in union elections comes from across the country. In this moment, retail and service workers—from Walgreens and Starbucks to restaurants and hospitals—are organizing new unions around the country.

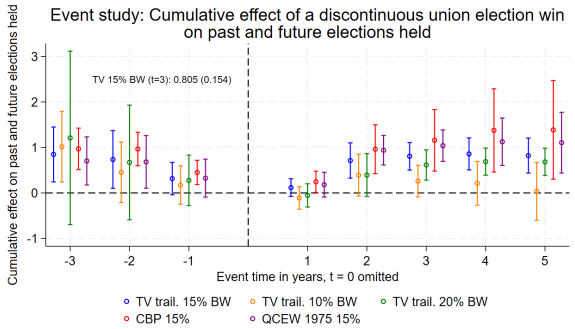
D Additional Figures

Figure D1: Union-related mentions in Top U.S. News Sources

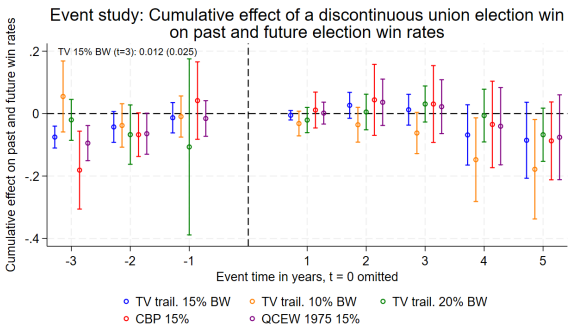


Source: Media Cloud. Access: May 28, 2026

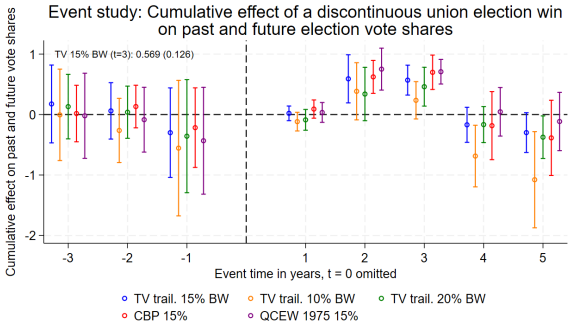
Figure D2: RDA Estimates: Effect of Lagged Wins on Future Elections Held, Win Rate, and Vote Share



(a) Effect of Lagged Wins on Future Elections Held



(b) Effect of Lagged Wins on Win Rate

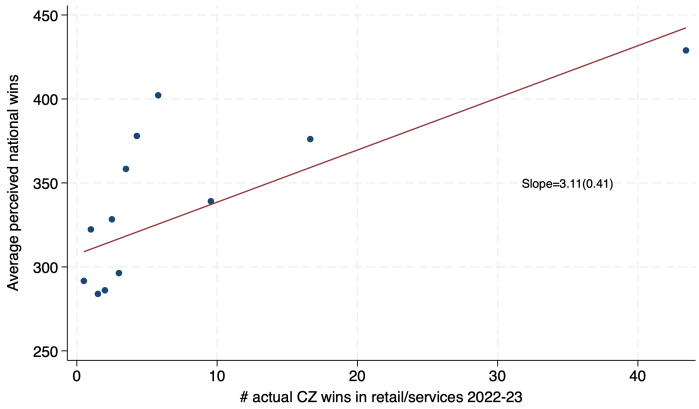


(c) Effect of Lagged Wins on Vote Share

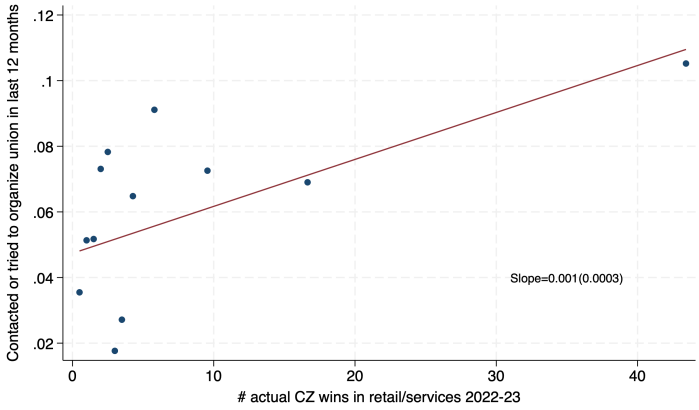
Notes: This figure presents aggregate regression-discontinuity event-study estimates of the cumulative effect of a share-weighted union election win on past and future outcomes within state×industry cells, for three outcomes: the share-weighted number of elections held (panel a), the average share-weighted win rate (panel b), and the share-weighted vote share (panel c) (see Section 2.3 for details on the share weights used). Each coefficient represents the effect of one additional share-weighted win at $t = 0$ on the cumulative share-weighted outcome at $t + k$. We show estimates that vary both the weights used to aggregate election-level variables to the state×industry level and the vote-margin bandwidth considered. Our preferred specification uses a 15% vote-margin bandwidth and time-varying employment weights constructed using a 5-year trailing average (employment from the five years before each election). We also show results using time-varying employment weights for 10% and 20% vote-margin bandwidths. "CBP 15%" uses fixed County Business Patterns weights averaging over 1959-1962 and a 15% vote-margin bandwidth, and "QCEW 1975 15%" uses fixed QCEW weights from 1975 and a 15% vote-margin bandwidth. 95% confidence intervals are shown.
Source: NLRB elections data from 1961-2025 merged to County Business Patterns (CBP) and Quarterly Census of Employment and Wages (QCEW).

Figure D3: Relationship between local union wins and beliefs and demand for unions

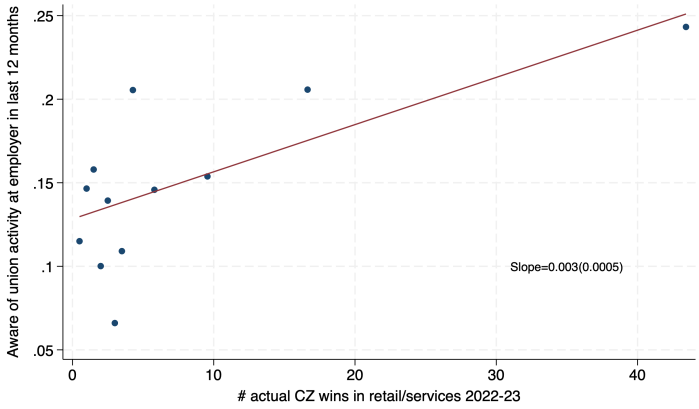
(a) Local wins and national beliefs about union wins (2022–23)



(b) Local wins and union activity at workplace



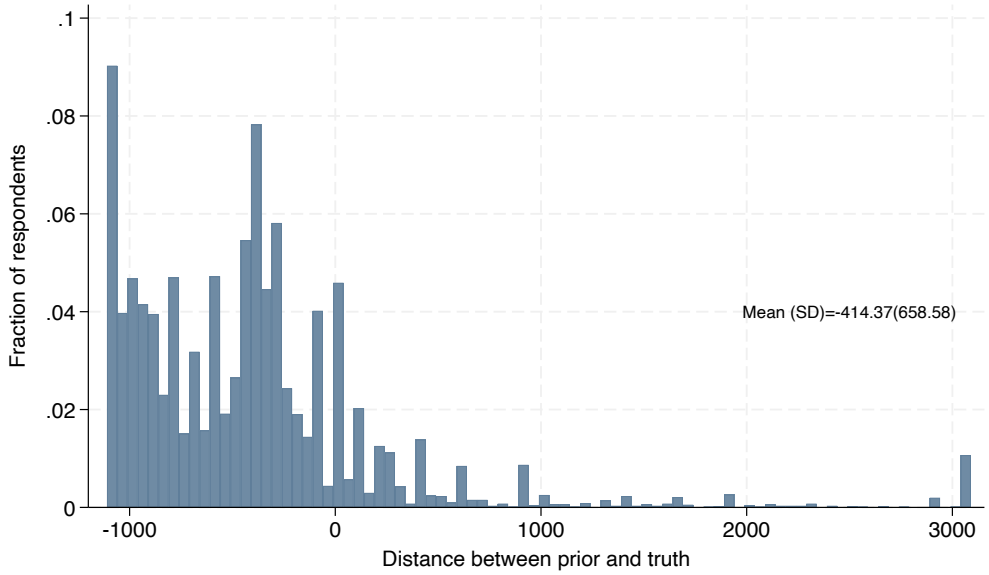
(c) Local wins and awareness of union activity at employer



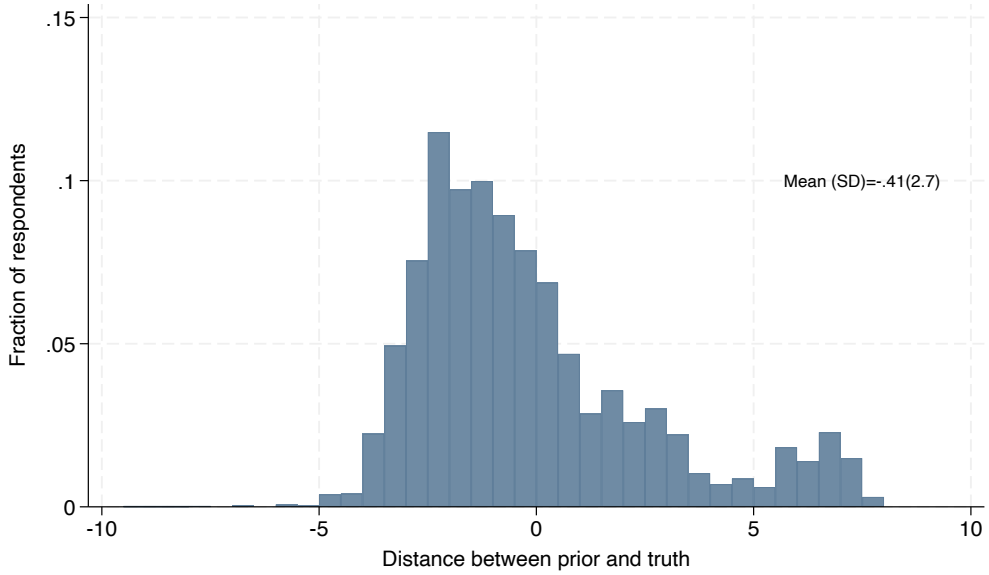
Notes: This figure presents binned scatterplots of the relationship between local NLRB election wins and beliefs about, and engagement with, unions. The x-axis in each panel measures the number of actual NLRB union wins in the retail/services sector across 158 commuting zones (CZ) between 2022–23. Panel (a) plots this against the average perceived number of national union wins in retail and services reported by survey respondents; Panel (b) plots it against the share of respondents who report having been contacted by or having tried to organize a union at their workplace in the last 12 months; and Panel (c) plots it against the share of respondents aware of union activity at their employer in the last 12 months. The sample excludes CZs with 0 wins and is restricted to CZs with at least 5 survey responses.

Figure D4: Distance between prior beliefs and truth

(a) Prior on union elections

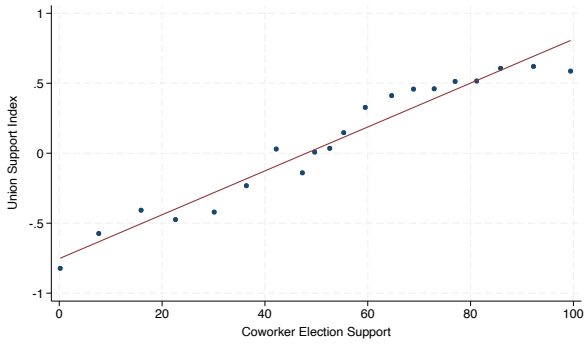


(b) Prior on UE duration

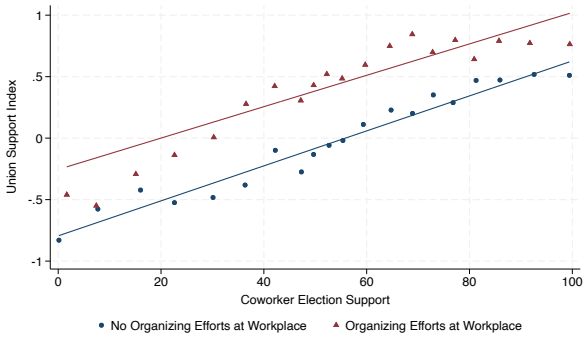


Notes: These figures show the distribution of gaps between respondents' prior beliefs and the truth for our two key treatment variables. Panel (a) shows the gap between each respondent's prior on average annual union elections in 2022–23 and the true value from NLRB data, as obtained through Bloomberg Law. Negative values indicate underestimation of union elections. Panel (b) shows the gap between respondents' prior about the expected number of months it would take to find a comparable job and the CPS average for retail and service workers in their state. Negative values indicate respondents believe finding a job is easier than it actually is, i.e. they underestimate UE duration.

Figure D5: Learning Correlates of Union Support for Control Group



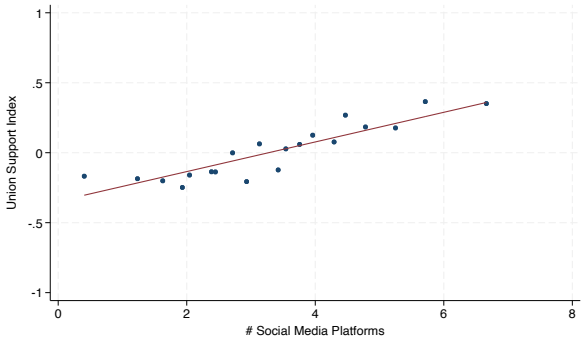
(a) Coworker Support



(b) Knowledge of Organizing in Workplace



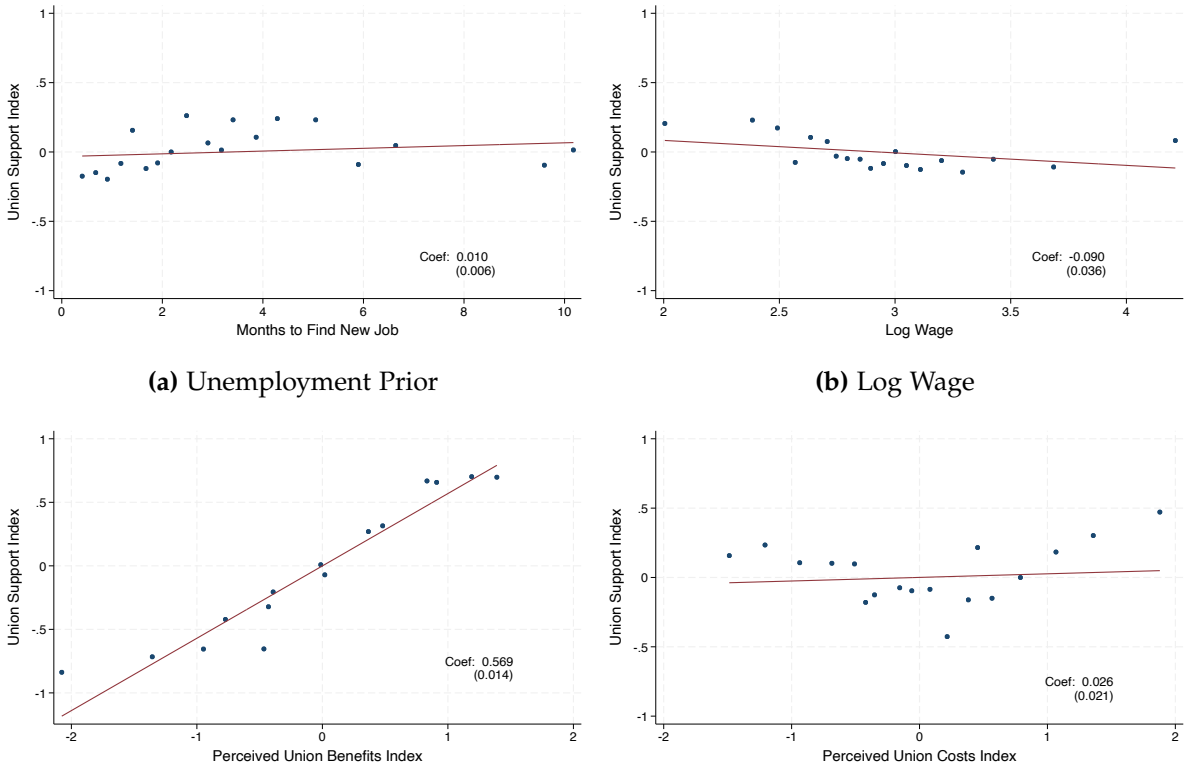
(c) Knowledge of Organizing at Employer



(d) Social Media Use

Notes: These binned scatterplots show the relationship between (control) respondents' own union support and beliefs about coworker union support (panels (a)-(c)) or the number of social media platform respondents use (panel (d)). In panels (b) and (c), the sample is divided by those who know of organizing activity in their own establishment or their employer more broadly, and those who do not know of such activity. Each plot is constructed after controlling for the sampling source and additionally age group bins in panel (d) only.

Figure D6: Economic Correlates of Union Support for Control Group



(a) Unemployment Prior

(b) Log Wage

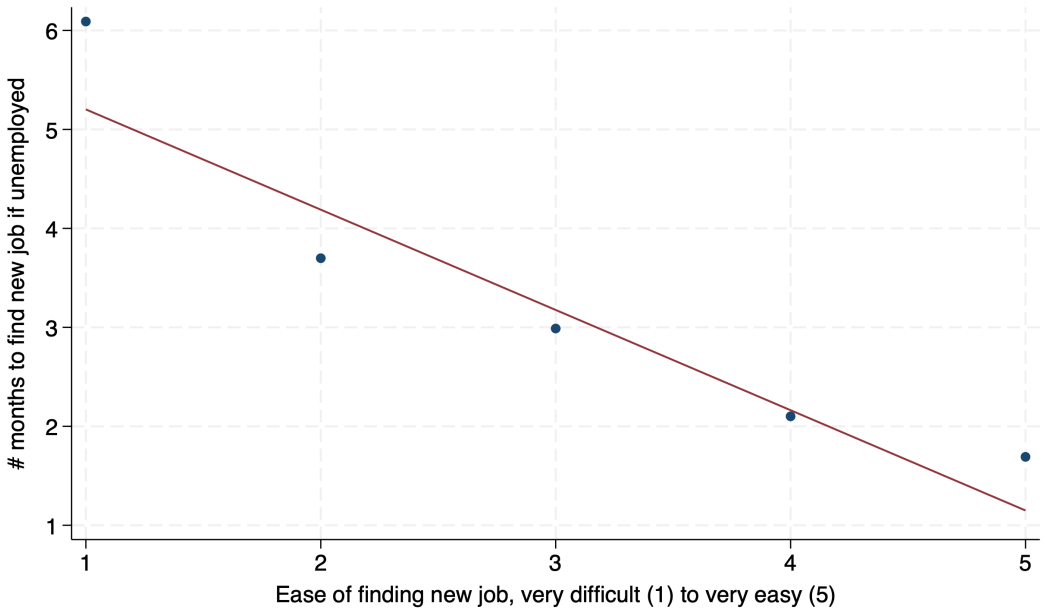
(c) Beliefs about Benefits of Union

(d) Beliefs about Costs of Union

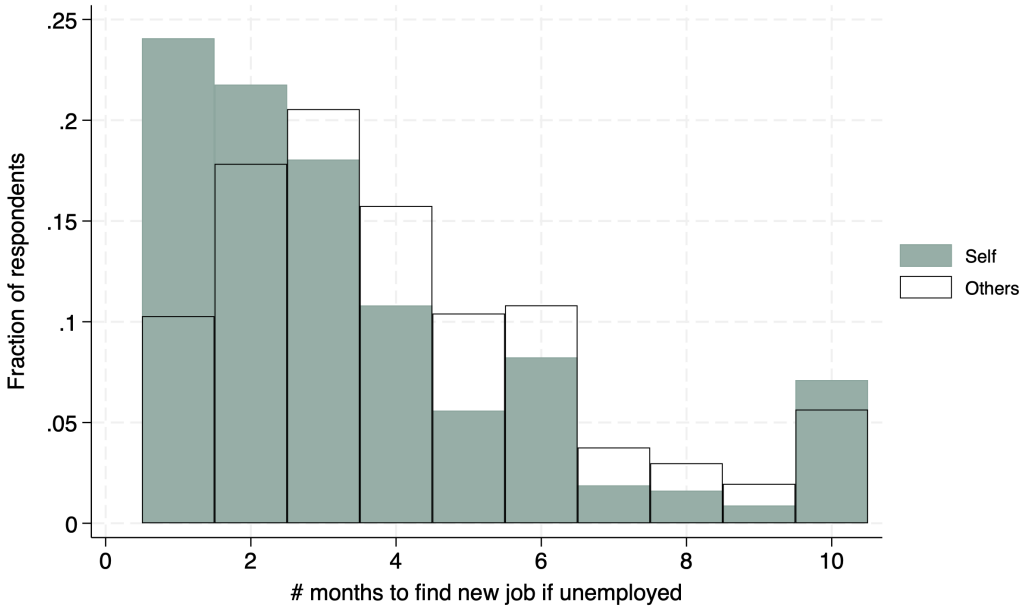
Notes: These binned scatterplots show the relationship between economic variables and an index of union support among control respondents. Panel (a) plots union support against the UE prior variable, defined as expected months to find a comparable job if unemployed. Panel (b) plots union support against the respondent's log hourly wage. Panels (c) and (d) plot union support against indices of perceived benefits and costs of unionizing respectively. The coefficient and robust standard errors from each bivariate regression are reported on the graph. Each plot is constructed after controlling for the sampling source.

Figure D7: Relationship between measures of labor market perceptions

(a) Expected unemployment duration by perceived ease of finding a new job



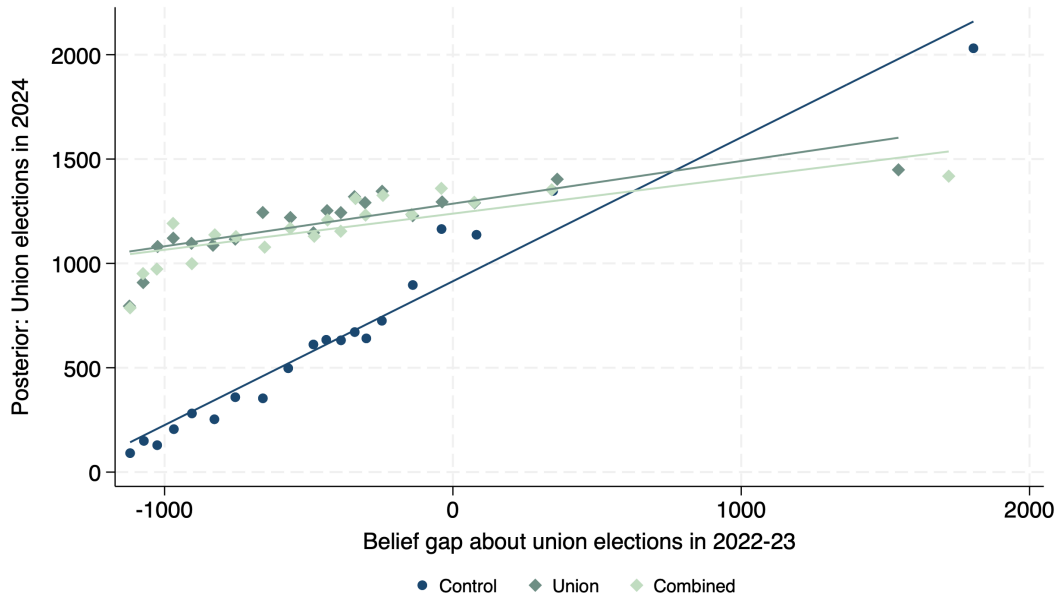
(b) Expected unemployment duration: Self versus others



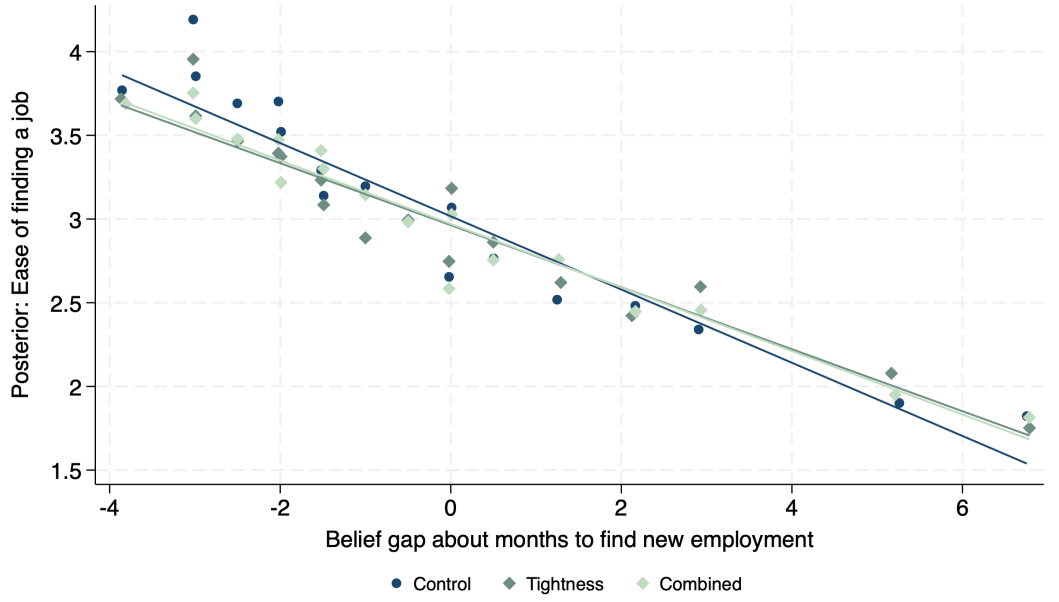
Notes: This figure shows the relationship between the three survey measures of labor market perceptions. Panel (a) presents a binned scatterplot of respondents' prior beliefs about the number of months it would take them to find a new job similar to their current one, plotted against a scale measure of perceived job-finding ease (ranging from 1=very difficult to 5=very easy). Panel (b) compares respondents' beliefs about their own job-finding prospects with their beliefs about "others like them."

Figure D8: First-stage effects

(a) Information about union activity

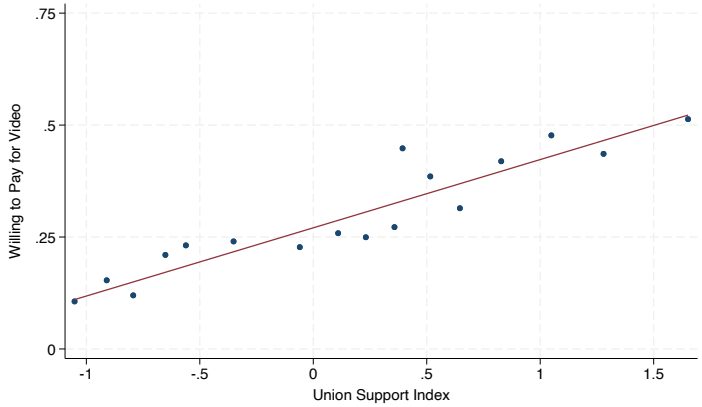


(b) Information about labor market conditions

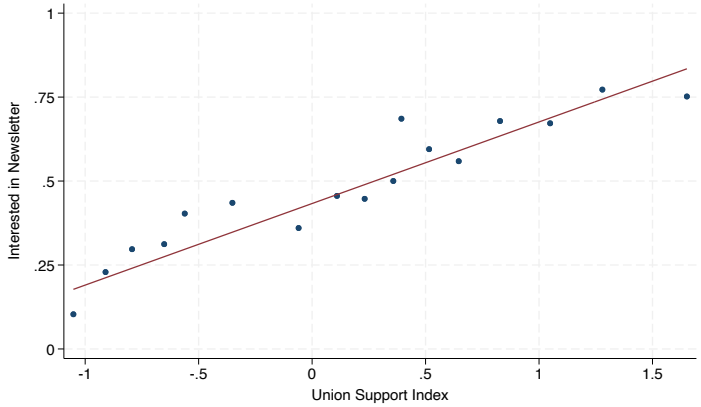


Notes: This figure presents binned scatterplots of treatment effects on posterior beliefs for the two types of information provided in the experiment. Panel (a) shows the effects on beliefs about union elections in 2024, plotted against the belief gap in union elections during 2022–23. Panel (b) shows the effects on reported ease of finding a new job on a 5-pt scale, plotted against the belief gap in the expected number of months to find a new job under unemployment. The belief gap is calculated as the difference between respondents' prior and the true value. Each plot is constructed after controlling for the sampling source.

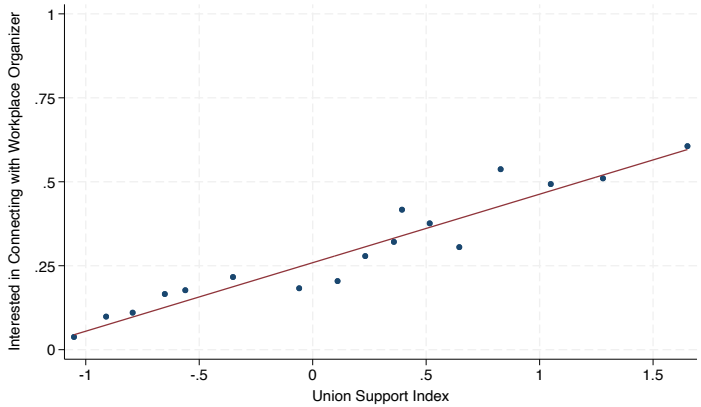
Figure D9: Relationship between union support and behavioral outcomes for control group



(a) Pay for Informational Video



(b) Interested in Newsletter



(c) Interested in connecting with workplace organizer

Notes: This figure shows binned scatterplots of the relationship between an index of union support and behavioral measures from the survey. Sample is restricted to control respondents only. Panel (a) measures willingness to pay for accessing videos seen in the experiment. Panel (b) reports interest in receiving a newsletter about organizing in the workplace. Panel (c) reports interest in connecting with a volunteer workplace organizer.

E Additional Tables

Table E1: Summary statistics

	(1) YouGov Mean	(2) Prolific Mean	(3) Overall Mean	(4) ACS 2023
Gender				
Male	0.47	0.31	0.41	0.42
Female	0.52	0.67	0.57	0.58
Other gender identity	0.01	0.02	0.02	
Race				
White	0.63	0.64	0.63	0.57
Black	0.17	0.14	0.16	0.13
Other	0.19	0.23	0.21	0.29
Age group				
18 to 24	0.13	0.16	0.14	0.16
25 to 39	0.40	0.48	0.43	0.37
40 plus	0.46	0.36	0.43	0.48
Highest education				
High school or less	0.25	0.15	0.22	0.35
Some college/2-yr degree	0.37	0.39	0.38	0.33
College 4-yr degree	0.26	0.33	0.29	0.21
Post-graduate degree	0.12	0.13	0.12	0.11
Family income				
Upto 39,999	0.28	0.25	0.27	0.10
40,000 to 69,999	0.28	0.30	0.29	0.13
70,000 to 119,999	0.27	0.30	0.28	0.22
Above 120,000	0.17	0.15	0.16	0.33
Employment details				
Sector: Retail	0.34	0.37	0.35	0.28
Sector: Services	0.66	0.63	0.65	0.72
Hourly wage	21.33	21.18	21.28	24.51

Notes: This table reports basic demographic information on our sample of hourly, employed workers in the retail and services sectors. Columns 1 and 2 report the means of the variables separately for the YouGov and Prolific samples. Column 3 reports the overall sample mean, combining across the YouGov and Prolific samples. Column 4 shows summary statistics from the 2023 American Community Survey, restricting the ACS sample to individuals aged 18–65 who work part or full-time in the retail or service sectors, defined by industry codes 4070-5790, 7580-7790, 7940-8470, 8561-8670, 8680-8670, and 8780-9290. The hourly wage for the ACS is calculated by taking the average yearly income ($50,985.11) and dividing by 2,080 (40 hours of work per week times 52 weeks).

Table E2: Sample size by baseline misperceptions

	Union optimist	Union pessimist	Total
Labor market optimist	1025	5276	6301
Labor market pessimist	404	2223	2627
Total	1429	7499	8928

Notes: This table shows the available sample sizes by subgroups defined by baseline misperceptions about union activity $\times$ labor market tightness. There are fewer than 9,089 observations due to item non-response for baseline beliefs. A 'union optimist' is defined as an individual whose prior on the number of annual union elections in 2022–23 exceeded the true value (and vice versa for pessimists). A 'labor market optimist' is defined as someone who estimated a shorter-than-actual duration to find a new job in the retail and service sectors under unemployment (and vice versa for pessimists).

Table E3: Summary statistics: RDA Variables

	Mean	SD	N	Min	Max
Win	0.35	0.48	197,288	0.00	1.00
Close	0.54	0.50	197,288	0.00	1.00
Close Win	0.16	0.37	197,288	0.00	1.00
Close(10%)	0.41	0.49	197,288	0.00	1.00
Close (10%) Win	0.10	0.31	197,288	0.00	1.00
Close(20%)	0.66	0.47	197,288	0.00	1.00
Close (20%) Win	0.20	0.40	197,288	0.00	1.00
Vote Share	0.48	0.21	197,288	0.00	1.00
TV Trailing Employment	701.58	891.47	197,288	0.02	8,520.29
CBP Employment	452.80	463.42	197,288	0.15	1,845.13
1975 QCEW Employment	560.15	491.15	197,288	0.00	1,582.43
Eligible Employees	67.45	192.69	197,288	6.00	44,919.00

Notes: This table shows summary statistics at the election level. Employment counts are reported in thousands. Win, Vote Share, and all Close and Close Win variants (10%, 15%, and 20% bandwidths) are weighted by 5-year trailing time-varying employment share. TV Trailing Employment is time-varying employment from the QCEW (CBP before 1975), averaged over the five years before each election. CBP Employment is average County Business Patterns employment from 1959–1962. 1975 QCEW Employment is employment as reported in the 1975 QCEW. Employment counts and eligible employees are unweighted. Vote share defined as $\frac{\text{Votes For}}{\text{Total Votes}}$. A close election is defined as an election decided within a bandwidth of 15% unless otherwise specified.

Table E4: Summary statistics: RDA Variables – State-by-Industry Level

	Mean	SD	N	Min	Max
Win	6.21	11.51	17,929	0.00	136.00
Close	5.43	10.10	17,929	0.00	117.00
Close Win	2.22	4.46	17,929	0.00	58.00
Close(10%)	3.58	6.69	17,929	0.00	77.00
Close (10%) Win	1.38	2.89	17,929	0.00	39.00
Close(20%)	6.95	12.76	17,929	0.00	143.00
Close (20%) Win	2.89	5.71	17,929	0.00	72.00
Vote Share	0.51	0.13	17,925	0.00	1.00
TV Trailing Employment	357.51	608.39	17,929	0.10	8,179.55
Eligible Employees	729.17	1,581.18	17,929	6.00	49,275.00
CBP Employment	181.91	288.05	17,934	0.15	1,845.13
1975 QCEW Employment	248.42	320.23	17,927	0.13	1,582.43

Notes: This table shows summary statistics at the state-industry level. Employment counts are reported in thousands. Win, Vote Share, and all Close and Close Win variants (10%, 15%, and 20% bandwidths) are weighted by 5-year trailing time-varying employment share. TV Trailing Employment is time-varying employment from the QCEW (CBP before 1975), averaged over the five years before each election. CBP Employment is average County Business Patterns employment from 1959–1962. 1975 QCEW Employment is employment as reported in the 1975 QCEW. Employment counts and eligible employees are unweighted. Vote share defined as $\frac{\text{Votes For}}{\text{Total Votes}}$. A close election is defined as an election decided within a bandwidth of 15% unless otherwise specified.

Table E5: RDA Regressions

		RDA				
	OLS	Preferred	QCEW 1975	CBP 1959-62 Avg	10% BW	20% BW
	(1)	(2)	(3)	(4)	(5)	(6)
$Wins_t$	0.158***	0.496***	0.689***	0.656***	0.284**	0.463**
	(0.0553)	(0.160)	(0.160)	(0.249)	(0.141)	(0.209)
N	25,185	17,929	17,927	17,934	17,929	17,929
First-Stage F	.	623.15	846.43	691.91	365.12	972.84
$Elections_t$	0.203*	0.805***	1.040***	1.157***	0.260	0.614***
	(0.105)	(0.154)	(0.176)	(0.345)	(0.177)	(0.169)
N	25,185	17,929	17,927	17,934	17,929	17,929
First-Stage F	.	623.15	846.43	691.91	365.12	972.84
$WinRate_t$	-0.0477***	0.0124	0.0222	0.0307	-0.0622*	0.0308
	(0.0135)	(0.0252)	(0.0440)	(0.0628)	(0.0337)	(0.0295)
N	13,956	12,971	12,971	12,972	12,971	12,971
First-Stage F	.	220.36	199.09	178.96	183.00	355.23
$VoteShare_t$	-0.0383	0.569***	0.708***	0.699***	0.236	0.460***
	(0.0855)	(0.126)	(0.104)	(0.145)	(0.158)	(0.164)
N	13,956	12,971	12,971	12,972	12,971	12,971
First-Stage F	.	220.36	199.09	178.96	183.00	355.23

Notes: This table reports OLS and IV estimates of the cumulative effect of a union election win on future wins, elections held, win rates, and vote shares over the following three years; each panel corresponds to one outcome. Column 1 reports OLS estimates, while columns 2–6 report IV estimates that instrument the employment-share–weighted count of wins ($Wins_{i,t}$) with a weighted sum of close election wins ($\sum_{j \in i,t} s_{j,i,t} CloseWin_j$). Our preferred specification (column 2) uses time-varying employment weights constructed as a 5-year trailing average of CBP and QCEW employment (the five years before each election, years $t-5$ to $t-1$) and a 15% vote-margin bandwidth. Column 3 uses fixed 1975 QCEW weights; column 4 uses fixed County Business Patterns weights averaged over 1959–62; columns 5 and 6 use the preferred time-varying weights but change the vote-margin bandwidth to 10% and 20% respectively. All RDA regressions control for $\sum_j s_{j,i,t} Close_j \times VoteShare_j$, $\sum_j s_{j,i,t} Close_j \times VoteShare_j \times Win_j$, and $\sum_j s_{j,i,t} Close_j$, and include state-industry and year fixed effects. Standard errors, clustered at the state-industry level, are in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table E6: Randomization balance

	Control	Difference			N	*p*-values for tests of equality			
	(1) Mean	(2) T	(3) U	(4) C	(5)	(6) T=U	(7) T=C	(8) U=C	(9) All=0
Female	0.58	-0.01	-0.01	-0.02	9,089	0.94	0.47	0.51	0.62
		(0.02)	(0.02)	(0.01)					
Age, in years	39.65	-0.56	-0.44	-0.89**	9,088	0.76	0.41	0.25	0.16
		(0.40)	(0.40)	(0.40)					
Has 4-year college degree	0.40	0.00	0.01	0.01	9,089	0.53	0.71	0.80	0.81
		(0.02)	(0.02)	(0.01)					
Race: White	0.65	-0.02	-0.02	-0.01	9,089	0.90	0.61	0.52	0.57
		(0.01)	(0.01)	(0.01)					
Sector: Retail	0.35	0.00	0.00	0.01	9,089	0.65	0.37	0.66	0.84
		(0.01)	(0.01)	(0.01)					
Hourly wage	21.45	0.00	-0.32	-0.34	9,069	0.50	0.46	0.95	0.79
		(0.46)	(0.45)	(0.46)					
Job tenure: More than 2 years	0.54	0.01	0.00	0.00	9,089	0.74	0.82	0.91	0.99
		(0.02)	(0.02)	(0.02)					
Establishment size	0.44	-0.01	-0.03**	-0.03**	9,088	0.28	0.23	0.91	0.10
		(0.02)	(0.02)	(0.02)					
Union elections per year (2022–23)	697.05	-3.63	-22.28	17.19	9,083	0.31	0.29	0.04	0.21
		(20.26)	(19.34)	(20.53)					
Expected wage change under union	1.74	0.03	0.07	0.01	9,078	0.50	0.70	0.28	0.64
		(0.06)	(0.06)	(0.06)					
Coworker support for union	52.55	-0.14	0.37	0.76	7,891	0.56	0.30	0.65	0.73
		(0.88)	(0.88)	(0.87)					
# months to find new job if unemployed	3.35	-0.05	-0.07	-0.02	9,087	0.79	0.69	0.51	0.82
		(0.08)	(0.08)	(0.08)					

Notes: This table reports randomization balance using data from the pre-treatment survey modules. Each row presents results from a separate regression. Column 1 reports the mean of the covariate for the control group. Columns 2–4 display coefficients and standard errors from regressions in which the covariate is regressed on mutually exclusive indicators for the *Tightness* (T), *Union* (U), and *Combined* (C) treatment groups. Column 5 reports the number of observations. Columns 6–8 show *p*-values for tests of equality between treatment group coefficients. Column 9 reports the *p*-value from a joint test that all three treatment indicators are jointly equal to zero. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table E7: Index components of union support

	Interested in union		Discuss with coworkers		Vote for union	
	(1)	(2)	(3)	(4)	(5)	(6)
Tightness (T)	0.033	0.049	0.045*	0.050	0.011	0.030
	(0.032)	(0.038)	(0.027)	(0.033)	(0.016)	(0.019)
Union (U)	0.084***	0.092**	0.081***	0.087***	0.046***	0.056***
	(0.031)	(0.037)	(0.027)	(0.033)	(0.016)	(0.019)
Combined (C)	0.126***	0.150***	0.122***	0.119***	0.063***	0.078***
	(0.031)	(0.038)	(0.028)	(0.033)	(0.016)	(0.019)
T × Pessimist LM		-0.058		-0.018		-0.069*
		(0.071)		(0.061)		(0.036)
U × Pessimist LM		-0.017		-0.020		-0.035
		(0.071)		(0.062)		(0.035)
C × Pessimist LM		-0.094		0.007		-0.055
		(0.070)		(0.062)		(0.035)
Pessimist LM		0.114**		-0.007		0.051**
		(0.050)		(0.042)		(0.025)
p: T=U	0.104	0.252	0.201	0.265	0.030	0.188
p: T=C	0.003	0.008	0.007	0.042	0.001	0.013
p: U=C	0.185	0.119	0.153	0.353	0.306	0.246
p: T+TxP=U+UxP		0.163		0.501		0.046
p: T+TxP=C+CxP		0.260		0.077		0.036
p: U+UxP=C+CxP		0.752		0.280		0.944
N	7588	7458	7584	7454	7586	7456
Control Mean	2.214	2.214	1.510	1.510	0.539	0.539

Notes: This table reports treatment effects on individual components of the union support index. Sample is restricted to baseline union pessimists. The dependent variables are three components of the union support index: interest in talking to a coworker who wants to form a union (columns 1–2); likelihood of discussing unions with coworkers in the next month (columns 3–4); and willing to vote yes for a union (columns 5–6). The 4-point scale ranges from 1 ("not at all likely/interested") to 4 ("very likely/interested"). A respondent is classified as a "Pessimist LM" if they overestimated the number of months it would take to find a job comparable to their current one if unemployed. The bottom panel reports *p*-values from tests of equality of coefficients between treatment arms. Robust standard errors are reported in parentheses.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table E8: Index components of determinants of union support

	Coworker support		Perceived benefits		Perceived costs		
	(1) Sign cards	(2) Vote yes	(3) ↑ Pay/benefits	(4) ↑ Respect	(5) ↓ Hours	(6) Firing	(7) Shut down
Panel A: Simple ITT effects							
Tightness (T)	-0.184	-0.088	0.059*	0.074**	-0.034	0.011	0.030
	(0.975)	(0.992)	(0.034)	(0.038)	(0.035)	(0.035)	(0.037)
Union (U)	2.757***	2.395**	0.058*	0.126***	-0.033	0.012	0.012
	(0.975)	(0.995)	(0.034)	(0.037)	(0.035)	(0.035)	(0.037)
Combined (C)	3.365***	3.703***	0.084**	0.084**	-0.006	0.037	0.020
	(0.984)	(1.009)	(0.034)	(0.037)	(0.035)	(0.036)	(0.037)
p: T=U	0.003	0.012	0.990	0.170	0.979	0.969	0.638
p: T=C	0.000	0.000	0.455	0.798	0.426	0.467	0.784
p: U=C	0.537	0.192	0.450	0.261	0.440	0.498	0.847
N	6050	6036	7584	7573	7577	7575	7575
Control Mean	48.401	49.837	3.587	3.294	2.834	2.550	2.613
Panel B: Effects by labor market pessimism							
Tightness (T)	0.015	0.627	0.059	0.097**	-0.016	0.004	0.034
	(1.163)	(1.186)	(0.040)	(0.045)	(0.042)	(0.042)	(0.044)
T × Pessimist LM	-0.914	-2.783	-0.012	-0.079	-0.029	0.032	0.011
	(2.178)	(2.211)	(0.077)	(0.084)	(0.078)	(0.078)	(0.083)
Union (U)	4.110***	4.246***	0.094**	0.133***	-0.033	0.004	0.025
	(1.165)	(1.190)	(0.040)	(0.045)	(0.042)	(0.042)	(0.044)
U × Pessimist LM	-4.706**	-6.240***	-0.130*	-0.043	0.012	0.044	-0.032
	(2.180)	(2.228)	(0.078)	(0.083)	(0.080)	(0.080)	(0.083)
Combined (C)	3.754***	4.532***	0.064	0.094**	-0.006	0.031	0.023
	(1.181)	(1.209)	(0.041)	(0.045)	(0.042)	(0.043)	(0.045)
C × Pessimist LM	-1.550	-2.947	0.070	-0.038	0.012	0.021	0.004
	(2.188)	(2.247)	(0.076)	(0.082)	(0.078)	(0.080)	(0.083)
Pessimist LM	2.169	3.619**	-0.043	-0.014	0.050	0.078	0.027
	(1.544)	(1.587)	(0.054)	(0.058)	(0.056)	(0.056)	(0.059)
p: T=U	0.000	0.002	0.380	0.411	0.688	0.995	0.840
p: T=C	0.002	0.001	0.918	0.950	0.805	0.516	0.812
p: U=C	0.763	0.812	0.445	0.379	0.514	0.519	0.969
p: T+TxP=U+UxP	0.869	0.930	0.221	0.309	0.721	0.857	0.462
p: T+TxP=C+CxP	0.091	0.044	0.189	0.581	0.430	0.802	0.803
p: U+UxP=C+CxP	0.127	0.057	0.011	0.624	0.682	0.945	0.627
N	5947	5932	7454	7443	7447	7446	7445
Control Mean	48.401	49.837	3.587	3.294	2.834	2.550	2.613

Notes: This table reports treatment effects on individual components of the determinants on union support—perceived coworker support, and perceived benefits and costs of unionization. Sample is restricted to baseline union pessimists. Panel A show simple ITT estimates, whereas Panel B shows heterogeneous effects by baseline labor market pessimism. Columns 1–2 consider two measures of coworker support: the share of coworkers willing to (i) sign cards to have a union election or (ii) vote yes to form a union. Columns 3–4 consider benefits from unionization by measuring the respondents' agreement on a 5-point scale with whether a union would increase (i) pay and benefits and (ii) respect at work. Columns 5–7 measure perceived costs of forming a union by assessing respondents' agreement on a 5-point scale with whether a union would (i) reduce work hours, (ii) increase chance of firing, and (iii) shut down worksite. Columns 1–2 have fewer observations due to item non-response. A respondent is classified as a "Pessimist LM" if they overestimated the number of months it would take to find a job comparable to their current one if unemployed. The bottom panel reports *p*-values from tests of equality of coefficients between treatment arms. Robust standard errors are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table E9: Behavioral measures

	WTP>0		Interest in newsletter		Interest in organizer	
	(1)	(2)	(3)	(4)	(5)	(6)
Tightness (T)	0.010	0.003	0.003	0.002	0.008	0.001
	(0.014)	(0.017)	(0.016)	(0.019)	(0.014)	(0.017)
Union (U)	0.030**	0.028	0.011	0.007	0.003	0.001
	(0.014)	(0.017)	(0.016)	(0.019)	(0.014)	(0.017)
Combined (C)	0.009	0.000	0.010	-0.003	0.005	0.001
	(0.014)	(0.017)	(0.016)	(0.019)	(0.014)	(0.017)
T × Pessimist LM		0.012		-0.009		0.016
		(0.031)		(0.035)		(0.031)
U × Pessimist LM		-0.013		0.007		-0.001
		(0.031)		(0.035)		(0.030)
C × Pessimist LM		0.020		0.032		0.009
		(0.031)		(0.035)		(0.030)
Pessimist LM		-0.027		-0.015		-0.015
		(0.022)		(0.025)		(0.021)
p: T=U	0.158	0.140	0.610	0.785	0.720	0.989
p: T=C	0.981	0.887	0.663	0.794	0.879	0.997
p: U=C	0.148	0.106	0.940	0.595	0.836	0.992
p: T+TxP=U+UxP		0.976		0.476		0.534
p: T+TxP=C+CxP		0.839		0.217		0.792
p: U+UxP=C+CxP		0.863		0.607		0.716
N	7584	7454	7584	7454	7581	7452
Control Mean	0.254	0.254	0.409	0.409	0.239	0.239

Notes: This table reports treatment effects on behavioral measures of willingness to engage with unions. Sample is restricted to baseline union pessimists. The dependent variables are: whether the respondent was willing to pay to gain access to videos they saw in the survey (columns 1–2); interest in receiving a newsletter on organizing in the workplace (columns 3–4); and interest in connecting with a volunteer workplace organizer (columns 5–6). Odd-numbered columns report simple ITT estimates, while even-numbered columns report heterogeneous effects by respondents' labor market perceptions. A respondent is classified as a "Pessimist LM" if they overestimated the number of months it would take to find a job comparable to their current one if unemployed. The bottom panel reports *p*-values from tests of equality of coefficients between treatment arms. Robust standard errors are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table E10: Effect heterogeneity by baseline information environment, Simple ITT estimates

	Dependent variable: Union Support Index			
	Organizing activity at employer		Perceived coworker support	
	(1) No	(2) Yes	(3) Bottom 75%	(4) Top 25%
Tightness (T)	0.033	0.021	0.038	0.057
	(0.026)	(0.062)	(0.029)	(0.048)
Union (U)	0.103***	0.019	0.127***	0.031
	(0.025)	(0.064)	(0.029)	(0.047)
Combined (C)	0.129***	0.115*	0.141***	0.092*
	(0.026)	(0.060)	(0.029)	(0.047)
p: T=U	0.006	0.978	0.002	0.597
p: T=C	0.000	0.116	0.000	0.473
p: U=C	0.297	0.121	0.638	0.206
N	6444	1144	5007	1568
Control Mean	-0.178	0.617	-0.174	0.541

Notes: This table reports effect heterogeneity by respondents' baseline information environment. Sample is restricted to baseline union pessimists. The dependent variable is the union support index. Heterogeneity is shown along two pre-treatment covariates: (i) awareness of organizing activity at the respondent's employer (columns 1–2); and (ii) perceived coworker support for a union, split into the bottom 75% and top 25% of the distribution (columns 3–4). A respondent is classified as a "Pessimist LM" if they overestimated the number of months it would take to find a job comparable to their current one if unemployed. The bottom panel reports *p*-values from tests of equality of coefficients between treatment arms. Robust standard errors are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table E11: Effect heterogeneity by current job quality

	Dependent variable: Union support index			
	Job quality: Wage		Job quality: Non-Wage	
	(1) Below p50	(2) Above p50	(3) Below p50	(4) Above p50
Tightness (T)	0.013	0.103**	0.038	0.074*
	(0.043)	(0.044)	(0.042)	(0.044)
T × Pessimist LM	-0.108	-0.074	-0.082	-0.072
	(0.085)	(0.075)	(0.075)	(0.083)
Union (U)	0.061	0.145***	0.111***	0.098**
	(0.042)	(0.044)	(0.041)	(0.044)
U × Pessimist LM	-0.046	-0.061	-0.048	-0.039
	(0.082)	(0.075)	(0.075)	(0.082)
Combined (C)	0.121***	0.182***	0.196***	0.109**
	(0.042)	(0.044)	(0.041)	(0.044)
C × Pessimist LM	-0.088	-0.078	-0.087	-0.058
	(0.083)	(0.076)	(0.074)	(0.085)
Pessimist LM	0.138**	0.041	0.066	0.072
	(0.060)	(0.052)	(0.053)	(0.058)
p: T=U	0.253	0.338	0.081	0.592
p: T=C	0.011	0.072	0.000	0.431
p: U=C	0.149	0.406	0.040	0.799
p: T+TxP=U+UxP	0.120	0.368	0.080	0.431
p: T+TxP=C+CxP	0.079	0.230	0.012	0.523
p: U+UxP=C+CxP	0.799	0.755	0.450	0.915
N	3626	3825	3611	3847
Control Mean	0.011	-0.128	-0.069	-0.054

Notes: This table reports effect heterogeneity by respondents' job quality. Sample is restricted to baseline union pessimists. The dependent variable is the union support index. Heterogeneity is shown along below and above median values of two pre-treatment covariates: (i) hourly wage (columns 1–2); and (ii) an index of non-wage job attributes (columns 3–4). The non-wage attributes include: measures of job satisfaction, friends at work, problems at work, voice on the job, and fair treatment at work. A respondent is classified as a "Pessimist LM" if they overestimated the number of months it would take to find a job comparable to their current one if unemployed. The bottom panel reports *p*-values from tests of equality of coefficients between treatment arms. Robust standard errors are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table E12: Union support and determinants of support, Sample of baseline union optimists

			Determinants of Support					
	Union Support Index		Perceived Coworker support		Perceived Benefits		Perceived Costs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tightness (T)	-0.133**	-0.201***	-0.038	-0.071	-0.138**	-0.210***	-0.010	-0.001
	(0.061)	(0.073)	(0.059)	(0.068)	(0.064)	(0.074)	(0.065)	(0.076)
Union (U)	-0.014	-0.053	0.045	0.019	0.029	-0.023	-0.006	0.031
	(0.060)	(0.071)	(0.058)	(0.066)	(0.064)	(0.073)	(0.064)	(0.074)
Combined (C)	-0.005	-0.043	0.048	0.042	-0.040	-0.109	-0.047	-0.018
	(0.059)	(0.072)	(0.056)	(0.065)	(0.064)	(0.076)	(0.063)	(0.075)
T × Pessimist LM		0.218		0.115		0.194		-0.064
		(0.137)		(0.134)		(0.147)		(0.148)
U × Pessimist LM		0.151		0.126		0.133		-0.170
		(0.138)		(0.140)		(0.150)		(0.152)
C × Pessimist LM		0.122		0.042		0.177		-0.134
		(0.129)		(0.129)		(0.139)		(0.141)
Pessimist LM		-0.165*		-0.079		-0.220**		-0.014
		(0.092)		(0.092)		(0.102)		(0.105)
p: T=U	0.050	0.035	0.166	0.191	0.009	0.011	0.959	0.655
p: T=C	0.033	0.026	0.143	0.099	0.124	0.186	0.547	0.818
p: U=C	0.883	0.877	0.970	0.729	0.272	0.250	0.510	0.491
p: T+TxP=U+UxP		0.517		0.421		0.347		0.577
p: T+TxP=C+CxP		0.584		0.724		0.484		0.466
p: U+UxP=C+CxP		0.876		0.614		0.737		0.917
N	1446	1428	1448	1429	1446	1428	1445	1427
Control Mean	0.317	0.317	0.209	0.209	0.249	0.249	0.058	0.058

Notes: This table reports treatment effects on union support and the three determinants on union support for baseline union *optimists*. Baseline union optimists are individuals who overestimated union activity at baseline. The dependent variables are all indices: Columns 1–2 measure union support; columns 3–4 measure perceived coworker support for unionization; and columns 5–8 measure perceived benefits and costs from unionization. Odd-numbered columns present simple ITT estimates, whereas even-numbered columns present ITT estimates by baseline labor market perceptions. A respondent is classified as a "Pessimist LM" if they overestimated the number of months it would take to find a job comparable to their current one if unemployed. The bottom panel reports *p*-values from tests of equality of coefficients between treatment arms. Robust standard errors are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$