# Social Networks and the Spread of Strikes

Nancy H. Chau * Huiyi Chen† Oleg Firsin‡

August 2026

**Abstract**

This paper examines how labor strikes spread through social networks in the United States. Using the Cornell ILR Labor Action Tracker for 2021–2024 and Facebook-based county connectedness measures, we find that exposure to strikes in socially connected counties is strongly associated with local strike incidence. A one-standard-deviation increase in contemporaneous network exposure is associated with about 16% higher expected strike incidence, while lagged exposure is positive but imprecisely estimated. The relationship is strongest within industries and extends to strike scale, measured by the number of participants. We then test whether strike diffusion operates through information discovery or behavioral responses and find stronger evidence for the behavioral channel.

**Keywords —** Labor strikes, peer learning, social networks, information diffusion

**JEL Codes —** J52, J51, D83, L14, J08

*Charles H. Dyson School of Applied Economics and Management. Cornell University. E-mail: hyc3@cornell.edu.

†Department of Economics. James Madinson University. E-mail: chen9hx@jmu.edu.

‡Department of Economics. University of Maryland Baltimore County. E-mail: ofirsin@umbc.edu. We thank participants at the 2026 Society of Labor Economists (SOLE) Annual Meeting, the 101st Annual Conference of the Western Economic Association International (WEAI), the 2025 Global GLO–JOPE Conference, the Economics Workshop at James Madison University, and the Economics Brown Bag at the University of Maryland, Baltimore County, for their helpful comments and feedback. We also gratefully acknowledge funding from the Cornell SC Johnson College of Business and the 2025 Summer Research Grant from the James Madison University College of Business.

## 1 Introduction

Strikes rarely occur in isolation (Biggs, 2003). Workers draw lessons from others' experiences and are motivated by observing peers mobilize (Fantasia, 1988). Peer learning can therefore play an important role in the spread of labor strikes. Given the substantial opportunity costs associated with striking (Ashenfelter and Johnson, 1969; Hayes, 1984; Card, 1988, 1990; Cramton and Tracy, 1994), observing strike activity in socially, spatially, and economically connected peer locations can influence workers' willingness to strike. With the rise of social media as a tool for communication and mobilization by unions (Houghton and Hodder, 2021, Blanc, 2025), networks have become important channels for information sharing and coordination. This paper addresses a hitherto understudied issue in the empirical labor strike literature by evaluating the role of networks in overcoming the collective action problem inherent in labor strikes (Granovetter, 1978; Sikes, 2023).

Systematic evidence on peer learning in strikes remains limited. More broadly, labor strikes are understudied despite their historically important role in shaping labor relations and economic outcomes. Although major work stoppages involving more than 1,000 workers have declined from over 400 per year in the 1950s to around 20 per year in recent decades, strikes continue to affect large numbers of workers. In 2022, for example, 23 major work stoppages involved 120,600 workers and resulted in more than 2.19 million idle days.[1] Moreover, our study period captures a revival in labor activism: strike activity during 2021–2024 reached its highest level in roughly two decades, although it remained far below mid-century levels. This revival occurred amid conditions favorable to collective action, including a tight labor market, inflation that outpaced wage growth, and heightened public attention to strikes and unions.[2]

Despite this renewed activity, our understanding of the determinants of strikes in the United States remains limited in two important respects. First, much of the U.S. literature on strikes dates to the 1990s and therefore predates the recent resurgence in labor activism. Second, commonly used data sources provide only a partial picture of strike activity. The BLS Work Stoppages program,[3] for example, records only events involving at least 1,000 workers. By excluding smaller strikes, these data make it difficult to study collective action across different scales or to examine how strike activity spreads across locations. The Federal Mediation

[1] U.S. Bureau of Labor Statistics, Work Stoppages Program, *Annual Work Stoppages Involving 1,000 or More Workers, 1947–Present*, `https://www.bls.gov/web/wkstp/annual-listing.htm`. The counts of stoppages and workers refer to stoppages beginning in 2022; days of idleness cover all stoppages in effect during the year.

[2] See Appendix A for an illustration of recent labor activity in comparison to historical levels and for broader context on the time period of analysis.

[3] U.S. Bureau of Labor Statistics, Work Stoppages program, `https://www.bls.gov/wsp/`.

and Conciliation Service (FMCS) also used to release data on work stoppages, but these data covered only the private sector, contained relatively little detail on each stoppage, and the associated data release stopped in 2020.[4]

Thus, in this paper, we draw on a new dataset compiled by the Cornell School of Industrial and Labor Relations Labor Action Tracker (ILRLAT), which began collecting data in late 2020 (Kallas et al., 2026). The ILRLAT aims to comprehensively, though not universally,[5] document labor strikes in the United States at all scales. For our analysis, we use strike data from 2021 to 2024. The dataset provides detailed information on each strike, including the date, location, employer, labor organization, industry, approximate number of participants, strike duration, and workers' demands (e.g., pay increases, benefits). This level of detail allows us to study the spread of strikes at a much finer scale than was previously possible.

Our empirical strategy combines the ILRLAT data with measures of network exposure to strikes across multiple dimensions. We define network exposure using a standard linear-in-means model of social interactions with leave-one-out exposure that is specific to each unit (Bramoullé et al., 2009; Goldsmith-Pinkham and Imbens, 2013). Our main focus is on social connectedness, which we measure using Facebook friendship links between counties. Throughout, our argument concerns social networks broadly—the web of interpersonal connections among workers—with social media serving as one prominent and measurable channel through which these networks transmit information, rather than as the object of interest in itself, except where explicitly discussed that way. In addition, to capture other potential channels through which strikes may spread, we construct geographic, industrial, occupational, political, ethnic, and educational connectedness indicators linking any two locations prior to the analysis period. These complementary network measures allow us to distinguish the role of social networks from other forms of connectedness that may influence strike activity.

To estimate the spread of strikes through network exposure, we use a Poisson regression model where the dependent variable is the number of strikes per 1,000 residents in a given county and month. The key explanatory variables measure exposure to strikes through social networks in the current month and the cumulative exposure from the preceding months. To address potential confounding factors, we also include exposure measures based on geographic, occupational, educational, industrial, ethnic, and political connectedness for both

[4]Although the FMCS data are not our preferred source, we use them for robustness checks and find broadly similar relationships to our main results in the most recent decades.

[5]Cornell University School of Industrial and Labor Relations, "Methodology," *Labor Action Tracker*, https://striketracker.ilr.cornell.edu/methodology.html.

current and lagged periods. All specifications include county and month fixed effects to account for unobserved heterogeneity across locations and over time.

Our results reveal a strong and robust relationship between strike activity in a given county and the social network exposure of that county to strikes elsewhere, measured at the county–month level. Contemporaneous exposure through social networks is positively associated with local strike incidence, even after controlling for geographic, industrial, political, and demographic similarities. A one-standard-deviation increase in contemporaneous social-network-weighted strike exposure is associated with approximately 16% higher expected strike incidence. Estimates for cumulative lagged exposure are also positive but less precisely estimated. Additional analyses show that the strength of this association depends on the characteristics of the observed strikes. Social network exposure is most strongly linked to strike activity within the same industry. Moreover, contemporaneous exposure to strike participation is positively associated with local strike participation, indicating that social network exposure is correlated with strike activity both at the extensive and the intensive margins.

Next, we examine the mechanisms that may explain such relationship between social network exposure to strikes and local strike activity. The first potential mechanism is what we refer to as the *information discovery channel*. The theoretical literature on labor strikes has long emphasized the role of costs, benefits, and risks as informational determinants of strike behavior. In a body of studies based primarily on U.S. data and individual wage negotiation episodes rather than national-level analyses, various proxies for the potential costs and gains from striking have been shown to co-move with strike incidence in intuitive ways. These include real wages (Ashenfelter and Johnson, 1969), prime-age male unemployment rates (Vroman, 1989; Cramton and Tracy, 1992), product market sales concentration (Abowd and Tracy, 1989), and judicial labor injunctions that suppress strikes (Naidu and Yuchtman, 2016). Other determinants have yielded mixed results, such as positive product price effects in Tracy (1986, 1987) and McConnell (1990), contrasted with negative product price effects in Kennan (1985) and Harrison and Stewart (1989).

To test the existence and relevance of the information discovery channel, we focus on two key measures: (i) strikes carried out by unions with large numbers of social media followers and (ii) strikes that result in a deal between workers and employers. Prior studies emphasize that union strength and settlement outcomes transmit information shaping workers' beliefs. Signals about union rank-and-file and leadership intentions (Ashenfelter and Johnson, 1969) and the perceived feasibility of higher wage demands (Fudenberg and Tirole, 1983; Sobel and Takahashi, 1983; Hayes, 1984; Cramton and Tracy, 1994) are amplified through networks, which allow workers to observe credible information about the economic rationale for striking (McConnell, 1990; Lehr et al., 2015). Specifically, if exposure to strikes by unions with more followers in socially connected locations increases the incidence of new strikes, this would indicate that union social media networks serve as important channels of information transmission. Because comprehensive measures of union social media presence are unavailable, we use Facebook follower counts as a proxy for a union's broader social media presence.[6] Similarly, if observing successful strikes elsewhere raises strike activity in connected locations, this is consistent with workers updating their beliefs based on observed outcomes. Other characteristics, such as large strikes or those in the same industry, may also convey information critical to motivate strike participation.

Beyond information transmission, behavioral motivations such as solidarity, shared identity, and emotional contagion may also drive the spread of strikes. Workers may be motivated by observing peers mobilize, shared grievances, or collective emotions. We refer to this as the *behavioral channel* in the diffusion of strikes through social networks. A central insight of labor sociology is that cultures and solidarity are key drivers of mobilization (Fantasia, 1988), while social movement studies highlight how emotions and shared frames diffuse through networks (Snow and Benford, 1988). Historical analyses of strike waves also document behavioral contagion alongside informational spillovers (Shorter and Tilly, 1974; Biggs, 2003).

Likewise, to assess whether the evidence is consistent with a behavioral channel, we examine two sources of variation: (i) unions' social media presence and (ii) the conclusion of strikes through settlements. If local strikes respond no more strongly to strikes involving unions with a larger social media presence, this would be inconsistent with social media amplifying the transmission of information, pointing instead toward behavioral motivation. In addition, if settlements of strikes in peer locations do not influence subsequent local strike activity, while overall strike incidence in peer locations does, this pattern is consistent with a behavioral channel. In this case, workers may be responding to their peers' mobilization out of solidarity rather than using observed settlements as informational cues.

To collect data on union strength and strike outcomes, which are not included in the ILRLAT dataset, we hand-collected information from the Facebook pages of unions and their local chapters for each strike listed in the original ILRLAT entries, recording the number of followers for each page. To determine whether a strike resulted in a deal between the union

[6] While follower counts do not measure actual message exposure or engagement, they provide a standardized and readily available indicator of the size of a union's social media presence (Mohammadi et al., 2017).

and the employer, we use a large-language-model research agent that searches the web for the outcome of each strike, archives the supporting source, and verifies that the source names the same employer and bargaining unit and contains a verbatim quote supporting the outcome and its date; the full pipeline prompt is reproduced in Appendix B. Only outcomes with such documented confirmation are coded as deals, and validation against manually adjudicated subsets indicates no false positives and a low rate of missed deals. Based on these data, we construct exposure measures to strikes weighted by the number of union followers, as well as exposure measures to settlements.

The results on union Facebook followers and strike settlements provide limited evidence for the informational channel and stronger support for the solidarity channel. We find little evidence that unions' social media presence independently strengthens the relationship between social network exposure and local strike activity. Exposure to strike settlements is not associated with local strikes, whereas contemporaneous exposure to strike activity remains positively associated with local strike incidence. Together, these findings suggest that workers respond more to observing mobilization among socially connected peers, regardless of the social media presence of the affected unions, or the settlement of the mobilization effort. These observations do not lend support to strike exposure as an information discovery channel.

We also examine an alternative measure of union strength, based on administrative and policy environments that limit union activities. In terms of the administrative environment, we compare public- and private-sector strikes. Public-sector unions are generally governed by state law and often face advance-notice and mediation requirements that may delay workers' responses to strikes elsewhere.[7] Although it appears that the administrative distinction matters—the network spillover effect of strikes is sector-specific—we find no systematic difference between the public and private sectors in how contemporaneous or lagged network exposure translates into local strike activity.

In terms of policy environment, we focus on state-level Right-to-Work (RTW) laws, which weaken union power by prohibiting union security agreements and allowing workers to opt out of dues (Chen et al., 2026). RTW laws apply to the private sector only based on §14(b) of the National Labor Relations Act. Public-sector bargaining falls outside the NLRA and is governed by state law, but *Janus v. AFSCME* (2018) prohibited unions nationwide from collecting agency fees from nonconsenting public employees.[8] Right-to-work is thus a state option in the

[7] Minnesota Bureau of Mediation Services, "Strike Timelines as of February 2024," `https://www.revisor.mn.gov/statutes/cite/179A.18`.

[8] 29 U.S.C. §164(b); *Janus v. American Federation of State, County, and Municipal Employees, Council 31*, 138 S. Ct. 2448 (2018), overruling *Abood v. Detroit Board of Education*, 431 U.S. 209 (1977).

private sector and a constitutional rule in the public sector, so public employees are effectively covered in every state throughout our 2021–2024 sample.[9] Overall, this leads us to cautiously expect RTW laws to matter more in the private sector.

We test whether the relationship between strike activity in socially connected areas and local strike incidence differs by RTW status and whether that difference is mediated by the sector. We find that the RTW status is associated with stronger contemporaneous transmission in the private sector—consistent with weaker unions and greater reliance on peer networks. On the other hand, the effect of RTW status in the public sector—where the specific RTW laws do not apply—is positive but imprecisely estimated, and is not statistically different from 0 nor from the effect in the private sector. Overall, the evidence suggests that public-sector administrative constraints do not substantially alter the strength or timing of network diffusion, whereas evidence on the role of the RTW status is mixed.

Our results contribute to the empirical literature on peer learning in labor strikes. Because the sources of peer learning that feed into strikes are difficult to observe, empirical work on this topic remains relatively limited. Earlier studies examine how learning from one's own experience with the costs of strikes can discourage future strikes (Mauro, 1982; Schnell and Gramm, 1987), or encourage further action once the sunk costs of learning and organization have been paid (Stern, 1975; Lipsky and Drotning, 1977; Kochan and Baderschneider, 1978). Other work highlights the role of the media, examining how errors and biases affect perceptions (Snow et al., 1986; Schmidt, 1993) and how media reports can fuel commitment and consistency biases (Jellison and Mills, 1969; Cialdini, 1984; Flynn, 2000). Most closely related, recent work documents spillovers within union organizing itself: Schaller et al. (2025) find that counties experiencing Starbucks union elections subsequently saw substantially more union elections at other employers. We contribute to this literature by providing systematic evidence on how peer networks transmit information relevant to strike decisions, using new data on both social connections and strike outcomes.

Our results also contribute to a growing body of research on how online social networks shape collective action more broadly. For example, Flückiger and Ludwig (2025) show that Facebook friendship networks played a crucial role in the spread of Black Lives Matter protests across U.S. counties following the killing of George Floyd. Similarly, Qin et al. (2024) document the role of online social media in diffusing protest activity in China, while Enikolopov

[9] One caveat to this is that other restrictions to public union activities exist, and pre-*Janus* decision, RTW states were generally also more restrictive for the public sector, making RTW status correlated with public sector policy environment restrictiveness.

et al. (2020) find that greater penetration of VKontakte increased protest participation in Russia. Beyond protest settings, online networks have also been shown to shape other collective behaviors such as compliance with lockdown policies during the COVID-19 pandemic (Bailey et al., 2024). In the labor context specifically, recent evidence documents that the post-2020 U.S. organizing upsurge was substantially organized online: unionization drives in 2022 were led disproportionately by young workers who relied on social media for publicity and diffusion and were often coached remotely by experienced organizers (Blanc, 2026), and new worker-to-worker structures such as the Emergency Workplace Organizing Committee institutionalized online peer support for organizing across industries (Blanc, 2024, 2025). Our paper extends this literature by focusing on labor strikes as a form of collective action, showing how social network connections mediate both informational and behavioral spillovers across locations.

From these findings, we draw several key takeaways. First, we provide suggestive evidence that labor strikes spread primarily through social networks rather than through other types of linkages based on spatial proximity, economic similarity, or political ideology. Second, the strength of network effects depends on specific characteristics of the observed strikes, including the similarity of industries or grievances and the size of the strikes. Finally, we test two key mechanisms—information discovery and solidarity—by examining how network effects vary with the social media presence of the unions involved and whether the observed strikes resulted in successful agreements with employers. We also show whether policy and administrative environments, such as right-to-work laws and public-sector notice requirements, shape how network exposure translates into strike activity. Together, these patterns highlight the dual role of social networks in facilitating both information transmission and solidarity, shaping workers' peer learning about striking.

# 2 Data

## 2.1 ILR Labor Action Tracker

We analyze labor strikes from 2021 to 2024 using data from the Cornell School of Industrial and Labor Relations' Labor Action Tracker (ILRLAT). The ILRLAT provides one of the most comprehensive public databases on strike and labor protest activity across the United States. Data collection began in late 2020, with systematic and complete coverage starting in January 2021. The ILRLAT records two types of labor actions: strikes, which involve work stoppages, and labor protests, which are collective actions without work stoppages. For brevity, we combine both forms and refer to them as "strikes" throughout the paper. According to the project's methodology documentation, events are identified through ongoing monitoring of official databases, media reports, labor organization sources, and social media accounts; coded according to standardized protocols; and reviewed by trained researchers to ensure consistency across cases. Although the tracker strives for accuracy and detail, it does not provide a universal record of labor actions because of their high frequency and decentralized nature.[10]

Nevertheless, the ILRLAT captures a broader range of labor actions than the U.S. Bureau of Labor Statistics (BLS) Work Stoppages data, which include only major stoppages involving 1,000 or more workers.[11] As a validation exercise, we manually cross-checked all 103 work stoppages recorded by the BLS between 2021 and 2024 and found that every BLS-recorded strike is also included in the ILRLAT dataset. This suggests that the ILRLAT captures the full universe of major strikes recorded by the BLS while also documenting thousands of smaller labor actions excluded from the BLS data. Appendix A places the 2021–2024 study period in broader historical and economic context using alternative data sources, including the BLS and FMCS (which stopped its private sector work stoppage data release in 2020), on long-run strike activity, industry composition, labor-market conditions, and public attention to labor organizing. Comparisons with these sources indicate that the ILRLAT data are broadly representative of prevailing labor conditions during the study period.

Each ILRLAT entry is manually coded using a combination of Tier I (official databases), Tier II (media and labor sources), and Tier III (social media or informant accounts), with cross-verification protocols to ensure reliability. Variables include the strike date, address, employer, labor organization, industry, and the approximate number of participants. Events are categorized as strikes or labor protests, and, when applicable, variables include strike duration (measured in total days, including holidays and weekends), whether the action was authorized by union leadership, and the workers' core demands (e.g., pay, job security, healthcare, union recognition). Each event also links to original sources used for verification.

The ILRLAT records strikes with multiple locations in a single data entry. When a strike is organized by workers from the same union across multiple cities, or when sympathy strikes occur alongside a main strike in multiple locations, the ILRLAT typically lists these as additional locations within the original strike rather than as separate strikes. For example, if Teamsters at Sysco go on strike in Baltimore and other Teamsters hold the line in Seattle, it

[10] Cornell University School of Industrial and Labor Relations, "Methodology," *Labor Action Tracker*, https://striketracker.ilr.cornell.edu/methodology.html.

[11] U.S. Bureau of Labor Statistics, "Work Stoppages Program," https://www.bls.gov/wsp/overview.htm.

would be recorded as a single data entry. For such multi-location strikes, the ILRLAT is unable to disaggregate the number of workers participating across the different locations.

The ILRLAT does not provide information on strike outcomes, such as whether or when a strike resulted in a deal. We therefore determine the outcome of each recorded strike using a large-language-model research agent (Claude Code) executing a multi-phase search-and-verification pipeline; the full prompt is reproduced in Appendix B. For each strike, the agent runs outcome-focused web searches to determine whether the workers' stated demand was met within roughly twelve months of the strike and, if so, the date of the settlement. It then archives the supporting source as saved page text and a full-page screenshot, and builds a verification record confirming that the source names the same employer and bargaining unit as the strike and contains a verbatim quote, located in the saved text, that supports both the outcome and its date. Only outcomes confirmed, directly or indirectly, by documentable sources are coded as deals; strikes whose outcomes cannot be confirmed from documentable sources are coded as not resulting in a deal. We validated the resulting classifications against manually adjudicated subsets: a manual check of confirmed deals found no false positives, and a manual review of ten percent of the negative classifications suggests a rate of missed deals below five percent, mostly in the form of partial or indirect wins. In addition, for the confirmed settlements we located worker-side public announcements of the agreements (found for 92.8% of settlements) and classified their sentiment and framing; Tables B15 and B16 summarize this dataset, and Appendix C reproduces the corresponding pipeline prompt. A strike counts as ending in a deal if any of its stated demands was met within roughly twelve months, including partial or interim concessions. For the county–month analysis we further exclude settlements attributed to legislation or policy and restrict attention to *single-county* settlements: a settlement enters the deal count of a county only when the striking bargaining unit's footprint lies entirely within that county, dated by the month of the settlement rather than the month the strike began. Settlements spanning multiple counties are therefore excluded from the county–month deal counts rather than counted in each county they touch. The archived sources, verification records, and announcement corpus are included with the replication materials.

Table 1 summarizes the ILRLAT strikes, reporting total incidences, average number of participants per incidence, average duration (in days),[12] and the types of strike demands and forms. Under the "Count of Strikes" category, "Strike-by-Location Observations" counts each

[12]The duration variable comes directly from the ILRLAT data and reflects the number of days reported by the media for a strike. It does not measure the number of days from the start of a strike to the signing of a deal. Figure 2 separately reports the distribution of days from strike onset to settlement.

instance in which a strike occurs in a distinct location on the first date it is observed. "Main Strikes" refers to the original ILRLAT entries. "Independent Strikes" includes only strikes that take place at a single location. For example, a multi-day strike spanning five different locations is counted as five strikes in "Strike-by-Location Observations," recorded as one strike in "Main Strikes," and excluded from "Independent Strikes" because it spans multiple locations. All of these categories count only the first date a strike is observed, so strikes lasting multiple days are not counted repeatedly.

This paper uses "Strike-by-Location Observations" for the primary analysis, so references to "strikes" throughout the rest of the paper refer to this measure. Under this definition, each strike occurrence in a distinct location is counted separately. We focus on this measure because the geographic spread of strike activity across locations is itself meaningful when evaluating how information and mobilization may diffuse through social networks. However, one concern is that a single coordinated strike spanning multiple locations, may generate correlations in strike activity across counties, and in such a strike the role of network spillovers may be hard to disentangle from the organizing role of a large union. To address this concern, we also conduct robustness checks using "Independent Strikes," which include only strikes occurring in a single location and exclude all multi-location strike events. As a result, the Independent Strikes measure is not affected by the possibility that the same coordinated strike is counted in multiple locations. The results remain qualitatively similar when Independent Strikes are used in place of Strike-by-Location Observations as the outcome variable, suggesting that our findings are not driven by multi-location strikes.

Overall, based on Table 1, there are 4,805 recorded strikes from 2021 to 2024, with just over half ending in a deal. This contrasts sharply with the BLS Work Stoppages count, which records only 103 major work stoppages over the same period.[13] To examine patterns by industry, we group strikes into representative sectors where strike activity is most common: education, healthcare, other service-providing industries, and goods-producing industries. The ILRLAT dataset includes over 70 detailed industry categories, so some simplification was necessary. Education and healthcare follow the original ILRLAT classification. We use the North American Industry Classification System (NAICS) to combine agriculture, construction, manufacturing, and mining into a single "goods-producing" category, and classify all remaining industries under "other service-providing." Examples of the latter include retail, food services, transportation, and more. According to Table 1, education and healthcare each account for ap-

[13] U.S. Bureau of Labor Statistics, "Work Stoppages Program," Monthly Listings, 2021–2024, https://www.bls.gov/web/wkstp/monthly-listing.htm.

Table 1: Summary Statistics for Labor Strikes

| | All Strikes | Strikes with Deal | Strikes by Industry | | | |
|---|---|---|---|---|---|---|
| | | | Education | Healthcare | Other Service-Prov | Goods-Prod |
| *Count of Strikes* | | | | | | |
| Strike-by-Location Observations | 4,805 | 2,529 | 858 | 884 | 2,667 | 421 |
| Main Strikes | 3,545 | 1,807 | 655 | 647 | 1,926 | 332 |
| Independent Strikes | 3,095 | 1,537 | 577 | 552 | 1,676 | 303 |
| *Size of Strikes* | | | | | | |
| Average Number of Participants per Strike | 2,927 | 4,565 | 3,882 | 3,774 | 2,397 | 4,366 |
| Median Number of Participants per Strike | 100 | 200 | 130 | 250 | 60 | 160 |
| *Strike Duration* | | | | | | |
| Average Number of Days per Strike | 14 | 19 | 9 | 18 | 10 | 41 |
| *Strike Demands (Strike Counts)* | | | | | | |
| Pay Raise | 2,817 | 1,857 | 605 | 563 | 1,381 | 289 |
| Health and Safety | 1,819 | 1,020 | 308 | 389 | 967 | 172 |
| Others (Job Security, Benefits, etc.) | 1,359 | 447 | 157 | 175 | 939 | 92 |
| *Strike Form (Strike Counts)* | | | | | | |
| Strikes | 2,307 | 1,308 | 375 | 301 | 1,382 | 259 |
| Protests | 2,498 | 1,221 | 483 | 583 | 1,285 | 162 |

*Notes*: This table summarizes basic information from the Cornell School of Industrial and Labor Relations Labor Action Tracker (ILRLAT) for the years 2021–2024. The sample includes both strikes (work stoppages) and labor protests (collective labor actions without a work stoppage), as reported in the Strike Form panel; this paper refers to all such labor actions collectively as “strikes.” Under the "Count of Strikes" category, "Strike-by-Location Observations" counts each instance in which a strike occurs in a distinct location. "Main Strikes" refers to the original ILRLAT entries. "Independent Strikes" includes only strikes that occur at a single location. For example, a multi-day strike spanning several locations, or a strike that prompts sympathy actions in other locations, is recorded as one strike in "Main Strikes," counted as multiple strikes by location in "Strike-by-Location Observations," and excluded from "Independent Strikes" because it spans multiple locations. This paper uses "Strike-by-Location Observations" for the primary analysis but uses "Independent Strikes" for robustness checks.

proximately 18% of all strike-by-location observations, other service-providing industries for about 55%, and goods-producing industries for around 9%.

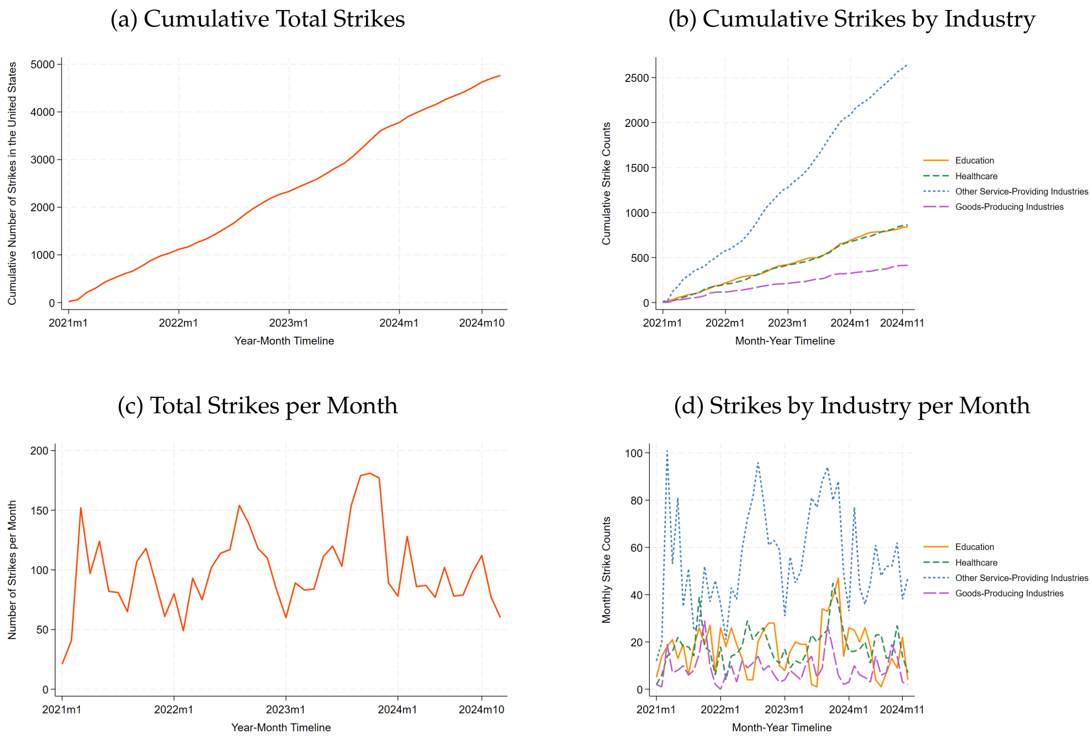
Figure 1 complements Table 1 by illustrating trends in monthly strike activity from 2021 to 2024. Panel (a) displays cumulative monthly strike activity, while Panel (b) disaggregates activity by selected industries: education, healthcare, other service-providing industries, and goods-producing industries. Across both panels, cumulative strike counts increased steadily over time. The largest cumulative counts occurred in other service-providing industries, followed by education and healthcare (which grew at similar rates), and then goods-producing industries. Panels (c) and (d) present the corresponding monthly strike counts. Unlike the cumulative trends, monthly strike activity exhibits substantial variation over time, with periods of elevated strike activity followed by temporary declines. Despite this short-run volatility, the overall upward cumulative trends in Panels (a) and (b) indicate sustained strike activity throughout the sample period.

When examining strike size using Table 1, we find that the median strike involves just over 100 participants, while the average number of participants per strike is much higher, approaching 3,000. This suggests that strike sizes are heavily skewed to the right, with most strikes involving a few hundred participants and a small number involving thousands. The average strike duration is about 14 days. Regarding reasons for striking, about 60% of strikes demand a pay raise, and nearly 38% focus on health and safety. Other common concerns include job security and benefits. As for the form of strikes, the distribution is roughly even, with about half being protests and the other half involving work stoppages.

The ILRLAT records strikes in Puerto Rico and the other U.S. territories; we exclude them, so the analysis covers the fifty states and the District of Columbia. Our analysis aggregates strikes to the county-month level, covering 3,141 counties over 48 months from 2021 to 2024. As shown in Figure 2, panel (a), 98% of county-months experienced no strikes, while a small fraction experienced one strike and very few experienced two or more. County-months with more than five strikes were extremely rare, and the maximum number of strikes observed in a single county-month was 22. Panel (b) presents the distribution of strike outcomes by the time required to reach a deal, based on the original "Main Strikes".[14] About half of the strikes did not result in a recorded agreement. Among those that reached a deal, settlements after more than 60 days were the most common. Settlements reached within 8–30 days and 31–60 days were less frequent, while settlements within one week were relatively rare.

[14]Because settlement information is recorded at the strike-event level, assigning the same settlement outcome to each location involved in a multi-location strike would mechanically duplicate the outcome in the data.

Figure 1: Labor Strike Trends, 2021-2024




(a) Cumulative Total Strikes

(b) Cumulative Strikes by Industry

(c) Total Strikes per Month

(d) Strikes by Industry per Month

*Note*: This figure presents the monthly cumulative strike counts and strikes per month across industries for 2021-2024, using data from Cornell University's School of Industrial and Labor Relations' Labor Action Tracker (ILRLAT). The ILRLAT data includes over 70 industry categories. The figure above highlight representative sectors with the most frequent levels of strike activities. Education and healthcare follow the original dataset's classification. We followed the North American Industry Classification System (NAICS) to combine agriculture, construction, manufacturing, and mining into a single "goods-producing" category, and the rest are under the broader "other service-providing" category.

To further characterize strike activity, we also examine for each county-month the cumulative number of strikes over the past one to six months. Figure A2 shows that as the time span increases, more county-months exhibit higher frequencies of strikes. While most counties still record only a small number of strikes, aggregating across longer horizons reveals a growing share of county-months with higher strike activity.

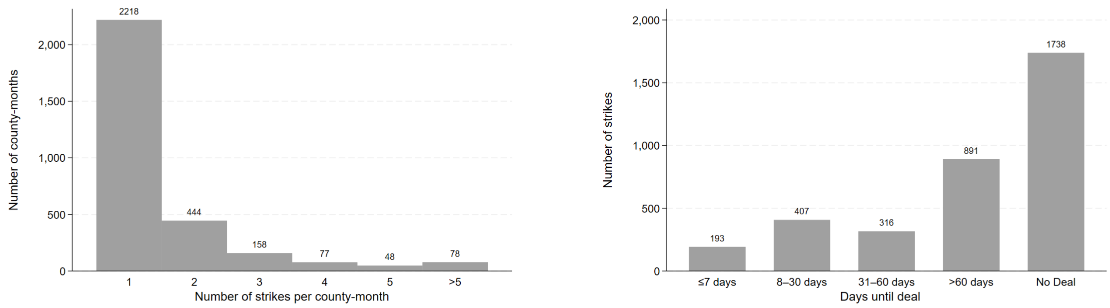
## 2.2 Additional Datasets

### 2.2.1 Facebook Social Connectedness Index

We use the Facebook Social Connectedness Index (SCI) to measure county-to-county social media connectivity. This index captures the strength of ties between two U.S. counties based on Facebook friendship connections. Specifically, the SCI reflects the relative probability that individuals from two different locations are Facebook friends. Further details about this measure are provided in Section 3. The SCI methodology was developed by Bailey et al. (2018), who used an anonymized dataset of all active Facebook users and their friendship networks

Figure 2: Distributions of Strikes and Settlement Times

(a) Distribution of Strikes per County-Month　　(b) Distribution of Strike Settlement Times




*Note*: Figure (a) shows the distribution of strikes across county-months using *Strike-by-Location Observations*, noting that county-months with zero strikes account for 97% of all observations. Figure (b) displays the distribution of strike settlement times using *Main Strikes*. We use *Main Strikes* in Figure (b) because settlement information is recorded at the strike-event level. Assigning the same settlement outcome to each location involved in a multi-location strike would mechanically duplicate the same settlement outcome in the data. Both figures are constructed using data from the 2021–2024 Cornell University School of Industrial and Labor Relations Labor Action Tracker (ILRLAT).

to quantify the intensity of social connections between counties. We use the SCI index based on August 2020 data, which is before our sample period and avoids the issue of endogenous networks formed in part due to collective action.

#### 2.2.2 Union Social Media Information

For each union that organized a strike recorded in the ILRLAT, we collected additional information on Facebook follower counts from the official Facebook pages of the local union chapters, where such pages exist. Where a page did not report a follower count, we recorded instead the number of accounts that had liked the page. If no page could be found, no social media information was recorded. All data were collected as of June 2025. Counts recorded as zero indicate that no page could be located rather than a union without followers, and are treated as missing; pages belonging to the struck employer, to unrelated campaigns, or to a national or international union rather than the striking local are excluded, so the measure reflects the reach of the local organizations that ran each strike; where several locals jointly organized a strike, their follower counts are combined. While we acknowledge that these follower or membership counts may differ from those at the time the strike occurred, historical data on such metrics is unavailable, and we are unable to reconstruct follower counts retrospectively.

#### 2.2.3 Alternative Distance Measures

We also utilize alternative measures of county-to-county connectivity beyond the SCI. Specifically, we select measures that influence the likelihood of forming friendship links and may generate correlated strike activity across locations. These include geographical proximity,[15] as well as similarities in political ideology (MIT Election Data and Science Lab, 2022), education levels, ethnicity, occupational structure, and industrial activity, all drawn from Manson et al. (2021).

#### 2.2.4 County Characteristics

To account for potential confounding factors at the local level, we include a rich set of county-level controls capturing demographic, socioeconomic, political, and geographic characteristics. These controls include the percentage of male, White, Black, Hispanic, and Asian residents; educational attainment measures such as the share of the population with a college degree or without a high school diploma; and median household income and age. We also control for geographic and structural factors including land area, population density, and the share of the population living in urban areas. Finally, to account for political orientation, we include the share of Democratic voters in each county. Most of these variables are drawn from the 2020 American Community Survey (ACS) 5-Year Estimates. Political variables are taken from the 2020 MIT Election Lab county-level votes on the 2020 presidential election, and land area data are from the U.S. Census.

## 3 Empirical Strategy

The primary objective of our empirical analysis is to examine whether the number of strikes is systematically associated with strike activity transmitted through social networks. A secondary objective is to explore the possible mechanisms underlying this relationship. While this task is inherently challenging because different mechanisms can generate similar empirical patterns and are difficult to disentangle with observational data alone, we make a systematic effort to outline the key channels through which network effects may operate and to provide empirical evidence that helps distinguish among them.

Most strikes in our data are organized by labor unions or similar entities, not only because of their ability to coordinate collective action but also because workers' legal protections during strikes depend on whether the strike is classified as "protected," which in turn depends on how it is organized and conducted.[16] Consider therefore the decision problem of a potential participant in a strike organized or considered by a recognized labor organization. To help think about the multiple factors that shape the decision to participate in or organize a strike,

[15]U.S. Census Bureau, "County Adjacency File," https://www.census.gov/geographies/reference-files/time-series/geo/county-adjacency.html. National Bureau of Economic Research, "County Distance Database," https://www.nber.org/research/data/county-distance-database.

[16]National Labor Relations Board (NLRB). "Employee Rights: Strikes." https://www.nlrb.gov/strikes.

we begin with a highly simplified expression of the net expected value of striking. Let

$$E[\text{NB}_{\text{strike}}] = E[\text{Benefits}_{\text{strike}}] - E[\text{Cost}_{\text{strike}}],$$

where the expected benefits and costs reflect workers' beliefs about the likely outcomes of striking. We focus on the net benefit rather than modeling benefits and costs separately, since social networks can simultaneously influence perceptions of both potential gains and potential risks.

We stipulate two complementary ways in which social networks can impact the expected net benefits of strikes: (i) informational discovery channel, and (ii) behavioral channel.

Information Discovery Channel

The first mechanism follows a class of conventional explanations for strike participation, which focus on how workers compare the expected benefits and costs of striking. Social networks and social media can influence these expectations by exposing workers to information about strikes that have occurred elsewhere. Observing such information can shape beliefs about the net benefit of striking, which in turn affects participation decisions. For instance, workers may adjust their expectations of success based on observed settlements elsewhere, but they may also update their assessment of the risks of employer retaliation by learning how other firms responded to strikes in comparable settings. In this sense, information shapes overall beliefs about whether striking is worthwhile, rather than affecting benefits or costs in isolation.

To capture the role of information, we examine how observable characteristics of previous strikes can inform prospective participants about whether to strike. Previous strike counts, industry affiliation, and strike size are particularly relevant. Strikes occurring in the same or similar industries provide information about the feasibility of mobilizing collective action under comparable institutional and economic conditions. The number of participants in past strikes signals the scale of mobilization elsewhere and can shape expectations about potential bargaining outcome.

While these variables capture important aspects of the information discovery channel, they are not sufficient to isolate this mechanism from others, since they may also influence emotional responses such as solidarity. Information on strike settlements is particularly important for identifying this channel. The idea is that if workers are more likely to strike after observing a successful settlement elsewhere, this behavior reflects learning through information transmission rather than other forms of network spillovers, such as emotional solidarity.

Unions often publicize favorable settlement outcomes widely through various communication channels, including social media platforms such as Facebook.[17] In addition, weighting strikes by the number of Facebook union followers in other locations provides another way to test the information mechanism. Locations with stronger social media connections to places experiencing strikes are more likely to be exposed to timely and detailed information about those events, making informational spillovers more salient relative to other channels. In addition to Facebook follower counts, we also examine how policy and administrative environments that affect the strength of unions—state right-to-work laws and the public–private sector distinction—may shape how network exposure translates into local strike activity. Weaker unions may make it more difficult to organize strikes but simultaneously increase the role of non-union factors, such as peer networks.

Behavioral Channel

In addition to expected utility revision based on informational updates, the economic literature has highlighted several behavioral determinants of strike activity mediated by social networks. A central tenet of the labor movement has been solidarity among workers. Observing strikes by friends or colleagues in other locations may motivate individuals to join the larger collective struggle of workers against employers and shareholders. This solidarity-based channel of strike contagion posits that individual workers harbor social preferences that are activated when workers elsewhere go on strike. Thus, workers consider exercising an expression of solidarity to fellow workers on strike as itself a source of net benefits of strikes. Distinct from the information channel, therefore, solidarity-based motivation to strike can be activated even when observed strikes are unsuccessful, as the rationale justifying strikes is unrelated to the likelihood of a deal to be struck with employers per se.

Observing strikes by socially connected peers may also shift perceived social norms: as strikes become more common among the workers one knows, striking itself may come to appear more socially acceptable—a form of social proof (Cialdini, 1984). We group such norm-based responses with the behavioral channel, since—like solidarity—they can operate regardless of whether the observed strikes succeed.

Therefore, to test the behavioral channel separately from the information discovery channel, whether a strike elsewhere resulted in a settlement should not affect workers' decisions, since workers would be expected to strike regardless. Consequently, settlement outcomes provide a useful way to distinguish between the two channels.

[17] For example, the United Auto Workers (UAW) highlighted the benefits of a strike-ending deal with John Deere in a widely shared Facebook post.

### 3.1 PCI Construction

We construct a baseline measure of exposure to strikes through social networks. The intuition is straightforward: *ceteris paribus*, the more strikes occur in places where an individual's friends, family, or other social connections live, the more likely it is that the individual will hear about these strikes.

To operationalize this idea, we begin by measuring the likelihood of Facebook friendship links between residents of different counties. We then combine this measure with strike activity in other counties to construct a county-level indicator of exposure to information about strikes elsewhere.[18]

Specifically, we define the *population-weighted social connection index* ($PCI_{ij}$) as the fraction of all Facebook friends of a representative resident in county $i$ who live in county $j$. This follows the approach in Bailey et al. (2024), who refer to this measure as *FracConnect*:

$$PCI_{ij} = \frac{SCI_{ij} \times \text{Pop}_j}{\sum_{j \in J} SCI_{ij} \times \text{Pop}_j},$$

where

$$SCI_{ij} = \frac{\text{FB Connections}_{ij}}{\text{FB Users}_i \times \text{FB Users}_j}.$$

The $SCI_{ij}$ measures the relative intensity of Facebook friendships between counties $i$ and $j$ compared to all possible connections. A higher $PCI_{ij}$ indicates that residents of county $i$ are more likely to be socially connected to those in county $j$ through Facebook, and therefore more likely to receive strike-related information from them.

Finally, we construct our measure of Facebook-mediated exposure to strikes in county $i$ at time $t$ as:

$$PCIST_{it} = \sum_{j \neq i} PCI_{ij} \times ST_{jt},$$

where $ST_{jt}$ denotes the number of strikes in county $j$ during month $t$. Thus, $PCIST_{it}$ captures the extent to which county $i$ is exposed to strike activity occurring in socially connected counties, with exposure weighted by the strength of Facebook social connections.

Other Proximity/Similarity Measures

In addition to social network connections captured by $PCI$, we also need to account for other forms of similarity between counties that may influence both friendship formation and strike participation patterns. People who live near each other, work in similar occupations

[18]For the construction of this and other exposure measures, we follow Basu et al. (2025) in the context of Covid-19 vaccination.

or industries, or share demographic and socio-economic characteristics are more likely to be friends and may engage in similar collective actions—for example, because they belong to the same national union. To capture these alternative linkages, we construct several county-to-county similarity measures. Specifically, we define a population-weighted similarity index as

$$PSM_{ij} = \frac{SM_{ij} \times \text{Pop}_j}{\sum_{j \in J} SM_{ij} \times \text{Pop}_j},$$

where $SM_{ij}$ represents a particular measure of similarity between counties $i$ and $j$.

A natural starting point is geographic proximity, defined as whether two counties share a border ($SM_{ij} = 1$ if county $j$ borders county $i$). Since geographic proximity is strongly correlated with the strength of social network ties (Bailey et al., 2018), controlling for it allows us to separate the role of social networks from common geographic factors.

We also consider occupational and industrial similarity, which may be correlated with both the likelihood of friendship and similar unionization or strike behaviors. In addition, demographic and socio-economic similarity, such as similarities in education, ethnicity, and political preferences, can shape both social connections and collective action tendencies. We quantify these similarities using the Euclidean distance between counties in the relevant category shares, with further details provided in Appendix D.

Using these similarity measures, we construct alternative exposure variables that capture strike activity in counties that are similar to county $i$ along various dimensions. For county $i$ in month $t$, exposure mediated by a given similarity measure is defined as

$$PSMST_{it} = \sum_{j \neq i} PSM_{ij} \times ST_{jt},$$

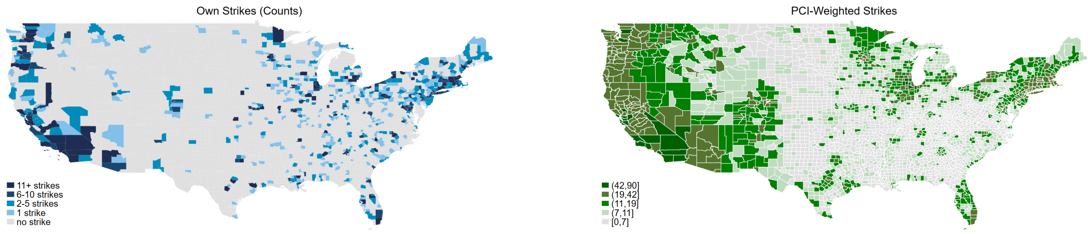
where $PSM$ refers to a specific similarity or proximity index, such as the border-contiguity dummy or occupational similarity.

### 3.2 Descriptive Statistics

Figure 3 presents a pair of county-level maps comparing the total number of strikes and PCI weighted strikes, across the United States from 2021 to 2024. Panel (a) shows that strikes are highly concentrated along the East and West Coasts, with notable clusters in parts of the Midwest. In contrast, much of the South, Southwest, and interior West see little to no strike activity. This spatial distribution aligns with regions of higher population density and economic activity. Panel (b) reveals a similar geographic pattern in PCI weighted strikes, indicating that areas with more strikes also tend to have higher social network exposure to strikes.

Figure 3: Comparison of Total Strikes and PCI-Weighted Strikes by County, 2021-2024

(a) Total Strikes (b) PCI-Weighted Strikes Exposure




*Note*: This figure compares total strikes by county with PCI-weighted strikes exposure. In the maps, deeper shades indicate a greater number of strikes. For the PCI-weighted strikes exposure map, strikes are categorized by percentile. Given that most US counties in 2021-2024 saw no strikes, darker shades begin at the 75th percentile. Data is sourced from the ILR Labor Action Tracker for the years 2021-2024.

Figure A1 compares the geographic distribution of several types of exposure to strikes, including industrial, occupational, ethnic, educational, political, and border-based exposures. Visual inspection suggests no strong spatial correlation between total strike incidence and most of these alternative exposure types, except for some overlap in areas with higher border exposure. To account for potential confounding effects, one of our empirical specifications, discussed next, includes controls for all six exposure measures, including border exposure. Overall, none of these alternative patterns are statistically significant when compared to the effect of social network exposure to strikes.

### 3.3 Main Specification

The previous section shows a clear visual correlation between strike activity in a county and exposure to strikes in other counties through Facebook connections. In this section, we formally estimate this relationship.

Because our dependent variable—strikes per 1,000 residents in a county-month—is non-negative, positively skewed, and equal to zero in 98% of county-months (Figure 2, panel (a)), OLS regression may yield inconsistent estimates in the presence of heteroskedasticity (Silva and Tenreyro, 2006). Therefore, following common practice in similar settings, we use a Poisson regression model and estimate it by Poisson Pseudo Maximum Likelihood, which requires only correct specification of the conditional mean and is consistent for any non-negative dependent variable, including rates (Silva and Tenreyro, 2006; Correia et al., 2020).

The base specification takes the following form:

$$y_{it} = \exp\left(\beta_1 \sum_{k=t-1}^{t-m} y_{ik} + \beta_2 PCIST_{it} + \beta_3 \sum_{k=t-1}^{t-n} PCIST_{ik} + C_i + T_t\right) \times \epsilon_{it}, \tag{1}$$

where $y_{it}$ is the number of strikes per 1,000 residents in own county $i$ in month $t$, $PCIST_{it}$ is the PCI-mediated exposure to strikes in other counties in month $t$, and $C_i$ and $T_t$ are county and month fixed effects, respectively. Normalizing the outcome by population prevents populous counties from exerting disproportionate influence on the estimates: with a raw count as the outcome, large counties mechanically display both higher strike counts and larger fluctuations around their fitted values, so the estimation would be driven disproportionately by their residual variation rather than by the typical county's response to exposure. Exposure, in contrast, enters as a raw count of strike events, since each strike in a socially connected county is a discrete event whose salience to observers does not shrink with the sending county's population—so a large county appropriately transmits more signals to its connections, but derives no mechanical advantage as a receiver.

In addition to contemporaneous exposure, we are also interested in how past strike activity, both within the county and in socially connected counties, influences current strike incidence. Specifically, we include $\sum_{k=t-1}^{t-m} y_{ik}$, the sum of strikes per 1,000 residents in county $i$ over the previous $m$ months, and $\sum_{k=t-1}^{t-n} PCIST_{ik}$, the sum of PCI-weighted strike exposure from connected counties over the previous $n$ months. There is no universally accepted standard for the appropriate lag length; we select $m = 2$ and $n = 4$ based on the sensitivity analyses in Tables B2 and B3, discussed in Section 4.1. We expect $n \geq m$, as it likely takes longer for information about strikes in other counties to diffuse through networks compared to learning about strikes within one's own county. To make the coefficients more comparable and easier to interpret, each right-hand-side variable is divided by its sample standard deviation, with the lagged terms standardized as sums.

All coefficients in Equation (1) represent semi-elasticities. A one-standard-deviation increase in a regressor changes the expected strike rate by $100 \times (e^{\beta} - 1)$ percent, or approximately $100 \times \beta$ percent for the coefficient magnitudes we estimate. When quoting percentage effects in the text we use this approximation, which slightly understates the exact figure: a coefficient of 0.16, for example, corresponds to an increase of about 17 percent. We are primarily interested in $\beta_2$ and $\beta_3$. The coefficient $\beta_2$ captures the contemporaneous relationship between strike activity in county $i$ and strike exposure in connected counties. Because county fixed effects are included, this relationship is identified from within-county variation over time and should therefore be interpreted as the effect of changes in exposure to strikes in connected counties on changes in strike activity within a county, rather than as an explanation of persistent differences in strike levels across counties. The coefficient $\beta_3$ captures the relationship with lagged

PCI-weighted exposure, reflecting the possibility that information about strikes in other counties influences strike activity with a delay. Intuitively, contemporaneous effects may capture real-time information flows and rapid mobilization through social networks, whereas lagged effects may reflect a slower process of learning, planning, and organizing strike actions.

Including the lagged number of own-county strikes in Equation (1) also helps account for serial correlation and similar strike dynamics that may unfold across socially connected counties over time. For example, if $\beta_1$ has the same sign and a similar magnitude as $\beta_3$, this may indicate that lagged own-county strikes and lagged PCI-weighted strikes are driven by common underlying factors, such as broader economic trends. In contrast, observing different signs or magnitudes would suggest that these variables capture distinct effects on current strike activity.

An important implication of estimating Equation (1) using PPML with county fixed effects is that counties that never experience a strike during the sample period do not contribute to identification. In our data, 2,503 of the 3,141 counties experience no strikes between 2021 and 2024 and are therefore omitted from the estimation sample. Consequently, the coefficients in Equation (1) should be interpreted as describing how changes in exposure to strikes in socially connected counties affect strike activity among counties that experience at least one strike during the sample period. In Section 4.2, we revisit this issue by estimating alternative specifications that include the full sample of counties.

### 3.4 Identification and Additional Specifications

It is important to consider why the relationship between $y_{it}$ and $PCIST_{it}$, as well as $\sum_{k=t-1}^{t-n} PCIST_{ik}$, may not be causal. It is beyond our ability to fully control for all time-varying county characteristics that are not observed in the data but may jointly affect both strike activity within a county and strike activity in socially connected counties. These unobserved factors can influence both the number of strikes and the likelihood of forming social connections.

The strongest predictor of Facebook friendship ties is geographic proximity, as documented by Bailey et al. (2018), and Figure 3 provides evidence of geographic clustering in strike activity. To account for this pattern, we control for exposure to strikes in geographically proximate counties. Political similarity is another relevant dimension: political leanings are predictive of Facebook friendship ties (Bailey et al., 2024) and are associated with support for strikes[19] and labor unions.[20] We therefore also control for exposure to strikes in politically similar counties. In addition, similarities in occupation, industry, education, and ethnicity may be related to both social network formation and strike participation. We account for these dimensions by including exposure measures constructed using each of these similarity characteristics. Accordingly, we augment Equation (1) with the vector of similarity-weighted strike exposures, $PSMST_{it}$, and their four-month lagged sums.

A related concern is that counties may experience correlated strike activity because local chapters of the same national or international unions are present in both, sharing strike funds, staff, and bargaining strategies, rather than because their residents are socially connected. To address this organizational channel directly, we construct union-connectedness indices (UCI) that link two counties to the extent that the same unions maintain a footprint in both—measured alternatively by union presence, the number of locals, and membership—and we examine strike exposure weighted by these indices, both on its own and alongside PCI-weighted exposure in Section 4.2. Appendix F details the construction of these measures.

Peer Learning and the Spread of Strikes

We further examine variation in $\beta_2$ and $\beta_3$ in Equation (1), which capture contemporaneous and lagged exposure to strikes in socially connected counties, respectively, by estimating the equation separately for education, healthcare, other service-providing industries, and goods-producing industries. We first examine within-industry relationships. Workers may be more likely to update their expectations about strike success or settlement outcomes when they observe strikes involving workers in similar industries, and industry similarity may also strengthen solidarity. We then examine cross-industry spillovers, since strikes in one sector may transmit information or mobilize support among workers in other sectors through broader social or union networks.

We also examine the intensive margin of the strike contagion effect: the scale of a strike depends on the number of willing participants, which may itself respond to strike activity in other counties. In Section 4.5 we therefore re-estimate Equation (1) replacing the number of strikes with the number of strike participants in all strike and exposure variables, so that the outcome becomes participants per 1,000 residents.

Testing Information Discovery vs. Behavioral Channels

[19] Gallup, "Unions Seen as Strengthening in U.S.," https://news.gallup.com/poll/510281/unions-strengthening.aspx.

[20] Pew Research Center, "Labor Unions," https://www.pewresearch.org/politics/2024/02/01/labor-unions/.

First, to assess the relevance of the information channel, we examine the social media presence of the unions involved in strikes. We posit that unions with larger followings may be more effective at disseminating information through social networks simply because of their broader reach: a group of 10,000 people is likely connected to more individuals than a group of 100. In addition, the involvement of a larger union may signal to workers that more people are on the same side of the bargaining table. We test these possibilities by augmenting Equation (1) with the PCI-weighted average number of Facebook followers of the unions involved in the strikes to which a county is exposed, as well as its interaction with the unweighted strike-exposure measure. As an alternative specification, we weight each strike by the number of Facebook followers of its leading union. These specifications are detailed in Equations (2) and (3) in Appendix D.

Second, to distinguish between the information and behavioral channels, we exploit differences in strike outcomes. If settlements reached elsewhere lead to an increase in strike activity in a county, this would be consistent with the information channel. As discussed earlier, unions often publicize successful settlements through various communication channels, including social media platforms such as Facebook. Consistent with interpreting settlements as positive signals, the worker-side announcements of the settlements in our data—which we locate for 92.8% of confirmed deals—overwhelmingly express satisfaction with the terms and frame the outcome as a full or partial victory, as shown in Tables B15 and B16. Strikes that result in settlements are expected to provide stronger informational signals and lead to more positive updates about the expected benefits of striking than strikes that end without a deal. To test this hypothesis, we augment Equation (1) by including variables that capture both contemporaneous and lagged counts of settlements observed in PCI-weighted counties. This allows us to compare the effects of strikes with and without settlements on subsequent strike activity.

In contrast, if settlements reached elsewhere have no differential impact on local strike activity, this would suggest the presence of behavioral channels, particularly solidarity, rather than informational updates. Observing strikes by friends and colleagues in other locations may motivate workers to participate in the broader struggle, even when those strikes do not succeed. This mechanism is distinct from the information channel in that it should operate regardless of whether the observed strikes result in a settlement.

Third, we examine whether the strength of unions themselves conditions network transmission, using the policy and administrative environments introduced above. Because state right-to-work laws bind only in the private sector, while after *Janus* the public sector is effectively right-to-work in every state, variation in right-to-work status is more informative about private-sector unions, with a caveat that RTW status correlates with restrictiveness of public sector labor-law environment through policies other than security agreements. We therefore proceed as follows: we interact PCI-weighted exposure with an indicator for right-to-work states; we estimate the specification separately for private- and public-sector strikes and compare the role of RTW status between the public and private sectors. If weaker unions increase workers' reliance on peer networks, exposure effects should be stronger in right-to-work states, and that gradient should be concentrated in the private sector, where the laws apply directly.

Overview of Estimated Specifications

All results in the remainder of the paper are variants of Equation (1), modified along one or more of three dimensions. First, the *outcome*: in Sections 4.2–4.5, in addition to strikes per 1,000 residents, we estimate specifications in which $y_{it}$ is restricted to independent (single-location) strikes, replaced by an indicator for any strike, or replaced by the number of strike participants per 1,000 residents. Second, the *exposure measure*: in Section 5, we augment PCI-weighted exposure with the six similarity-weighted exposure measures; replace it, separately, with union-connectedness (UCI) exposure built from the county footprints of national unions; and weight PCI exposure by the Facebook following of the unions involved, as defined in Equations (2) and (3) in Appendix D. We also divide exposure into strikes that did and did not end in settlement. Third, the *estimator and sample*: in Sections 4.2, 4.4, and 5, alongside PPML estimates using counties that experience at least one strike, we report linear probability, OLS, first-differenced, and Negative Binomial models using the full county panel. We also estimate separate specifications by industry, for public- and private-sector strikes, and across county subsamples defined by population and other characteristics. Each variant is defined when first introduced, and none changes the fixed-effects structure or the standardization of the right-hand-side variables unless explicitly noted.

# 4 Main Results

This section examines the relationship between social network exposure and strike incidence. We first document how network exposure is associated with strike activity across counties and over time. In Section 5, we then explore the potential mechanisms underlying this observed relationship.

### 4.1 Social Network Exposure to Strikes and Own County Strikes

Table 2 reports the baseline relationship between social-network exposure to strikes and local strike incidence. Column (1) includes contemporaneous PCI-weighted exposure with county and month–year fixed effects, while column (2) estimates Equation (1) by adding two-month own-county strike activity and four-month lagged PCI exposure. Column (3) further controls for contemporaneous and lagged exposure through geographic, political, occupational, industrial, educational, and ethnic networks, and column (4) adds interactions between 2020 county characteristics and year fixed effects.

Across all specifications, contemporaneous PCI-weighted exposure to strikes in other counties is strongly correlated with local strike activity. A one-standard-deviation increase in contemporaneous PCI-weighted strike exposure is consistently associated with approximately 16% increase in expected local strike incidence. The stability of the estimates across specifications indicates that this relationship is not explained by persistent county characteristics, aggregate monthly shocks, exposure through alternative proximity networks, or differential changes associated with counties' initial characteristics.

The lag lengths in the baseline specification are guided by the sensitivity analyses in Tables B2 and B3. Table B2 shows a broadly similar negative association between cumulative own-county strike activity and current strike incidence across one- to six-month horizons, motivating our use of the two-month sum as the preferred control for recent local strike dynamics. Table B3 shows that lagged PCI estimates are positive across all horizons but are imprecisely estimated. We retain the four-month window to capture a medium-run period"serve and respond to strikes in socially connected counties.

The four-month lagged PCI coefficient is positive in columns (2)–(4) of Table 2, with point estimates implying increases of approximately 7% to 13% in expected strike incidence following a one-standard-deviation increase in lagged exposure, but it is imprecisely. Therefore, based on the baseline results, we read the lagged estimates as at most suggestive of slower learning or organizing responses: the evidence points to rapid diffusion of information, attention, or mobilization through social networks, with little indication of a persistent cumulative relationship.

Another consistent result in Table 2 is the negative association between current strike incidence and recent own-county strikes. Across columns (2)–(4), a one-standard-deviation increase in the two-month sum of own-county strikes is associated with approximately 2% lower expected current strike incidence, consistent with short-run mean reversion through demand

Table 2: Correlations of Own-County and Distance-Weighted Exposure to Strike Incidence

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| PCI-Weighted Strikes of Other Counties, Same-Month | 0.16*** | 0.16*** | 0.15*** | 0.14*** |
| | (0.03) | (0.03) | (0.05) | (0.05) |
| Lagged Total Strikes in Own County, 2-Month Sum | | -0.02* | -0.02* | -0.02** |
| | | (0.01) | (0.01) | (0.01) |
| Lagged PCI-Weighted Strikes of Other Counties, 4-Month Sum | | 0.07 | 0.13 | 0.10 |
| | | (0.05) | (0.09) | (0.10) |
| Education-Weighted Strikes, Same-Month | | | 0.04 | 0.04* |
| | | | (0.02) | (0.02) |
| Ethnicity-Weighted Strikes, Same-Month | | | 0.05 | 0.09 |
| | | | (0.06) | (0.07) |
| Political-Weighted Strikes, Same-Month | | | 0.01 | 0.01 |
| | | | (0.02) | (0.02) |
| Bordering-County Strikes, Same-Month | | | 0.01 | 0.01 |
| | | | (0.03) | (0.03) |
| Occupation-Weighted Strikes, Same-Month | | | -0.10 | -0.13 |
| | | | (0.10) | (0.10) |
| Industry-Weighted Strikes, Same-Month | | | -0.08 | -0.10 |
| | | | (0.11) | (0.11) |
| Lagged Education-Weighted Strikes, 4-Month Sum | | | -0.01 | 0.02 |
| | | | (0.04) | (0.04) |
| Lagged Ethnicity-Weighted Strikes, 4-Month Sum | | | -0.10 | -0.04 |
| | | | (0.09) | (0.06) |
| Lagged Political-Weighted Strikes, 4-Month Sum | | | -0.03 | -0.04 |
| | | | (0.04) | (0.04) |
| Lagged Bordering-County Strikes, 4-Month Sum | | | -0.05 | -0.04 |
| | | | (0.06) | (0.06) |
| Lagged Occupation-Weighted Strikes, 4-Month Sum | | | 0.22** | 0.20* |
| | | | (0.10) | (0.11) |
| Lagged Industry-Weighted Strikes, 4-Month Sum | | | 0.03 | 0.05 |
| | | | (0.11) | (0.12) |
| County Fixed Effects | x | x | x | x |
| Month–Year Fixed Effects | x | x | x | x |
| Observations | 30624 | 27236 | 27236 | 27016 |

*Notes:* Column (1) includes only contemporaneous PCI-weighted strike exposure; column (2) adds own-county lagged strikes and the four-month lag of PCI-weighted exposure; column (3) adds the other distance-weighted proximity exposures (education, ethnicity, politics, bordering-county, occupation, and industry); column (4) adds 2020 county characteristics (demographics, education, income, age, density, vote share, urban share) interacted with year. The other proximity measures and their lags are jointly insignificant: a Wald test of the twelve proximity terms yields $\chi^2(12) = 12.18$ ($p = 0.43$) in column (3) and $\chi^2(12) = 12.26$ ($p = 0.42$) in column (4). PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations. The number of observations is smaller than the full county-month panel because the estimator retains only counties that experience at least one strike during the sample period. Specifications with additional controls may lose further observations to missing values. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

resolution, organizational exhaustion, or worker fatigue (Mauro, 1982; Schnell and Gramm, 1987). The negative own-county lag coefficient's contrast with the positive lagged PCI estimates is difficult to reconcile with a common aggregate shock generating serially correlated strike activity across connected counties. Instead, recent own-county strikes may reflect local

Table 3: Proximity Networks and Strike Diffusion: Alone and Conditional on PCI

| | (1) Educ. | (2) Ethnic. | (3) Polit. | (4) Border | (5) Occ. | (6) Ind. |
|---|---|---|---|---|---|---|
| *Panel A: proximity network entered alone* | | | | | | |
| Proximity network, same-month | 0.044** | 0.098* | 0.025 | 0.082*** | -0.069 | 0.014 |
| | (0.022) | (0.058) | (0.020) | (0.016) | (0.083) | (0.089) |
| Proximity network, 4-month lag | 0.024 | -0.048 | -0.011 | 0.019 | 0.258*** | 0.160 |
| | (0.035) | (0.086) | (0.039) | (0.037) | (0.090) | (0.102) |
| Own strikes, 2-month lag | -0.021* | -0.020* | -0.020* | -0.021* | -0.020* | -0.020* |
| | (0.012) | (0.011) | (0.011) | (0.011) | (0.012) | (0.011) |
| Observations | 27236 | 27236 | 27236 | 27236 | 27236 | 27236 |
| *Panel B: proximity network with PCI exposure* | | | | | | |
| Proximity network, same-month | 0.032 | 0.043 | 0.016 | 0.007 | -0.095 | -0.098 |
| | (0.023) | (0.065) | (0.020) | (0.028) | (0.085) | (0.090) |
| Proximity network, 4-month lag | -0.007 | -0.096 | -0.026 | -0.049 | 0.228** | 0.078 |
| | (0.038) | (0.091) | (0.040) | (0.058) | (0.091) | (0.108) |
| PCI exposure, same-month | 0.156*** | 0.155*** | 0.159*** | 0.149*** | 0.159*** | 0.164*** |
| | (0.029) | (0.029) | (0.029) | (0.049) | (0.029) | (0.030) |
| PCI exposure, 4-month lag | 0.070 | 0.088 | 0.073 | 0.120 | 0.060 | 0.066 |
| | (0.051) | (0.055) | (0.051) | (0.078) | (0.051) | (0.055) |
| Own strikes, 2-month lag | -0.021* | -0.021* | -0.021* | -0.021* | -0.021* | -0.021* |
| | (0.012) | (0.012) | (0.011) | (0.011) | (0.012) | (0.011) |
| Observations | 27236 | 27236 | 27236 | 27236 | 27236 | 27236 |

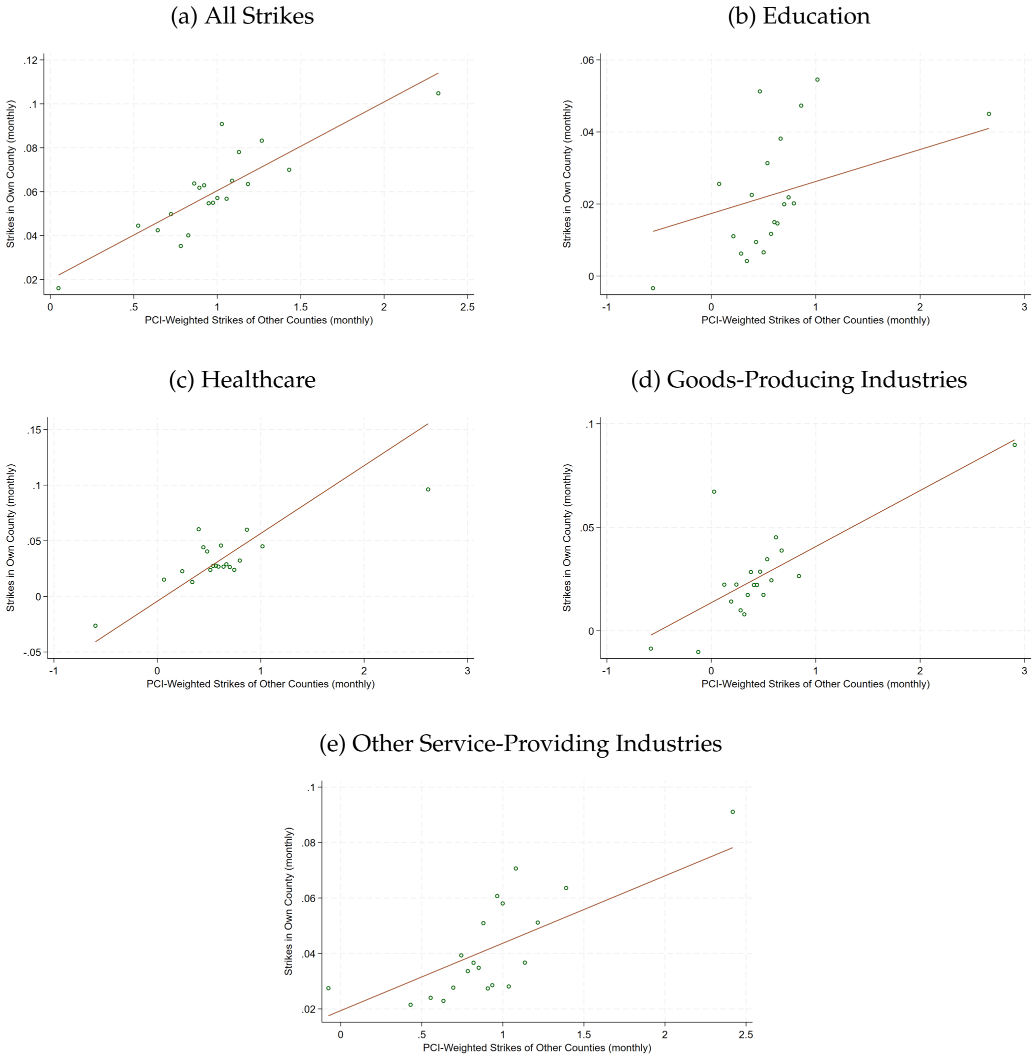
*Notes:* Each column is a separate PPML regression for one distance-based proximity network (education, ethnicity, politics, bordering-county, occupation, industry). Panel A enters that network's same-month and four-month-lag exposure with own-county lagged strikes and fixed effects only; Panel B adds PCI-weighted strike exposure (same-month and four-month lag). PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

resolution or exhaustion, whereas earlier strikes in connected counties may convey information or encourage subsequent mobilization.

A Wald test in Table 2 shows that the alternative proximity measures and their lags are jointly insignificant in columns (3) and (4): $\chi^2(12) = 12.18$ ($p = 0.43$) and $\chi^2(12) = 12.26$ ($p = 0.42$), respectively. Table 3 therefore examines each network separately: Panel A estimates the baseline equation with each alternative proximity measure replacing PCI exposure, while Panel B adds PCI exposure to the same specification. Educational, ethnic, bordering-county, and lagged occupational exposure are significant when entered alone, but these relationships largely disappear after PCI exposure is included. Conditional on PCI exposure, no contemporaneous alternative measure remains significant, while contemporaneous PCI exposure remains positive and significant in every specification.

Finally, Figure 4 visually confirms the contemporaneous relationship after county and month–year fixed effects are partialled out. Panel (a) shows a clear positive association be-

Figure 4: Monthly Correlations of Own-County Strikes and PCI-Weighted Strike Exposure




*Notes*: This figure illustrates the conditional relationship between own-county strike incidence and PCI-weighted strike exposure using binned scatter plots at the monthly level. Each plot partials out county and month–year fixed effects. Variables are standardized by their sample standard deviations, and the full estimation sample is plotted. Panel (a) presents the relationship for all strikes, while panels (b) through (e) present the corresponding relationships for education, healthcare, goods-producing industries, and other service-providing industries. The data are sourced from the ILR Labor Action Tracker for the years 2021–2024.

tween aggregate own-county strike incidence and PCI-weighted exposure, while panels (b)–(e) show similar positive fitted relationships in education, healthcare, other service-providing, and goods-producing industries, reinforcing that the regression results are not driven by a single sector. The rightmost bin pools the small number of very high-exposure county-months; it has high leverage but a small residual, so it matters primarily for precision, as much of the identifying variation lies in this range. Dropping these observations increases both the estimated contemporaneous coefficient and its standard error, and the estimate remains statistically different from zero.

Table 4: Correlation of Own-County and Network Exposure to Strikes, Labor Market Controls

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Unemployment rate, lag 1–2 | -0.01 | | | -0.00 | | | -0.07 | | |
| | (0.07) | | | (0.08) | | | (0.12) | | |
| Bartik wage shock, 2-mo. change | | -5.91 | | | -5.47 | | | -5.43 | |
| | | (3.67) | | | (3.94) | | | (4.08) | |
| Bartik employment shock, 2-mo. change | | | -0.38 | | | -0.35 | | | -0.36 |
| | | | (0.31) | | | (0.32) | | | (0.38) |
| Lagged Total Strikes in Own County, 2-Month Sum | | | | -0.02* | -0.02* | -0.02* | -0.02* | -0.02* | -0.02* |
| | | | | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| PCI-Weighted Strikes of Other Counties, Same-Month | | | | 0.16*** | 0.16*** | 0.16*** | 0.16*** | 0.16*** | 0.16*** |
| | | | | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Lagged PCI-Weighted Strikes of Other Counties, 4-Month Sum | | | | 0.07 | 0.07 | 0.06 | 0.07 | 0.07 | 0.07 |
| | | | | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) |
| PCI-Weighted Unemployment Rate, Same-Month | | | | | | | 0.17 | | |
| | | | | | | | (0.26) | | |
| Lagged PCI-Weighted Unemployment Rate, 4-Month Avg | | | | | | | 0.09 | | |
| | | | | | | | (0.24) | | |
| PCI Bartik wage shock | | | | | | | | -0.57 | |
| | | | | | | | | (0.90) | |
| PCI Bartik wage shock, 4-mo. change | | | | | | | | -1.00 | |
| | | | | | | | | (10.90) | |
| PCI Bartik employment shock | | | | | | | | | -0.76 |
| | | | | | | | | | (0.80) |
| PCI Bartik employment shock, 4-mo. change | | | | | | | | | 0.29 |
| | | | | | | | | | (0.72) |
| County Fixed Effects | x | x | x | x | x | x | x | x | x |
| Month–Year Fixed Effects | x | x | x | x | x | x | x | x | x |
| Observations | 28842 | 29210 | 29210 | 26884 | 27236 | 27236 | 26884 | 27236 | 27236 |

*Notes:* Columns (1)–(3) add own-county labor-market controls (unemployment and the Bartik wage/employment shocks) to the baseline; columns (4)–(6) add PCI-weighted strike exposure; columns (7)–(9) add PCI-weighted labor-market conditions in socially connected counties. PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations, except the own-county unemployment rate, which is in percentage points. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## 4.2 Robustness of the Main Results

### Labor Market Conditions

One concern is that the relationship between social network exposure and local strike activity may reflect shared labor market conditions across connected counties. Table 4 addresses this by controlling for the county's two-month average unemployment rate and two-month changes in Bartik wage and employment shocks, motivated by evidence linking labor market conditions to strike incentives (Ashenfelter and Johnson, 1969; Card, 1990; McConnell, 1990).[21] Columns (1)–(3) include own-county labor market conditions, columns (4)–(6) add contemporaneous and lagged PCI-weighted strike exposure, and columns (7)–(9) further add PCI-weighted labor market conditions in connected counties.

The labor market measures are generally insignificant both within counties and across socially connected counties, unlike findings in some earlier studies. This pattern is consistent with Pezold et al. (2023), who similarly find that labor demand shocks have little independent

[21] Specifications that include labor-market controls are estimated on 3,132 counties rather than the full 3,141. The BLS Local Area Unemployment Statistics carry no pre-2022 Connecticut county series—Connecticut replaced its eight counties with nine planning regions for federal statistical purposes—so all eight Connecticut counties drop from these columns, as does Kalawao County, Hawaii, which has no separate employment series. No other specification in the paper is affected.

effect on strike activity, and that the role of perceived labor market tightness is conditional on the strength of perceived peer support for collective action—in line with our interpretation that network factors, rather than local economic conditions alone, are central to strike contagion. One possible explanation is that our baseline specification includes county and month–year fixed effects, whereas many previous studies rely on state-level or cross-sectional variation; our estimates therefore use more limited within-county deviations from common monthly trends. Moreover, the effect of the remaining within-county variation is theoretically ambiguous and may net out: wage growth associated with positive industry shocks, for example, may reduce the grievances that lead to strikes while simultaneously increasing the bargaining power that raises the expected payoff to striking. In contrast, contemporaneous PCI-weighted strike exposure remains positive and significant across columns (4)–(9), suggesting that its relationship with local strike activity is unlikely to be explained solely by shared labor market conditions.

**Independent Strikes**

It is also possible that correlated strike activity across counties may reflect large unions coordinating multi-location campaigns rather than decentralized diffusion through social networks. For example, in early 2021, the Retail, Wholesale and Department Store Union (RWDSU) organized actions among Amazon workers at 50 sites across 24 states. We therefore define independent strikes as single-location events, excluding all multi-location strikes. In Table 5, columns (1) and (2) use independent strikes as the outcome, with exposure based on independent and all strikes, respectively, while column (3) uses all strikes as the outcome and independent strikes for exposure.

Table 5: Correlation of Own-County and Network Exposure to Independent Strikes

| | Outcome: Independent strikes | | Outcome: All strikes |
|---|---|---|---|
| | (1)<br>Indep. exp. | (2)<br>All exp. | (3)<br>Indep. exp. |
| Lagged Total Strikes in Own County, 2-Month Sum | -0.013 | -0.013 | -0.021* |
| | (0.012) | (0.012) | (0.011) |
| PCI-Weighted Strikes of Other Counties, Same-Month | 0.071* | 0.062 | 0.077** |
| | (0.042) | (0.039) | (0.033) |
| Lagged PCI-Weighted Strikes of Other Counties, 4-Month Sum | 0.100 | 0.134** | 0.097 |
| | (0.061) | (0.056) | (0.062) |
| Observations | 24376 | 24376 | 27236 |

*Notes:* The grid crosses the strike definition on both sides of the regression. In columns (1)–(2) the outcome is restricted to independent (single-location) strikes, with exposure measured to independent strikes in column (1) and to all strikes in column (2); in column (3) the outcome is all strikes while exposure is measured to independent strikes only. PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5 shows that a one-standard-deviation increase in contemporaneous PCI-weighted exposure is associated with approximately 6%–7% higher expected strike incidence, similar in magnitude across all three specifications, statistically significant in two of them and marginally insignificant in the third, and smaller than the baseline measure in Table 2. Lagged exposure is significant when independent strikes are used as the outcome but not when only the exposure measure is restricted to independent strikes. These results reduce concerns that the baseline relationship is driven by coordinated multi-location campaigns, while the smaller and less precise estimates likely reflect the lower frequency and limited variation of independent strikes.

**Union Connectedness**

Another concern is that correlated strike activity may reflect organizational ties among local chapters of the same national or international union rather than broader social connectedness. We therefore construct three union-connectedness indices (UCI) based on shared union presence, local-chapter counts, and membership, with details explained in Appendix F. Table 6 relates own-county strike incidence to contemporaneous and four-month-lagged UCI-weighted exposure: columns (1)–(3) include each UCI measure with recent own-county strikes, while columns (4)–(6) additionally control for contemporaneous and lagged PCI-weighted exposure.

Contemporaneous UCI-weighted exposure is insignificant across all specifications, while four-month lagged exposure is positive and significant for all three connectedness measures. After adding the PCI measures, the lagged UCI estimates become somewhat smaller: the local-chapter count and membership measures remain significant, while the presence-based measure is marginally insignificant. Meanwhile, contemporaneous PCI-weighted exposure remains positive and significant, indicating that union organizational ties matter over a longer horizon but do not explain the contemporaneous PCI relationship.

**Alternative Specifications**

We also estimate alternative specifications of Equation (1) using different functional forms and estimation approaches. The first addresses whether time-varying unobserved county factors are correlated with both local strike activity and PCI-weighted exposure. Table B4 reports OLS estimates using standardized month-to-month changes in the outcome and all explanatory variables. The contemporaneous change in PCI-weighted exposure remains positive and significant, showing that the contemporaneous relationship persists when identification relies on changes rather than levels.

Table 6: Union Connectedness (UCI) and Strike Diffusion

| | UCI only | | | With PCI | | |
|---|---|---|---|---|---|---|
| | (1) Presence | (2) Count | (3) Memb. | (4) Presence | (5) Count | (6) Memb. |
| Own strikes, 2-month lag | -0.02** | -0.02** | -0.02** | -0.02** | -0.02** | -0.02** |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| UCI, same-month | 0.20 | 0.07 | 0.04 | 0.15 | -0.01 | 0.01 |
| | (0.23) | (0.44) | (0.19) | (0.26) | (0.44) | (0.19) |
| UCI, 4-month lag | 0.42* | 0.81*** | 0.41*** | 0.34 | 0.74** | 0.35** |
| | (0.24) | (0.30) | (0.15) | (0.24) | (0.31) | (0.16) |
| PCI, same-month | | | | 0.15*** | 0.15*** | 0.15*** |
| | | | | (0.03) | (0.03) | (0.03) |
| PCI, 4-month lag | | | | 0.03 | 0.01 | 0.03 |
| | | | | (0.06) | (0.06) | (0.05) |
| County FE | x | x | x | x | x | x |
| Month–Year FE | x | x | x | x | x | x |
| Observations | 27,236 | 27,236 | 27,236 | 27,236 | 27,236 | 27,236 |

*Notes:* Specifications in columns. The union-connectedness (UCI) network links two counties to the extent that the same national/international unions maintain locals in both; the three measures weight that co-membership by union *presence*, *number of locals* (count), or *membership*, all population-weighted like the other proximity networks. Each shared affiliation is additionally weighted by its inverse document frequency, $\log(N/n_a)$, where $N$ is the number of counties in the network and $n_a$ the number in which affiliation $a$ has a local, so that sharing a geographically rare union counts for more than sharing a near-ubiquitous one; the un-weighted counterparts give the same sign pattern and similar magnitudes with wider standard errors, and leave the contemporaneous PCI coefficient unchanged. Columns 1–3 enter each UCI measure alone (with the own-county strike lag); columns 4–6 add same-month and lagged PCI (social-network) exposure. PPML (`ppmlhdfe`) with county and month–year fixed effects, standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The second alternative specification uses a Negative Binomial model, which allows the conditional variance of strike incidence to exceed its conditional mean. Two features of this estimator distinguish it sharply from the baseline. First, the negative binomial likelihood is defined only over non-negative integers, so the outcome must be the raw strike count rather than strikes per 1,000 residents, with population entering as an exposure offset; because the outcome is then in levels, larger counties generate larger residuals and receive disproportionate weight. Second, the model does not converge with county fixed effects, so Table B5 includes state and month–year fixed effects instead. Identification therefore no longer comes from within-county variation over time but is largely cross-sectional, and weighted toward populous counties. Table B5 accordingly reports both samples: columns (1)–(2) restrict to ever-strike counties for comparability with the baseline, while columns (3)–(4) use the full county panel. The full panel is the appropriate sample here—the ever-strike restriction exists because PPML with county fixed effects drops never-strike counties through separation, which does not apply under state fixed effects—and it is the sample used by the linear probability and OLS specifications alongside which this table sits. In the full panel, contemporaneous PCI-weighted exposure remains positive and strongly significant with and without controls. Two features of the estimates still reflect the cross-sectional identification. Lagged own-county strike activity switches to positive, and lagged PCI-weighted exposure is negative throughout. Because network exposure is constructed from *other* counties' strikes, a county's own strike activity and its exposure are negatively sorted across space, and the between-county components of contemporaneous and lagged exposure are almost perfectly collinear with one another; the negative lag therefore reflects this cross-sectional sorting rather than a diffusion effect operating with delay. The contemporaneous result is thus robust to the count-model functional form, while the timing pattern is informative only under the within-county identification that PPML with county fixed effects provides.

The third alternative specification tests sensitivity to the estimation sample and functional form. Because never-strike counties do not contribute to the baseline PPML estimates with county fixed effects, Table 7 reports linear probability models for any strike in Panel A and OLS models for the number of strikes in Panel B, varying the strike definition and using both the full and ever-strike samples. Contemporaneous PCI exposure is positive and significant in most specifications, while lagged exposure is generally significant in the linear probability models but not in OLS. Thus, the contemporaneous relationship is not confined to the baseline count model, the ever-strike sample, or multi-location strikes; together with the full-panel negative binomial estimates in Table B5, it holds across linear and count specifications and on both the full and ever-strike samples.

As a follow-up to the previous linear specifications, Table B12 decomposes each model's $R^2$ across groups of explanatory variables. Fixed effects account for most of the explained variation in strike outcomes, while PCI-weighted exposure contributes 1.7%–3.3% in the OLS models and 8.9%–10.2% in the linear probability models, exceeding the contribution of the labor market controls. Thus, PCI exposure helps explain both the extensive and intensive margins of strike incidence after accounting for fixed effects, labor market conditions, and differential changes associated with counties' initial characteristics.

The fourth alternative specification replaces cumulative lags with separate monthly lags through six months. Table B6 confirms the robust contemporaneous relationship, while lagged own-county strike activity remains generally negative. The individual monthly PCI lags, which are highly collinear by construction, are, with one marginal exception, not separately

Table 7: Linear Models of Strike Incidence: Linear Probability Model and OLS

| | All counties | | | | Ever-strike counties | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Any/all | Ind/ind | Ind/all | Any/ind | Any/all | Ind/ind | Ind/all | Any/ind |
| *Panel A: Linear probability model (any strike, coefficients in percentage points)* | | | | | | | | |
| Own strikes, 2-mo lag | -0.007 | 0.020 | 0.020 | -0.003 | -0.017 | 0.008 | 0.007 | -0.012 |
| | (0.036) | (0.037) | (0.037) | (0.036) | (0.036) | (0.037) | (0.036) | (0.036) |
| PCI exposure, same-month | 1.241*** | 0.357*** | 0.385** | 0.494*** | 1.656*** | 0.171 | 0.234 | 0.313 |
| | (0.179) | (0.128) | (0.157) | (0.136) | (0.306) | (0.233) | (0.281) | (0.247) |
| PCI exposure, 4-mo lag | 0.780*** | 0.737*** | 0.758*** | 0.859*** | 1.005** | 1.164*** | 1.115*** | 1.319*** |
| | (0.253) | (0.163) | (0.187) | (0.211) | (0.412) | (0.257) | (0.298) | (0.342) |
| Observations | 137808 | 137808 | 137808 | 137808 | 27720 | 27720 | 27720 | 27720 |
| *Panel B: OLS (strike count per 1,000 population)* | | | | | | | | |
| Own strikes, 2-mo lag | -0.018*** | -0.012 | -0.012 | -0.018*** | -0.019*** | -0.013 | -0.013 | -0.019*** |
| | (0.006) | (0.008) | (0.008) | (0.006) | (0.006) | (0.008) | (0.008) | (0.006) |
| PCI exposure, same-month | 0.038*** | 0.016*** | 0.017*** | 0.019*** | 0.057*** | 0.019* | 0.017 | 0.026** |
| | (0.007) | (0.006) | (0.006) | (0.006) | (0.014) | (0.011) | (0.011) | (0.011) |
| PCI exposure, 4-mo lag | 0.013 | 0.008 | 0.013 | 0.014* | 0.027 | 0.026* | 0.036** | 0.030 |
| | (0.008) | (0.007) | (0.008) | (0.008) | (0.019) | (0.015) | (0.016) | (0.019) |
| Observations | 137808 | 137808 | 137808 | 137808 | 27720 | 27720 | 27720 | 27720 |

*Notes:* Panel A is a linear probability model (dependent variable equal to 100 if the county experiences at least one strike in the month and zero otherwise, so coefficients are percentage points); Panel B is OLS with the per-capita strike count (per 1,000 population) as the outcome. Column headers are *outcome/exposure*: "Any" is any strike and "Ind" is independent (single-location) strikes on the outcome side, while "all"/"ind" denote exposure measured to all strikes or to independent strikes only. Columns (1)–(4) use all counties; columns (5)–(8) restrict to counties that experience at least one strike over 2021–2024. Every column includes labor-market controls, with county and month–year fixed effects (`reghdfe`); standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

significant; the delayed association is instead captured jointly by the cumulative four-month sums of the baseline specifications. Finally, Appendix A extends the main specification back to 1984–2019 using FMCS work stoppages in decade windows: the contemporaneous network association is present from the 2000s onward and, by 2010–2019, closely matches the baseline estimate in Table B1.

### 4.3 Strikes by Industry

To assess whether the relationship between social network exposure and strike incidence is industry-specific, Table 8 separates PCI-weighted exposure into same- and cross-industry components. Columns (1)–(3) stack county–month observations across education, healthcare, other service-providing, and goods-producing industries under alternative fixed-effect structures, with same-industry exposure capturing strikes in the outcome industry and cross-industry exposure aggregating strikes in the other three. Columns (4)–(7) separately estimate each industry and include exposure from all four industries.

Across columns (1)–(3), a one-standard-deviation increase in contemporaneous same-industry exposure is associated with approximately 13%–15% higher expected strike incidence, and this relationship is stable across fixed-effect structures. Contemporaneous cross-industry exposure and lagged same-industry exposure show little association with local strikes, while lagged cross-industry exposure provides less consistent evidence of delayed spillovers.

The separate industry estimates in columns (4)–(7) show that a one-standard-deviation increase in contemporaneous same-industry exposure is associated with approximately 9%–18% higher expected strike incidence across all four industries, with the largest association in healthcare. Cross-industry relationships are limited, as most off-diagonal contemporaneous and lagged estimates are insignificant. Overall, the relationship is primarily contemporaneous and industry-specific, consistent with stronger responses to peers facing similar grievances, working conditions, and institutional environments.

### 4.4 Heterogeneity

The relationship between PCI-weighted exposure and strike incidence may vary across local contexts. Table 9 examines heterogeneity by county population, recent strike activity, the 2021 COVID period, population density, and urban share. The contemporaneous association is stronger in smaller and less densely populated counties, suggesting that network exposure may matter more in smaller local labor markets. This makes sense as smaller groups face fewer coordination challenges and may therefore find it easier to overcome collective action prob-

Table 8: Diffusion of Strikes by Industry: Same versus Cross Industry, and by Outcome Industry

| | Stacked: same vs. cross | | | By outcome industry | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) Education | (5) Healthcare | (6) Other Serv. | (7) Goods |
| Same-industry PCI exposure, same-month | **0.132**$^{***}$ | **0.146**$^{***}$ | **0.132**$^{***}$ | | | | |
| | **(0.019)** | **(0.020)** | **(0.021)** | | | | |
| Same-industry PCI exposure, 4-month lag | **0.021** | **0.007** | **0.007** | | | | |
| | **(0.034)** | **(0.034)** | **(0.039)** | | | | |
| Cross-industry PCI exposure, same-month | 0.034 | -0.002 | 0.044 | | | | |
| | (0.034) | (0.034) | (0.036) | | | | |
| Cross-industry PCI exposure, 4-month lag | 0.083$^{*}$ | 0.115$^{**}$ | 0.101$^{*}$ | | | | |
| | (0.049) | (0.046) | (0.052) | | | | |
| PCI Education strikes, same-month | | | | **0.087**$^{**}$ | 0.046 | 0.004 | 0.099 |
| | | | | **(0.037)** | (0.034) | (0.024) | (0.091) |
| PCI Education strikes, 4-month lag | | | | **-0.079** | 0.105 | 0.017 | 0.082 |
| | | | | **(0.110)** | (0.077) | (0.070) | (0.120) |
| PCI Healthcare strikes, same-month | | | | 0.007 | **0.181**$^{***}$ | -0.029 | -0.062 |
| | | | | (0.063) | **(0.036)** | (0.035) | (0.072) |
| PCI Healthcare strikes, 4-month lag | | | | 0.036 | **-0.066** | 0.017 | -0.132 |
| | | | | (0.089) | **(0.085)** | (0.039) | (0.093) |
| PCI Other-Serv strikes, same-month | | | | 0.102 | -0.085 | **0.103**$^{***}$ | 0.027 |
| | | | | (0.096) | (0.073) | **(0.031)** | (0.078) |
| PCI Other-Serv strikes, 4-month lag | | | | 0.153$^{*}$ | 0.007 | **0.111**$^{*}$ | 0.240$^{*}$ |
| | | | | (0.093) | (0.069) | **(0.062)** | (0.145) |
| PCI Goods strikes, same-month | | | | -0.057 | 0.033 | 0.016 | **0.128**$^{**}$ |
| | | | | (0.052) | (0.033) | (0.034) | **(0.051)** |
| PCI Goods strikes, 4-month lag | | | | -0.024 | 0.066 | -0.039 | **-0.041** |
| | | | | (0.072) | (0.053) | (0.045) | **(0.070)** |
| $N$ | 108325 | 47652 | 47433 | 10472 | 10472 | 17072 | 9417 |
| County × industry FE | – | Yes | Yes | – | – | – | – |
| County FE | Yes | – | – | Yes | Yes | Yes | Yes |
| Month FE | – | Yes | – | Yes | Yes | Yes | Yes |
| Industry × month FE | Yes | – | Yes | – | – | – | – |

*Notes:* Columns (1)–(3) stack each county–month into four industry-group observations (education, healthcare, other service-providing, goods-producing); the outcome is that group's own strikes, and exposure is split into *same-industry* (PCI-weighted strikes in other counties in the same industry group as the outcome) versus *cross-industry* (the sum of PCI-weighted strikes in the other three groups). Same- and cross-industry exposures are built from raw counts and then standardized by their pooled standard deviations across the stacked sample. Columns (4)–(7) report the by-outcome-industry specification, one column per industry, with PCI-weighted exposure to strikes in each of the four industry groups. Fixed-effects schemes are indicated in the table; dropping county×industry fixed effects in column (1) retains the never-striking industry cells that PPML separation would otherwise remove, raising the sample. PPML (`ppmlhdfe`); right-hand-side variables enter in standard deviations; standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

lems, as members can more readily monitor one another, communicate, and identify shared interests (Olson, 1965; Hardin, 1982). The lagged association is stronger in counties that experienced a strike during the preceding six months, consistent with recent local strike activity increasing responsiveness to accumulated exposure from socially connected counties.

## 4.5 Strike Participation

The previous analyses examine the extensive margin of strike activity—whether and how often strikes occur. We now consider the intensive margin by examining the number of strike participants as outcome, which allows us to assess whether social network exposure is also associated with the scale of local mobilization.

Table 9: Heterogeneity of the Network-Exposure Effect: Interactions with Size, Recent Activity, Period, Density, and Urban Share

| | (1) By population | (2) By recent strikes | (3) By period (COVID) | (4) By density | (5) By urban share |
|---|---|---|---|---|---|
| PCI-weighted strikes, same-month | 0.388*** | 0.326*** | 0.235*** | 0.309*** | 0.324*** |
| | (0.095) | (0.079) | (0.048) | (0.064) | (0.072) |
| × upper subgroup | -0.187* | -0.125 | 0.166 | -0.130* | -0.125 |
| | (0.098) | (0.083) | (0.133) | (0.075) | (0.083) |
| Lagged PCI-weighted strikes, 4-month sum | 0.152 | -0.094 | 0.126 | 0.050 | 0.088 |
| | (0.188) | (0.121) | (0.082) | (0.114) | (0.158) |
| × upper subgroup | -0.034 | 0.249** | -0.190 | 0.120 | 0.051 |
| | (0.171) | (0.116) | (0.156) | (0.110) | (0.147) |
| Lagged own-county strikes, 2-month sum | -0.051* | | -0.083*** | -0.079*** | -0.079*** |
| | (0.028) | | (0.027) | (0.027) | (0.028) |
| × upper subgroup | 0.115* | | 0.186** | 0.158*** | 0.107** |
| | (0.067) | | (0.072) | (0.037) | (0.046) |
| $N$ | 27236 | 25284 | 27236 | 27192 | 27236 |

*Notes:* Each column reports a single pooled PPML regression in which the network-exposure effect is interacted with an indicator for the "upper" subgroup: above-median county population (column 1), any versus no strikes in the trailing six months (column 2), the 2021 COVID period versus 2022 onward (column 3), above-median population density (column 4), and above-median urban-population share (column 5). Population, density, and urban-share medians are taken across counties. The lagged own-county strike term is omitted from column 2 only: there the split variable is itself lagged own-county strike activity, so the own-lag regressor is identically zero in the lower subgroup and collinear with its own interaction; recent activity is conditioned on through the subgroup indicator instead. Estimated on the ever-strike sample with county and month–year fixed effects; standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Column (1) of Table 10 relates total local participation to PCI-weighted exposure to strike participation in other counties. Because the participation coefficients are larger than those in the strike-incidence specifications, the linear approximation understates the implied effects more substantially; in this subsection we therefore report exact semi-elasticities, $100 \times (e^{\beta} - 1)$. A one-standard-deviation increase in contemporaneous participation exposure is associated with approximately 20% higher local participation, while cumulative exposure over the previous four months is associated with about 22% lower participation.

The remaining columns of Table 10 examine how PCI-weighted strike incidence is associated with local participation. Columns (2)–(4) stack the four industry groups and show that a one-standard-deviation increase in contemporaneous same-industry strike exposure is associated with approximately 37%–58% higher participation, while lagged same-industry and cross-industry relationships are less consistent. The separate industry estimates in columns (5)–(8) show approximately 52%, 57%, and 68% higher contemporaneous participation in other service-providing, goods-producing, and healthcare industries, respectively, while lagged same-industry estimates again are inconsistent across industries. Overall, the clearest pattern is a strong positive contemporaneous relationship between strike exposure and participation within the same industry.

Table 10: Strike Participation and Network Exposure: Total, Same versus Cross Industry, and by Outcome Industry

| | | Stacked: same vs. cross | | | By outcome industry | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) Total | (2) | (3) | (4) | (5) Education | (6) Healthcare | (7) Other Serv. | (8) Goods |
| PCI participation exposure, same-month | 0.182*** | | | | | | | |
| | (0.057) | | | | | | | |
| PCI participation exposure, 4-month lag | -0.245** | | | | | | | |
| | (0.115) | | | | | | | |
| Same-industry PCI strikes, same-month | | **0.319*****  | **0.460***** | **0.393***** | | | | |
| | | **(0.099)** | **(0.102)** | **(0.083)** | | | | |
| Same-industry PCI strikes, 4-month lag | | **-0.445***** | **-0.278*** | **0.334**** | | | | |
| | | **(0.152)** | **(0.160)** | **(0.149)** | | | | |
| Cross-industry PCI strikes, same-month | | -0.083 | -0.134 | -0.180** | | | | |
| | | (0.131) | (0.135) | (0.092) | | | | |
| Cross-industry PCI strikes, 4-month lag | | 0.004 | -0.051 | 0.030 | | | | |
| | | (0.142) | (0.140) | (0.101) | | | | |
| PCI Education strikes, same-month | | | | | **-0.020** | -0.379** | 0.174* | 0.037 |
| | | | | | **(0.133)** | (0.176) | (0.094) | (0.098) |
| PCI Education strikes, 4-month lag | | | | | **-0.399***** | 0.166 | -0.146 | 0.216 |
| | | | | | **(0.113)** | (0.134) | (0.224) | (0.186) |
| PCI Healthcare strikes, same-month | | | | | -0.079 | **0.521***** | -0.086 | 0.012 |
| | | | | | (0.065) | **(0.089)** | (0.151) | (0.082) |
| PCI Healthcare strikes, 4-month lag | | | | | 0.145 | **0.442***** | 0.159 | -0.348 |
| | | | | | (0.136) | **(0.144)** | (0.245) | (0.224) |
| PCI Other-Serv strikes, same-month | | | | | -0.117 | -0.343 | **0.420**** | -0.113 |
| | | | | | (0.151) | (0.297) | **(0.166)** | (0.156) |
| PCI Other-Serv strikes, 4-month lag | | | | | 0.130 | -0.312 | **0.503** | 0.142 |
| | | | | | (0.167) | (0.218) | **(0.332)** | (0.160) |
| PCI Goods strikes, same-month | | | | | 0.069 | 0.048 | -0.206 | **0.459***** |
| | | | | | (0.158) | (0.077) | (0.226) | **(0.101)** |
| PCI Goods strikes, 4-month lag | | | | | -0.136 | 0.290 | 0.013 | **0.619***** |
| | | | | | (0.162) | (0.355) | (0.122) | **(0.205)** |
| $N$ | 21472 | 82472 | 34408 | 33280 | 7014 | 6794 | 13112 | 6360 |
| County × industry FE | – | – | Yes | Yes | – | – | – | – |
| County FE | Yes | Yes | – | – | Yes | Yes | Yes | Yes |
| Month FE | Yes | – | Yes | – | Yes | Yes | Yes | Yes |
| Industry × month FE | – | Yes | – | Yes | – | – | – | – |

*Notes:* The outcome is per-capita strike participation. Column (1) is total participation on PCI-weighted total-participation exposure. Columns (2)–(4) stack each county–month into four industry-group observations (education, healthcare, other service-providing, goods-producing), with participation in each group regressed on PCI-weighted *strike* exposure split into same-industry versus cross-industry (the sum of the other three groups' exposure); the stacked exposures are built from raw counts and standardized by their pooled standard deviations. Columns (5)–(8) report participation by outcome industry on PCI-weighted strikes in each industry group. Fixed-effects schemes are indicated in the table. PPML (`ppmlhdfe`); right-hand-side variables enter in standard deviations; standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B7 reverses the outcome and explanatory variables used in Table 10, using the number of local strikes as the outcome and PCI-weighted participation as the explanatory variable. A one-standard-deviation increase in contemporaneous total participation exposure is associated with approximately 5% more local strikes, with corresponding increases of 3%–8% across all four industry groups. Lagged participation exposure and lagged own-county participation are generally insignificant, with the exception of a positive association in other service-providing industries, reinforcing that the strongest relationships are contemporaneous and within industries.

# 5 Potential Mechanisms and Additional Results

The results from Section 4 show that strike incidence in a county is correlated with social network–based exposure to strikes in other counties. This relationship remains consistently significant even after controlling for other distance-based exposure measures and holds across industries as well as on the intensive margin. This section explores the mechanisms through which strikes may spread via social media and presents additional results that shed light on how the spread of strikes varies under different policy and administrative environments.

## 5.1 Union Facebook Followers

Because the PCI network is based on Facebook friendships, we examine whether strikes organized by unions with larger Facebook audiences are more strongly associated with strike activity in connected counties. Following Equation (3) in Appendix D, we construct follower-weighted PCI exposure by weighting strikes in each connected county by both PCI connection and the number of Facebook followers of the organizing union. To distinguish whether local strike incidence is associated primarily with PCI-weighted strike exposure or with union visibility, we decompose follower-weighted PCI exposure into the standard PCI-weighted strike exposure used in the baseline specification (*volume*), average follower presence per exposed strike (*intensity*), and their interaction.

Column (1) of Table 11 summarizes this decomposition. A one-standard-deviation increase in contemporaneous PCI exposure is associated with approximately 15% higher expected local strike incidence. In contrast, none of the follower-presence terms is statistically significant: neither contemporaneous nor lagged presence, nor either of their interactions with exposure volume. Thus, the decomposition suggests that the contemporaneous relationship is driven primarily by the volume of strike exposure rather than systematically amplified by unions' Facebook follower reach.

Some more detailed estimates in Table B8 show that, when PCI connectedness and union follower presence are combined into a single exposure measure, a one-standard-deviation increase in contemporaneous follower-weighted PCI exposure is associated with approximately 17% higher expected strike incidence, while the lagged relationship is insignificant. This is essentially the same estimate as for unweighted PCI exposure, which is itself the clearest indication that weighting strikes by their union's following adds nothing. A similar contemporaneous pattern appears for within-industry exposure.

Overall, there is little evidence that unions with larger Facebook followings independently strengthen the relationship between PCI-weighted exposure and local strike activity, suggesting that exposure to strikes through social networks matters more than union follower presence itself. This pattern echoes the UCI results in Table 6 and is consistent with workers responding to strike-related information from socially and professionally relevant peers beyond formal union connections, although informational learning cannot yet be fully distinguished from solidarity or other behavioral responses.

### 5.2 Strike Outcomes

We examine how social media helps spread strike activity across counties through two mechanisms in our theoretical framework: informational discovery and behavioral responses. We distinguish between them empirically using strike settlements. We interpret settlements as successful strikes based on evidence from Tables B15 and B16, which shows that worker-side announcements were identified for 92.8% of settlements and overwhelmingly expressed satisfaction or framed the outcomes as wins. If exposure to successful strikes or prior outcomes predicts local strike incidence, this suggests learning consistent with informational discovery. If strike activity spreads regardless of outcomes, it instead suggests behavioral responses driven by solidarity, identification, or emotional cues.

Columns (2)–(3) of Table 11 show that exposure to deals in socially connected counties, defined as strikes that resulted in settlements, does not predict either local strikes or local deals, whether measured contemporaneously or with a lag. In contrast, exposure to contemporaneous strike activity is positively associated with local strike incidence. The industry-specific results in Table B13 are broadly consistent with this pattern. Deal counts and deal exposure throughout use the single-county definition described in Section 2. Together, these findings suggest that workers respond more to observing mobilization itself than to learning from settlement outcomes, providing stronger support for the behavioral channel than for the informational discovery channel.

More detailed results on strike settlements in Table B9 further show that local deal activity is positively associated with recent own-county strikes but negatively associated with lagged own-county deals. This pattern suggests that settlement activity reflects local strike and resolution cycles rather than learning from outcomes observed in socially connected counties.

Table 11: Mechanisms and Policy Environment: Summary of Headline Specifications

| | Follower presence | Strike outcomes | | Right-to-work | | | Public vs. private | |
|---|---|---|---|---|---|---|---|---|
| | (1) Strikes | (2) Strikes | (3) Deals | (4) All | (5) Public | (6) Private | (7) Public | (8) Private |
| Lagged own-county strikes, 2-month sum | -0.021* | -0.020* | 0.150*** | -0.021* | -0.111*** | -0.004 | -0.111*** | -0.004 |
| | (0.011) | (0.011) | (0.053) | (0.011) | (0.029) | (0.010) | (0.029) | (0.011) |
| Standard PCI exposure (volume), same-mo | 0.165*** | | | | | | | |
| | (0.030) | | | | | | | |
| Standard PCI exposure (volume), 4-mo lag | -0.000 | | | | | | | |
| | (0.081) | | | | | | | |
| Mean follower presence (intensity), same-mo | -0.073 | | | | | | | |
| | (0.100) | | | | | | | |
| Mean follower presence (intensity), 4-mo lag | -0.298 | | | | | | | |
| | (0.508) | | | | | | | |
| Volume × reach, same-mo | 0.001 | | | | | | | |
| | (0.023) | | | | | | | |
| Volume × reach, 4-mo lag | 0.206 | | | | | | | |
| | (0.168) | | | | | | | |
| PCI-weighted deals, same-month | | -0.000 | 0.013 | | | | | |
| | | (0.031) | (0.039) | | | | | |
| PCI-weighted deals, 4-month lag | | -0.014 | 0.043 | | | | | |
| | | (0.051) | (0.073) | | | | | |
| PCI-weighted strikes, same-month | | 0.158*** | -0.005 | | | | | |
| | | (0.029) | (0.063) | | | | | |
| PCI-weighted strikes, 4-month lag | | 0.075 | 0.030 | | | | | |
| | | (0.052) | (0.086) | | | | | |
| PCI-weighted strike exposure, same-month | | | | 0.150*** | 0.095*** | 0.144*** | | |
| | | | | (0.031) | (0.027) | (0.024) | | |
| × Right-to-work, same-month | | | | 0.090 | 0.131 | 0.167** | | |
| | | | | (0.080) | (0.166) | (0.077) | | |
| PCI-weighted strike exposure, 4-month lag | | | | 0.078 | -0.024 | 0.074 | | |
| | | | | (0.049) | (0.074) | (0.051) | | |
| × Right-to-work, 4-month lag | | | | -0.060 | 0.169 | -0.190 | | |
| | | | | (0.196) | (0.236) | (0.191) | | |
| PCI-weighted public strikes, same-month | | | | | | | 0.092*** | -0.000 |
| | | | | | | | (0.033) | (0.029) |
| PCI-weighted public strikes, 4-month lag | | | | | | | -0.031 | 0.039 |
| | | | | | | | (0.080) | (0.046) |
| PCI-weighted private strikes, same-month | | | | | | | 0.001 | 0.155*** |
| | | | | | | | (0.082) | (0.024) |
| PCI-weighted private strikes, 4-month lag | | | | | | | 0.153* | 0.057 |
| | | | | | | | (0.089) | (0.054) |
| Observations | 27236 | 27236 | 16236 | 27236 | 14168 | 22616 | 14168 | 22616 |

*Notes:* Each column reports the headline specification of the corresponding mechanism or policy test in Sections 5.1–5.4; the full detailed tables appear in the appendix (Tables B8, B9, B11, and B10). Column (1) regresses strikes on the follower-weighted exposure decomposition: *volume* (standard PCI-weighted strike exposure), *intensity* (mean $\ln(1+\text{followers})$ reach of the strikes one is exposed to), and their interaction, each same-month and as four-month lagged sums (interacted variables demeaned). Columns (2)–(3) regress strikes and settlements ("deals"), respectively, on PCI-weighted deal and strike exposure. Columns (4)–(6) interact PCI-weighted strike exposure with state right-to-work status, for all strikes, public-sector strikes, and private-sector strikes (own-sector exposure). Columns (7)–(8) regress public- and private-sector strike counts on both sectors' PCI-weighted exposures. All columns control for own-county lagged strikes, shown in the first row (columns (2)–(3) additionally control for lagged own-county deals; columns (7)–(8) use the outcome sector's own lag). PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

### 5.3 Policy Environments

We examine whether Right-to-Work policies affect the relationship between social network exposure and strike activity. By allowing workers to opt out of union membership and dues, RTW laws may weaken union resources and institutional support for collective action. This could either increase workers' reliance on information and encouragement from socially connected workers elsewhere or reduce their ability to translate such exposure into strikes. We therefore interact PCI-weighted strike exposure with an indicator equal to one if the county is located in an RTW state. Additionally, because RTW laws cover the private sector, we estimate the interaction effect between PCI-weighted exposure to strikes and RTW status in the two sectors separately. In column (4) of Table 11, neither the contemporaneous nor the lagged interaction with RTW status is statistically significant. Columns (5) and (6) estimate the interaction separately by sector, pairing each sector's strikes with same-sector exposure: the RTW interaction is positive and significant for private-sector strikes but positive and imprecisely estimated for public-sector strikes. Although this is consistent with the hypothesis that the role of RTW should be more salient for the private sector, when we stack the observations and test if the interaction effect is statistically different between the two sectors ( Table B11 column (6)), we find that it is not. Thus, while the RTW gradient is individually significant only in the private sector—where the laws apply directly, consistent with weaker unions increasing workers' reliance on peer networks—we cannot reject that it is equal across the two sectors, and we read the evidence as suggestive rather than conclusive.

The industry-specific results nevertheless reveal some heterogeneity. Table B14 shows that, in RTW states, contemporaneous own-industry exposure is significant for healthcare and other service-providing industries, whereas in non-RTW states it is significant for education, healthcare, and goods-producing industries. Lagged own-industry and cross-industry exposure are largely insignificant throughout. Overall, the evidence remains inconclusive on the precise role RTW plays in mediating the association between strikes in a county and network exposure to strikes in other counties.

### 5.4 Public Sector vs. Private Sector

We next examine whether social network effects differ between public- and private-sector strikes. Public strikes are defined as those against government-owned firms or government agencies. In many states, public-sector unions must provide advance notice before striking, often 10 days or longer, and may face mandatory mediation periods, such as the 30-day period in Minnesota.[22] These constraints could make lagged exposure more relevant for public-sector workers, while private-sector workers may be able to respond more quickly. On the other hand, a more onerous process of public sector striking may blunt the role of network exposure, with ambiguous net effect. We therefore separate PCI-weighted exposure originating from public- and private-sector strikes.

Columns (7)–(8) of Table 11 show that neither public- nor private-sector strike activity responds significantly to lagged same-sector exposure (and Table B10 in the Appendix reports additional related specifications). In contrast, both are positively associated with contemporaneous exposure originating within the same sector, while contemporaneous cross-sector exposure is insignificant. The coefficient on lagged private-sector exposure is positive but only marginally significant in the specification with public sector strikes as the outcome. Overall, we find little evidence that public-sector institutions affect the timing of network diffusion.

## 6 Discussion

This paper examines how labor strikes spread through social networks across U.S. counties. Combining strike data from the Cornell ILR Labor Action Tracker for 2021–2024 with Facebook-based measures of county connectedness, we find that greater contemporaneous exposure to strikes in socially connected counties is strongly associated with higher local strike incidence. The relationship is robust across alternative specifications, is concentrated within industries, and extends to strike participation, suggesting that social networks are linked to both the incidence and scale of collective action.

Our mechanism analyses suggest that workers respond more to observing mobilization itself than to signals about union prominence or strike success. Union Facebook follower presence does not independently amplify network diffusion, and exposure to strikes that result in settlements predicts neither local strikes nor local settlements. These patterns provide stronger support for behavioral responses, such as solidarity, shared identity, and emotional contagion, than for the information discovery channel examined in the paper. We also find little evidence that right-to-work laws or public-sector administrative constraints systematically alter the strength or timing of diffusion.

Overall, our findings show that local strikes are strongly correlated with exposure to strikes in socially connected locations. Future research could distinguish the underlying mechanisms more directly by examining the content, framing, and emotional tone of strike-related communication. Exploiting plausibly exogenous changes in network exposure or institutional

[22] Minnesota Statutes §179A.18, https://www.revisor.mn.gov/statutes/cite/179A.18.

constraints could also strengthen the causal interpretation of these relationships. More broadly, this framework could be applied to other forms of labor and political mobilization, offering a wider perspective on how digital social networks reshape collective action.

## References


[1] John M. Abowd and Joseph S. Tracy. “Market Structure, Strike Activity, and Union Wage Settlements”. In: *Industrial Relations* 28.2 (1989), pp. 227–250.

[2] Orley Ashenfelter and George E. Johnson. “Bargaining Theory, Trade Unions, and Industrial Strike Activity”. In: *American Economic Review* 59.1 (1969), pp. 35–49.

[3] Michael Bailey, Rachel Cao, Theresa Kuchler, Johannes Stroebel, and Arlene Wong. “Social connectedness: Measurement, determinants, and effects”. In: *Journal of Economic Perspectives* 32.3 (2018), pp. 259–80.

[4] Michael Bailey, Drew Johnston, Martin Koenen, Theresa Kuchler, Dominic Russel, and Johannes Stroebel. “Social Networks Shape Beliefs and Behavior: Evidence from Social Distancing during the COVID-19 Pandemic”. In: *Journal of Political Economy Microeconomics* 2.3 (2024), pp. 463–494. DOI: 10.1086/729533.

[5] Arnab K. Basu, Nancy H. Chau, and Oleg Firsin. “Social Connections and COVID19 Vaccination”. In: *Health Economics* 34.6 (2025), pp. 1188–1213. DOI: 10.1002/hec.4953.

[6] Michael Biggs. “Positive Feedback in Collective Mobilization: The American Strike Wave of 1886”. In: *Theory and Society* 32.2 (Apr. 2003), pp. 217–254.

[7] Eric Blanc. “The Emergency Workplace Organizing Committee: A New Model for Worker-Led Organizing”. In: *New Labor Forum* 33.3 (2024), pp. 56–63.

[8] Eric Blanc. “Unionizing Young Workers: America’s Labor Uptick and Youth Outreach Strategies”. In: *ILR Review* (2026). Forthcoming/online first.

[9] Eric Blanc. *We Are the Union: How Worker-to-Worker Unionism Is Revitalizing Labor and Winning Big*. Oakland, CA: University of California Press, 2025.

[10] Yann Bramoullé, Habiba Djebbari, and Bernard Fortin. “Identification of peer effects through social networks”. In: *Journal of Econometrics* 150.1 (2009), pp. 41–55.

[11] David Card. “Longitudinal Analysis of Strike Activity”. In: *Journal of Labor Economics* 6 (1988), pp. 147–176.

[12] David Card. “Strikes and Wages: A Test of an Asymmetric Information Model”. In: *Quarterly Journal of Economics* 105.3 (1990), pp. 625–659.

[13] Kelly Chen, Yingying Dong, and Samia Islam. “Right-to-work laws, union decline, and the opioid crisis”. In: *Journal of Population Economics* 39.25 (2026). DOI: 10.1007/s00148-026-01168-w.

[14] Robert Cialdini. *Influence: The Psychology of Persuasion*. New York, NY: William Morrow and Company, 1984.

[15] Sergio Correia, Paulo Guimarães, and Tom Zylkin. “Fast Poisson estimation granovetter high-dimensional fixed effects”. In: *The Stata Journal* 20.1 (2020), pp. 95–115.

[16] Peter C. Cramton and Joseph S. Tracy. “Strikes and Holdouts in Wage Bargaining: Theory and Data”. In: *American Economic Review* 82.1 (1992), pp. 100–121.

[17] Peter C. Cramton and Joseph S. Tracy. "Wage Bargining with Time-Varying Threats". In: *Journal of Labor Economics* 12.4 (1994), pp. 594–617.

[18] Ruben Enikolopov, Alexey Makarin, and Maria Petrova. "Social Media and Protest Participation: Evidence from Russia". In: *Econometrica* 88.4 (2020), pp. 1479–1514. DOI: 10.3982/ECTA14281.

[19] Rick Fantasia. *Cultures of Solidarity: Consciousness, Action, and Contemporary American Workers*. University of California Press, 1988.

[20] Matthias Flückiger and Markus Ludwig. "The Structure of Online Social Networks and Social Movements: Evidence from the Black Lives Matter Protests". In: *Journal of Public Economics* 246 (2025), p. 105373.

[21] Francis J. Flynn. "No News is Good News: The Relationship Between Media Attention and Strike Duration". In: *Industrial Relations* 39.1 (2000), pp. 139–160.

[22] Drew Fudenberg and Jean Tirole. "Sequential Bargaining with Incomplete Information". In: *The Review of Economic Studies* 50.2 (1983), pp. 221–247.

[23] Paul Goldsmith-Pinkham and Guido W. Imbens. "Social Networks and the Identification of Peer Effects". In: *Journal of Business & Economic Statistics* 31.3 (2013), pp. 253–264.

[24] Mark Granovetter. "Threshold Models of Collective Behavior". In: *American Journal of Sociology* 83.6 (1978), pp. 1420–1443.

[25] Russell Hardin. *Collective Action*. Baltimore, MD: Johns Hopkins University Press, 1982.

[26] Alan Harrison and Mark Stewart. "Cyclical Fluctuations in Strike Durations". In: *The American Economic Review* 79.4 (1989), pp. 827–841.

[27] Beth Hayes. "Unions and Strikes and Asymmetric Information". In: *Journal of Labor Economics* 2.1 (1984), pp. 57–83.

[28] David J. Houghton and Andy Hodder. "Understanding Trade Union Usage of Social Media: A Case Study of the Public and Commercial Services Union on Facebook and Twitter". In: *New Technology, Work and Employment* 36.2 (2021), pp. 219–239.

[29] J. M. Jellison and J. Mills. "Effect of Public Commitment Upon Opinions". In: *Journal of Experimental Social Psychology* 5.3 (1969), pp. 340–346.

[30] Johnnie Kallas, Deepa Kylasam Iyer, and Eli Friedman. *Labor Action Tracker*. Dataset. 2026. URL: https://striketracker.ilr.cornell.edu/.

[31] John Kennan. "The duration of contract strikes in U.S. manufacturing". In: *Journal of Econometrics* 28.1 (1985), pp. 5–28. ISSN: 0304-4076.

[32] Thomas A. Kochan and Jean Baderschneider. "Dependence on Impasse Procedures: Police and Firefighters in New York State". In: *Industrial and Labor Relations Review* 31.4 (1978), pp. 431–449.

[33] Alex Lehr, A. Akkerman, and Rene Torenvlied. "The Influence of External Information on Collective Bargaining: Survey Evidence of Union and Firm Negotiators in the Netherlands". In: *Relations industrielles* 70 (June 2015), p. 327. DOI: 10.7202/1031488ar.

[34] David B. Lipsky and John E. Drotning. "The Relation between Teacher Salaries and the Use of Impasse Procedures under New York's Taylor Law: 1968-1972". In: *Journal of Collective Negotiations in the Public Sector* 6.3 (1977), pp. 229–244.

[35] Steven Manson, Jonathan Schroeder, David Van Riper, Tracy Kugler, and Steven Ruggles. *IPUMS National Historical Geographic Information System: Version 16.0*. 2021. DOI: 10.18128/D050.V16.0.

[36] Martin J. Mauro. "Strikes as a Result of Imperfect Information". In: *Industrial and Labor Relations Review* 35.4 (1982), pp. 522–538.

[37] Sheena McConnell. "Cyclical Fluctuations in Strike Activity". In: *Industrial and Labor Relations Review* 44.1 (1990), pp. 130–143.

[38] MIT Election Data and Science Lab. *County Presidential Election Returns 2000-2020*. 2022. DOI: 10.7910/DVN/VOQCHQ. URL: https://doi.org/10.7910/DVN/VOQCHQ.

[39] Samin Mohammadi, Reza Farahbakhsh, and Noël Crespi. "Popularity evolution of professional users on facebook". In: *2017 IEEE International Conference on Communications (ICC)*. 2017, pp. 1–6. DOI: 10.1109/ICC.2017.7997220.

[40] Suresh Naidu and Noam Yuchtman. "Labor Market Institutions in the Gilded Age of American Economic History". In: *Oxford Handbook of American Economic History*. Ed. by L. S. Cain, Price V. Fishback, and Paul Rhode. Oxford: Oxford University Press, 2016.

[41] Mancur Olson. *The Logic of Collective Action: Public Goods and the Theory of Groups*. Cambridge, MA: Harvard University Press, 1965.

[42] Chantal Pezold, Simon Jäger, and Patrick Nüß. *Labor Market Tightness and Union Activity*. Working Paper 31988. National Bureau of Economic Research, 2023.

[43] Baiqin Qin, David Strömberg, and Yanhui Wu. "Social Media and Collective Action in China". In: *Econometrica* 92.6 (Nov. 2024), pp. 1965–2003. DOI: 10.3982/ECTA19863.

[44] Zachary Schaller, Sal McCollum, and Prasiddha Shakya. *The Siren of the Labor Movement: Spillover Effects from Starbucks Organizing*. SSRN Working Paper No. 5273207. 2025.

[45] D.E. Schmidt. "Public Opinion and Media Coverage of Labor Unions". In: *Journal of Labor Research* 14 (1993), pp. 151–164.

[46] John F. Schnell and Cynthia L. Gramm. "Learning by Striking: Estimates of the Teetotaler Effect". In: *Journal of Labor Economics* 5.2 (1987), pp. 221–241.

[47] Edward Shorter and Charles Tilly. *Strikes in France, 1830–1968*. London / New York: Cambridge University Press, 1974, p. 458. ISBN: 0521202930.

[48] Scott Sikes. *Toward New Possibilities for Library and Information Science: The Use of Social Media in the 2018 West Virginia Teachers' Strike*. Advances in Librarianship, 2023. ISBN: 978-1-80382-380-5.

[49] JMC Santos Silva and Silvana Tenreyro. "The log of gravity". In: *The Review of Economics and statistics* 88.4 (2006), pp. 641–658.

[50] David A. Snow and Robert D. Benford. “Ideology, Frame Resonance, and Participant Mobilization”. In: *International Social Movement Research: From Structure to Action*. Ed. by Bert Klandermans, Hanspeter Kriesi, and Sidney Tarrow. Vol. 1. Greenwich, CT: JAI Press, 1988, pp. 197–217.

[51] David A. Snow, E. Burke Rochford, Steven K. Worden, and Robert D. Benford. “Frame Alignment Processes, Micromobilization, and Movement Participation”. In: *American Sociological Review* 51.4 (1986), pp. 464–481.

[52] Joel Sobel and Ichiro Takahashi. “A Multistage Model of Bargaining”. In: *The Review of Economic Studies* 50.3 (1983), pp. 411–426.

[53] James L. Stern. *Final-offer Arbitration: The Effects on Public Safety Employee Bargaining*. Lexington Books, 1975. ISBN: 978-0669000221.

[54] Joseph S. Tracy. “An Empirical Test of an Asymmetric Information Model of Strikes”. In: *Journal of Labor Economics* 5.2 (1987), pp. 149–173.

[55] Joseph S. Tracy. “An Investigation into the Determinants of U.S. Strike Activity”. In: *The American Economic Review* 76.3 (1986), pp. 423–436.

[56] Susan B. Vroman. “A Longitudinal Analysis of Strike Activity in U.S. Manufacturing: 1957-1984”. In: *American Economic Review* 79.4 (1989), pp. 816–826.

# Appendix

## A The 2021–2024 Study Period in Context

Because the ILRLAT began systematic collection only in 2021, it cannot by itself place the study window in historical or economic context. We use several independent sources—none sharing the ILRLAT's collection methodology—to show that 2021–2024 was a revival of U.S. strike activity, the most active in roughly two decades though far below mid-twentieth-century levels, occurring under economic conditions unusually favorable to collective action. The supporting figures appear in Appendix G.

**Strike activity over the long run.** Figure A3 plots BLS major work stoppages (1,000+ workers) by year since 1947 and by quarter since 1993. Annual counts fell from 200–470 in the 1950s–1970s to roughly 15–25 per year after 2000; against that low baseline, 2021–2024 stands out as a clear local upswing—the highest counts in about two decades, though 2018-2019 wave was also notable—while remaining an order of magnitude below the post-war peak. The recent rise is thus pronounced relative to the last twenty years but modest relative to the last fifty.

**Industry composition.** To place the industry mix of our data in perspective, we use the BLS Work Stoppages program (major stoppages of 1,000+ workers) and the Federal Mediation and Conciliation Service (FMCS, stoppages of all sizes, 1984–2019).

Figure A4 compares the industry composition of strikes in 2021–2024 across our data and the BLS, in the four harmonized sectors used throughout the paper. The two sources tell a consistent, services-heavy story, but the ILRLAT captures many more of the smaller "other service-providing" actions that fall below the BLS 1,000-worker threshold, whereas the BLS major stoppages are relatively more concentrated in education and health care (the sectors where the largest strikes, such as teacher and nurse strikes, occur).

Figure A5 places this composition in a longer time frame, showing the share of work stoppages by broad sector across periods in the BLS major-stoppage data (panel a) and the broader FMCS data (panel b). Both sources show the same shift: manufacturing, which accounted for roughly 60% of FMCS stoppages and over 40% of major BLS stoppages in the 1980s–1990s, had fallen to about one-quarter (FMCS) and one in ten (BLS) by the 2010s–2020s, with services—health care in both, and education in the BLS data—taking its place. The education-, health-care-, and service-heavy strikes the ILRLAT records in 2021–2024 thus sit squarely where strike activity has migrated, rather than reflecting a peculiarity of the tracker.

**Economic conditions.** The window also featured labor-market and macroeconomic conditions favorable to collective action. Figure A6 plots the ratio of job openings to unemployed workers: after the pandemic it climbed to roughly 2.0 in 2022, the tightest labor market in the JOLTS era, improving workers' outside options and making strikers harder to replace. Figure A7 shows that in 2021–2022 consumer-price inflation rose to about 9% and outpaced wage growth, so the median worker's real pay fell—a textbook trigger for cost-of-living strikes and contract disputes.

**Public attention.** Figure A8 shows that search interest in strike- and union-related terms rose broadly after 2021 to the highest levels in the 2004–2025 Google Trends record, mirroring the incidence measures above. Because search behavior reflects the attention of workers and the public rather than administrative events, this is suggestive of the heightened awareness and information flow the paper argues propagates strikes through social networks.

**Strike diffusion over the long run.** Finally, we ask whether the network transmission of strikes documented in the main analysis is specific to the 2021–2024 window by re-estimating the main specification on FMCS work stoppages, decade by decade, back to 1984 (Table B1). Three patterns emerge. First, the contemporaneous PCI-weighted association rises steadily toward the present—absent in the 1980s, fragile in the 1990s, robust from the 2000s—and by 2010–2019 (0.16) it closely matches the ILRLAT baseline (0.15), despite coming from an entirely different data source with a narrower, more union-dense set of striking counties. Second, the timing composition shifts: in the 1980s the association operates only with a lag, whereas in the most recent periods it is predominantly contemporaneous, consistent with faster diffusion of strike information as communication technology improved. Third, two features of the main analysis replicate in every period: recent own-county strike activity enters negatively, and the six alternative proximity exposures are never jointly significant. The FMCS data are not our preferred source—they cover only mediated, overwhelmingly private-sector bargaining units and lack the detail needed to identify multi-location coordinated actions—and the network measures are fixed at their 2020-era values, so the weaker estimates in earlier decades partly reflect distance from the measurement of social connectedness itself. With those caveats, the historical evidence suggests that the main results are not an artifact of the study window: network transmission of strikes is visible well before 2021 and strengthens as the era of online social networks arrives.

Table B1: Network Transmission of Strikes over the Long Run: FMCS Work Stoppages, 1984–2019

| | FMCS work stoppages | | | | ILRLAT |
|---|---|---|---|---|---|
| | 1984–89 | 1990–99 | 2000–09 | 2010–19 | 2021–24 |
| Lagged Total Strikes in Own County, 2-Month Sum | -0.086*** | -0.039** | -0.023 | -0.044*** | -0.02* |
| | (0.022) | (0.015) | (0.017) | (0.017) | (0.01) |
| PCI-Weighted Strikes of Other Counties, Same-Month | 0.039 | 0.038 | 0.093*** | 0.161*** | 0.15*** |
| | (0.049) | (0.036) | (0.026) | (0.040) | (0.05) |
| Lagged PCI-Weighted Strikes of Other Counties, 4-Month Sum | 0.220** | 0.096* | 0.077** | 0.048 | 0.13 |
| | (0.086) | (0.057) | (0.033) | (0.069) | (0.09) |
| Wald $\chi^2(12)$, proximity exposures | 14.96 | 13.66 | 18.97 | 17.28 | 12.18 |
| $p$-value | 0.24 | 0.32 | 0.09 | 0.14 | 0.43 |
| Number of Counties | 912 | 837 | 613 | 357 | 619 |
| County Fixed Effects | x | x | x | x | x |
| Month–Year Fixed Effects | x | x | x | x | x |
| Observations | 62,016 | 97,092 | 71,108 | 41,055 | 27,236 |

*Notes:* Each column re-estimates the specification of Table 2, column (3), on an earlier decade of Federal Mediation and Conciliation Service (FMCS) work stoppages (1984–2019; 27,494 stoppages assigned to counties by city and state, a 96% match rate); the final column reproduces the ILRLAT baseline for comparison. The outcome is strikes per 1,000 county residents; right-hand-side variables enter in standard deviations; all columns include the six alternative distance-weighted exposures and their four-month lags (unreported), tested jointly in the Wald row; county and month–year fixed effects; standard errors clustered by county in parentheses. The FMCS data are less detailed than the ILRLAT: they cover federally mediated, overwhelmingly private-sector bargaining units whose coverage varies over time, report no participant counts, and do not identify multi-location coordinated actions, so no independent-strike distinction is possible. The PCI and proximity networks and county populations are fixed at their 2020-era measurement throughout, so estimates for earlier decades rely on the persistence of the geography of social connections; the weakening of the association in earlier decades is consistent both with greater distance from the measurement date of social connectedness and with the absence of online social networks in those periods. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## B Strike-Outcome Research Pipeline: Reproducible Prompt

The strike-outcome research described in Section 2 was carried out by a large-language-model agent (Claude Code, running the Claude Opus 4.8 model) executing the single, self-contained prompt reproduced below, run from the project repository root against the raw master strike list. The pipeline classifies each action's outcome, archives the supporting sources as screenshots and text, and builds unit-matched, quote-anchored evidence records; the archived sources and evidence records are included with the replication materials. Execution was not fully hands-off: we provided occasional corrective feedback and manually checked interim output as the pipeline progressed, in addition to the ground-truth reconciliation built into its final phase.

```
You are running a labor-strike OUTCOME RESEARCH pipeline end to end. Input is a
↪  raw
master strike list (one row per U.S. labor action, ~2020-2024) with at least
↪  these
fields: a stable row id, Employer, City/Address, Union, Start Date, Worker
↪  Demands,
and a Source link (the ACTION announcement -- NOT the outcome). Goal: for each
↪  action,
find whether the workers' stated demand was MET within ~12 months, when it was
↪  met,
and archive the evidence. Everything is long-running and interruptible: always
↪  resume,
always append, never overwrite, save every 25 rows.

Work in four phases. Do NOT skip the archival/verification phases -- a
↪  classification
without a saved, unit-matched, quoted source does not count as confirmed.

-- PHASE 1 -- CLASSIFY OUTCOMES (write cc_findings.csv)
↪  -----------------------------
For each strike row, run 3-5 OUTCOME-focused web searches and classify.

Resume: on startup, if cc_findings.csv exists, read it, collect every row id
↪  already
present, and start at the first input row not yet in it. Process in input file
↪  order.
Append only. Save every 25 rows.

The core test -- mark a WIN ("flip") only if BOTH: (1) the specific stated
↪  demand in
Worker Demands was met, AND (2) within ~12 months of Start Date, causally tied
↪  to the
action. Match the outcome to the PRECISE demand (a raise != a recognition win).

Search the OUTCOME, not the action, and disambiguate multi-location employers
```

```
(Starbucks/Amazon/hospital systems) by City + exact Start Date:
  "<Employer>" <City> <Union> contract ratified OR agreement OR settled
   ↪  <year..year+1>
  <Union> <City> NLRB election result <year>        (recognition demands)
  "<Employer>" strike ended <month year>
Prefer the union's own site, local news, AFL-CIO/Labor Notes, NLRB, Becker's
↪  (health).

THE #1 RULE: output verified_no ONLY when EITHER (a) you found a source showing
↪  the
demand was NOT met (or met only after ~12 months), OR (b) the row clearly fits a
template category (single-store Starbucks/fast-food walkout; symbolic one-day
↪  Amazon/
HQ protest with no NLRB win in window; pandemic one-day safety rally not tied to
↪  a
contract that then settled; advocacy protest for a bill that failed / against a
↪  closure
that happened anyway; construction area-standards picket with no documented
↪  change;
informational picket where you searched and found NO contract within ~12 mo). In
↪  EVERY
other case -- including "I searched and couldn't find it" -- output UNCERTAIN.
↪  Strikes
with no outcome found thus remain UNCERTAIN at this stage; in the final dataset
↪  they are
coded as not resulting in a deal, alongside lean_no, but the verified_no label
↪  is
reserved for determinations backed by a source or a template category.

Columns in cc_findings.csv:
  row_id | status (flip/lean_yes/lean_no/verified_no/UNCERTAIN) |
  deal_date (YYYY-MM-DD; flip/lean_yes only; must be >= Start Date and <= ~12 mo
   ↪  after) |
  url1 (REQUIRED for flip/lean_yes and any non-template verified_no) | url2
   ↪  (optional) |
  extracted_fact (1-2 sentences in your words, quote <= a few words) |
  confidence (high/medium/low) |
  breadcrumbs (REQUIRED for every verified_no and UNCERTAIN: queries tried,
   ↪  near-miss
  source/date, union local #, exact facility, alternate employer names) |
  notes (brief reasoning; name the template category if you used one).
Never invent a URL or date. A deal landing just outside ~12 mo = lean_no, not
↪  flip.
Every few hundred rows, report rows done + running status counts.

-- PHASE 2 -- ARCHIVE THE SOURCES (screenshots + text)
↪  ----------------------------
Collect every url1/url2 from cc_findings.csv for flip/lean_yes/verified_no rows.
↪  For
each URL: fetch the page, save a full-page screenshot and the extracted page
↪  text to
disk as {row_id}_{site}.png / .txt, and log one row to archive_log.csv (row_id,
↪  url,
```

```
url_hash, http_status, saved_paths, timestamp). If the live page is dead or
↪  paywalled,
fetch the Wayback Machine copy and save that (suffix _wayback). Resume by
↪  skipping URL
hashes already logged (retry only prior timeouts/errors). Be polite: a few
↪  seconds
between fetches, and pause between large batches.

ITERATE FOR A USABLE SOURCE: after saving a source, check whether the downloaded
↪  text
actually SUPPORTS both the outcome (the demand being met/not met) AND the
↪  deal_date.
If it does not -- the page is a paywall stub, a nav-only shell, an unrelated
↪  action, or
silent on the date -- do NOT keep it. Go back to search, find a better source
↪  URL, and
archive that one instead. Repeat this at least TWICE (i.e., try at least 2
↪  alternative
sources) before giving up. If after those iterations no downloaded text supports
↪  both
outcome and date, downgrade the row's status accordingly (flip/lean_yes ->
↪  UNCERTAIN
unless a template applies) and record in breadcrumbs which URLs you tried and
↪  why each
failed.

-- PHASE 3 -- VERIFY & BUILD THE CORPUS (one record per row)
↪  ------------------------
For each archived row, read the SAVED source text (not a fresh search) and
↪  produce a
verification record appended to a human-readable corpus.md, containing:
  - facts: Union, Employer, outcome/deal date;
  - the source URL and the on-disk artifact filenames;
  - UNIT MATCH verdict: does the source actually name the SAME employer AND the
   ↪  same
    union/bargaining unit/facility as the strike row? (yes/partial/no + one line
     ↪  of why);
  - the OUTCOME QUOTE copied verbatim from the saved text, plus its CHARACTER
   ↪  OFFSET(S)
    into that text so it can be located exactly;
  - DATE SUPPORT: how the text pins the deal date to the deal_date value;
  - then the full saved source text in a code block.
If the saved text fails unit match, or the quote does not support the status, or
↪  the
date is outside the ~12-month window, treat the source as inadequate: return to
↪  Phase 2,
find and archive a better source (at least 2 iterations), and only then flag for
↪  human
re-adjudication if still unresolved.

-- PHASE 4 -- RECONCILE
↪  ------------------------------------------------------------
```

```
If a human-adjudicated ground-truth or manual-check file exists, compare your
↪  statuses
against it, report agreement rate and every disagreement (row_id, your status
↪  vs.
theirs, why), and report final counts: totals per status, and -- critically --
↪  how many
verified_no rows are backed by a real source URL vs. justified only by a
↪  template
category.

-- FINAL CODING (downstream of the pipeline)
↪  ----------------------------------------
For analysis, the statuses are collapsed to a binary outcome. A strike is coded
↪  as
ending in a deal ("loose yes") if ANY of its listed demands was met within ~12
↪  months
of the action start -- including partial, temporary, interim, imposed, court,
↪  ballot,
or legislative concessions the action pressured (flip and adjudicated lean_yes
↪  rows).
All other rows (lean_no, verified_no, UNCERTAIN) are coded as no deal. A
↪  stricter
variant (yes only when the primary demand was durably met by a ratified,
↪  non-imposed
collective bargaining agreement) is also constructed; the paper's deal measure
↪  uses
the loose definition. When these strike-level codes are aggregated to the
↪  county-month
panel used in the regressions, two restrictions apply: deals attributed to
↪  legislation or
policy are excluded, and a deal is assigned to a county only if the striking
↪  unit's
footprint lies entirely within that county, dated by the month of settlement.

Before starting, confirm which input file is the master strike list and what the
↪  row-id
column is called, then proceed. Ask me nothing else -- default to UNCERTAIN
↪  whenever a
judgment call is unclear.
```

## C Announcement-Sentiment Pipeline: Reproducible Prompt

The settlement-announcement sentiment data underlying Tables B15 and B16 were produced by the same agent and model in two stages: classification of the saved outcome sources archived by the pipeline of Appendix B, and a supplementary web search for confirmed deals whose saved sources contained no worker-side announcement, with web-found sources then verified for bargaining-unit match. Unlike the outcome pipeline, this pipeline ran end to end without intervening manual feedback or checks. The consolidated prompt is reproduced below, and the corpus-construction code is included with the replication materials.

```
You are running an ANNOUNCEMENT-SENTIMENT pipeline over confirmed strike
↪  settlements
("deals") for a labor-economics paper. Input is the set of confirmed-deal strike
↪  rows
from the outcome-research pipeline, each with: a stable row id, Employer, Union,
City/State, Start Date, deal date, Worker Demands, and the SAVED source text
↪  archived
for that row (the page text that documented the deal). Goal: for each confirmed
↪  deal,
determine whether a PROTESTER-SIDE (union/worker) public announcement or
↪  reaction to
the settlement exists, and if so classify its sentiment; where the saved source
↪  has
none, search the web for one and verify it. Everything is long-running and
interruptible: always resume, always append, never overwrite, save every 25
↪  rows.

A "protester-side announcement" is the union, a union local, a bargaining-team
↪  member,
a union officer/spokesperson, an organizing committee, or a rank-and-file worker
↪  on the
strikers' side, announcing OR reacting to the deal / tentative agreement /
↪  contract /
ratification. Tiers:
  union_authored       -- the source IS the union's own publication (press
  ↪  release on a
                          union site, union social post, union newsletter);
  union_quoted_in_news -- a third-party source that QUOTES the union or a worker
                          announcing/reacting to the deal;
  none                 -- no protester-side voice about the deal: only the
  ↪  employer's
                          statement, a government/NLRB notice, a bare factual
                          ↪  report
                          with no union/worker quote, PR wire, or paywall/nav
                          ↪  junk.
When in doubt between union_quoted_in_news and none, require an actual
↪  union/worker
VOICE (a quote or clearly attributed statement), not merely the union being
↪  named.
```

```
Sentiment fields (only when tier != none):
  satisfaction: very_satisfied | somewhat_satisfied | mixed | dissatisfied |
  ↪  not_stated
    (very_satisfied: celebratory, "historic win"; somewhat_satisfied: positive
    ↪  but
    qualified; mixed: explicitly both wins and shortfalls, or team split;
    ↪  dissatisfied:
    framed as loss/sellout/forced; not_stated: announcement conveys no
    ↪  evaluative
    stance on the terms.)
  framing: clear_win | partial_win | compromise | defeat_sellout | neutral
  speaker: short tag of who is voicing it (e.g. "local president", "bargaining
  ↪  team
    member", "rank-and-file worker"); the most senior/representative if
    ↪  multiple.
  supporting_quote: ONE short verbatim quote (<=300 chars) copied EXACTLY from
  ↪  the
    source text that best shows the sentiment. Never invent or paraphrase a
    ↪  quote.
  terms_mentioned: very short summary of the deal terms cited (or "").
  confidence: high | med | low.
This is academic sentiment research; charged union rhetoric ("sellout",
↪  "betrayed") is
sentiment DATA to record neutrally.

-- PHASE A -- CLASSIFY THE SAVED SOURCES
↪  -------------------------------------------
For each confirmed-deal row, read the row's SAVED source text (no web access in
↪  this
phase) and classify: tier, and the sentiment fields above. Write one CSV row per
↪  input
row, in input order, using the python csv module (never hand-format CSV):
  row,employer,union,source_type,domain,tier,satisfaction,framing,speaker,
  terms_mentioned,supporting_quote,confidence,notes
Rows with tier=none get satisfaction=not_stated, framing=neutral, empty
speaker/quote/terms. Resume by skipping row ids already present in the output.

-- PHASE B -- WEB-SEARCH THE GAP ROWS
↪  ----------------------------------------------
For each row Phase A left at tier=none, run 2-4 web searches for a
↪  protester-side
announcement of the SAME deal, e.g.:
  "<union> <employer> tentative agreement" / "<employer> <city> workers ratify
  ↪  contract"
  "<union local> <employer> statement" / "<employer> strike ends agreement
  ↪  <year>"
Include city/state and the deal year to disambiguate; prefer results dated
↪  between
Start Date and ~12 months after. Source priority: (1) the union's own site/local
site/press release/newsletter; (2) the union's social post; (3) union-aligned
↪  labor
```

```
press quoting the union; (4) a news outlet quoting the union/a worker. Fetch the
↪  page
and confirm an actual union/worker VOICE about the terms. Output columns:
  row,employer,union,city,state,found,found_url,found_source_type,tier,satisfac⌋
   ↪  tion,
  framing,speaker,terms_mentioned,supporting_quote,confidence,notes
DEFAULT TO found=no when you cannot locate a genuine protester-side voice -- do
↪  NOT
invent a quote or guess sentiment; give a brief reason in notes.

-- PHASE C -- VERIFY UNIT MATCH OF WEB-FOUND SOURCES
↪  ------------------------------
For every Phase-B row with found=yes, re-open found_url (if dead, try one
↪  re-search for
the same announcement) and verify the source is really about the SAME bargaining
↪  unit:
same employer (or a named facility/division of it) AND the same union/local,
↪  with the
quoted reaction coming from that unit's side. Verdicts:
  yes     -- source clearly names this employer AND this union/local and the
   ↪  deal it
             describes is that unit's (minor name variants are fine);
  partial -- right union OR right employer confirmed but the other side
   ↪  ambiguous, or a
             broader multi-unit deal that plausibly includes this unit, or a
              ↪  parent-
             union statement rather than the specific local;
  no      -- actually a different employer, union, locale, or dispute;
  unverifiable -- no source could be accessed or re-located.
Output columns:
  row,employer,union,found_url,unit_match,unit_match_evidence,quote_speaker_ok,
  verified_url,notes
unit_match_evidence (<=300 chars) must name the employer/facility + union/local
↪  as they
appear in the source. Also check quote_speaker_ok: the supporting_quote is a
union/worker voice, not the employer.

-- PHASE D -- ASSEMBLE THE FINAL DATASET
↪  ------------------------------------------
Merge Phase A with the verified Phase-B findings into one final CSV, one row per
confirmed-deal strike: keep the Phase-A classification where tier != none; fill
↪  gap
rows with Phase-B results only where unit_match is yes or partial (demote
↪  unit_match=no
or unverifiable back to tier=none). Carry a source flag distinguishing
↪  saved-source vs
web-found announcements, and keep the full quote + verification detail in a
↪  companion
quotes CSV. Report final counts: rows with an announcement (coverage rate), the
satisfaction and framing distributions, and how many announcements are
↪  union_authored
vs union_quoted_in_news vs web-found.
```

```
Before starting, confirm the input corpus file and output paths, then proceed.
↪  Ask me
nothing else -- when a judgment call is unclear, default to tier=none / found=no
↪  rather
than guessing.
```

## D Details on the Construction of Similarity Measures

The similarity measure between counties $i$ and $j$ for a given characteristic is defined as the Euclidean distance of category shares:

$$SM_{ij} = \sqrt{\sum_{k \in K} (\alpha_{ik} - \alpha_{jk})^2},$$

where $k$ indexes a category, $K$ is the set of all categories, $\alpha_{ik}$ is the share of population in county $i$ belonging to group $k$, and $\alpha_{jk}$ is the same for county $j$.

For political similarity, the categories include the shares of Democratic, Republican, and other-party voters in the 2020 presidential election. Specifically, $\alpha_{j,democrat}$ is the Democratic share in county $j$, $\alpha_{j,republican}$ is the Republican share, and $\alpha_{j,other}$ is the share voting for other parties.

For education, the categories are: less than high school, high school graduate but no college degree, and bachelor's degree or higher.

For ethnicity, the categories are: White, Black, Hispanic, Asian, and Other.

For occupations, we follow the Census classification. The management, business, science, and arts category includes management, business, and financial occupations; computer, engineering, and science occupations; education, legal, community service, arts, and media occupations; and healthcare practitioners and technical occupations. The service category includes healthcare support occupations; protective service occupations; food preparation and serving occupations; building and grounds cleaning and maintenance occupations; and personal care and service occupations. The sales and office category includes sales and related occupations, as well as office and administrative support occupations. The natural resources, construction, and maintenance category includes farming, fishing, and forestry occupations; construction and extraction occupations; and installation, maintenance, and repair occupations. Finally, the production, transportation, and material moving category includes production occupations, transportation occupations, and material moving occupations.

For industries, the categories are: agriculture, forestry, fishing and hunting, and mining; construction; manufacturing; wholesale trade; retail trade; transportation and warehousing, and utilities; information; finance, insurance, real estate, rental and leasing; professional, scientific, management, administrative, and waste management services; educational services, health care, and social assistance; arts, entertainment, recreation, accommodation, and food services; other services (except public administration); and public administration.

Additional Model Specifications

To examine the role of unions' social media presence, we estimate two modifications of Equation (1). The first augments the base specification with the average Facebook follower presence of the strikes a county is exposed to, together with its interaction with the raw exposure measure:

$$\begin{aligned} y_{it} = \exp\Big(\beta_1 \sum_{k=t-1}^{t-m} y_{ik} + \beta_2 PCIST_{it} + \beta_3 R_{it} + \beta_4 \left(PCIST_{it} \times R_{it}\right) \\ + \beta_5 \sum_{k=t-1}^{t-n} PCIST_{ik} + \beta_6 R_{it}^{L} + \beta_7 \Big( \sum_{k=t-1}^{t-n} PCIST_{ik} \times R_{it}^{L}\Big) + C_i + T_t\Big) \times \epsilon_{it}, \end{aligned} \tag{2}$$

where $R_{it}$ is the average follower presence of county $i$'s exposure in month $t$, defined as $R_{it} = PCIST_{it}^{F}/PCIST_{it}^{C}$ (and set to zero when $PCIST_{it}^{C} = 0$). Here $PCIST_{it}^{F} = \sum_{j \neq i} PCI_{ij} \sum_{s \in S_{jt}^{C}} \ln(1+F_s)$ is follower-weighted exposure, where $S_{jt}^{C}$ is the set of strikes in county $j$ in month $t$ whose organizing local unions have an observed Facebook following and $F_s$ is the combined following of the local unions behind strike $s$, and $PCIST_{it}^{C} = \sum_{j \neq i} PCI_{ij} \, |S_{jt}^{C}|$ is the corresponding exposure to the *number* of such strikes. Because the numerator sums $\ln(1 + F_s)$ strike by strike and the denominator counts exactly those strikes, $R_{it}$ is the average log-following per exposed strike. The lagged reach term $R_{it}^{L}$ is the analogous ratio of the four-month sums. As in Equation (1), right-hand-side variables are standardized by their sample standard deviations; the exposure and reach terms are additionally mean-centered before the interactions are formed, so that their main effects are evaluated at mean reach and mean exposure, respectively.

The second modification replaces PCI-weighted exposure with its follower-weighted counterpart:

$$y_{it} = \exp\Big(\beta_1 \sum_{k=t-1}^{t-m} y_{ik} + \beta_2 PCIST_{it}^{F} + \beta_3 \sum_{k=t-1}^{t-n} PCIST_{ik}^{F} + C_i + T_t\Big) \times \epsilon_{it}, \tag{3}$$

so that $\beta_2$ and $\beta_3$ capture exposure amplified by both social connectivity and the social media visibility of the unions involved in the observed strikes.

## E Right-to-Work States in the Sample Period

The right-to-work (RTW) indicator used in Section 5 and Tables B11 and B14 equals one for counties in the 27 states with right-to-work laws in force during the 2021–2024 sample period: Alabama, Arizona, Arkansas, Florida, Georgia, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Nebraska, Nevada, North Carolina, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, West Virginia, Wisconsin, and Wyoming. The indicator is time-invariant. Michigan is the one state whose status changed within the sample: its 2012 right-to-work law was repealed effective February 2024, so it was right-to-work for the first 37 of the 48 sample months and is classified as right-to-work throughout.[23]

[23] National Conference of State Legislatures, "Right-to-Work States," https://www.ncsl.org/labor-and-employment/right-to-work-states; U.S. Department of Labor, "State Right-to-Work Laws," https://www.dol.gov/agencies/whd/state/right-to-work.

## F Details on the Construction of the Union-Connectedness (UCI) Measures

We complement the social (PCI) and demographic proximity measures with a union-connectedness network that links two counties to the extent that the same national or international unions maintain local chapters in both. The premise is organizational: sister locals of the same international share strike funds, staff, legal support, and pattern-bargaining templates, so counties whose workers belong to the same unions are linked over and above any social or geographic proximity. Letting $A_{cu}$ record union $u$'s footprint in county $c$, connectedness is the shared co-membership $\mathrm{UCI}_{ij} = \sum_u A_{iu}A_{ju}$; we measure the footprint three ways—union presence, the number of locals, and membership—and assemble it from three complementary sources (a union counts as present if any source places it there): DOL OLMS financial filings (private and mixed unions, at the filing office), NLRB certified representation elections (private-sector worksites, which correctly spread regionally consolidated "mega-locals"), and the IRS 501(c)(5) Business Master File (public-sector locals exempt from federal labor reporting). We fix the network to a pre-period (2020) to avoid reflection, population-weight and row-normalize it as for the other proximity measures, and enter same-month exposure and its four-month lag. An inverse-frequency down-weighting of ubiquitous unions gives materially identical results and is omitted.

*Sources and forms.* The OLMS component uses the Department of Labor's Office of Labor-Management Standards union annual financial reports for filing year 2020, Forms LM-2, LM-3, and LM-4 (keeping each union's latest amended filing), with the national affiliation taken from the report's affiliation code and the location from the filing office. Unaffiliated locals, AFL-CIO umbrella and trade-department filings, and aggregate or headquarters-level reports (national headquarters, international-union, state-federation, and labor-council designations) are excluded, so that links reflect local chapters rather than double-counted aggregates. The NLRB component uses representation elections closed with a certification of representative, located at the worksite, with eligible voters as the membership proxy. The IRS component uses the Exempt Organizations Business Master File restricted to organizations exempt under section 501(c)(5) (labor organizations), located at the filing address. The IRS source yields union presence and the count of locals in a county directly; membership is derived from the file's revenue field, which comes from each organization's most recent Form 990-series return (Form 990, 990-EZ, or 990-N), divided by typical annual dues per member.[24]

[24] The division is by $700, which corresponds to approximately 1.2% of union members' median annual earnings of roughly $59,500 in 2020 (U.S. Bureau of Labor Statistics, *Union Members — 2020*, https://www.bls.gov/news.release/archives/union2_01222021.pdf).

*National affiliations.* OLMS reports identify the national affiliation directly. For NLRB and IRS records, whose union names are free text, we map names to national and international unions using a keyword crosswalk covering: AFT, NEA, AFSCME, SEIU, IAFF, FOP, AFGE, NTEU, NFFE, NATCA, ATDA, AFA, ALPA, AFM, NALC, APWU, NRLCA, NPMHU, the Teamsters (IBT), IAM, IBEW, IUOE, LIUNA, the Carpenters (UBC), the Painters (IUPAT), the Plumbers and Pipefitters (UA), USW, CWA, UAW, UFCW, NNU, ATU, TWU, SMART, the Ironworkers, the Boilermakers, the Bricklayers (BAC), IATSE, UNITE HERE, OPEIU, ILWU, ILA, UMWA, BCTGM, Workers United, SPFPA, BMWED, BLET, BRS, UWUA, IUEC, and the Insulators (HFIA). Among police organizations, only the Fraternal Order of Police is treated as a national federation; independent police benevolent and officers' associations are retained as present but left unlinked, to avoid generating spurious shared-affiliation ties.

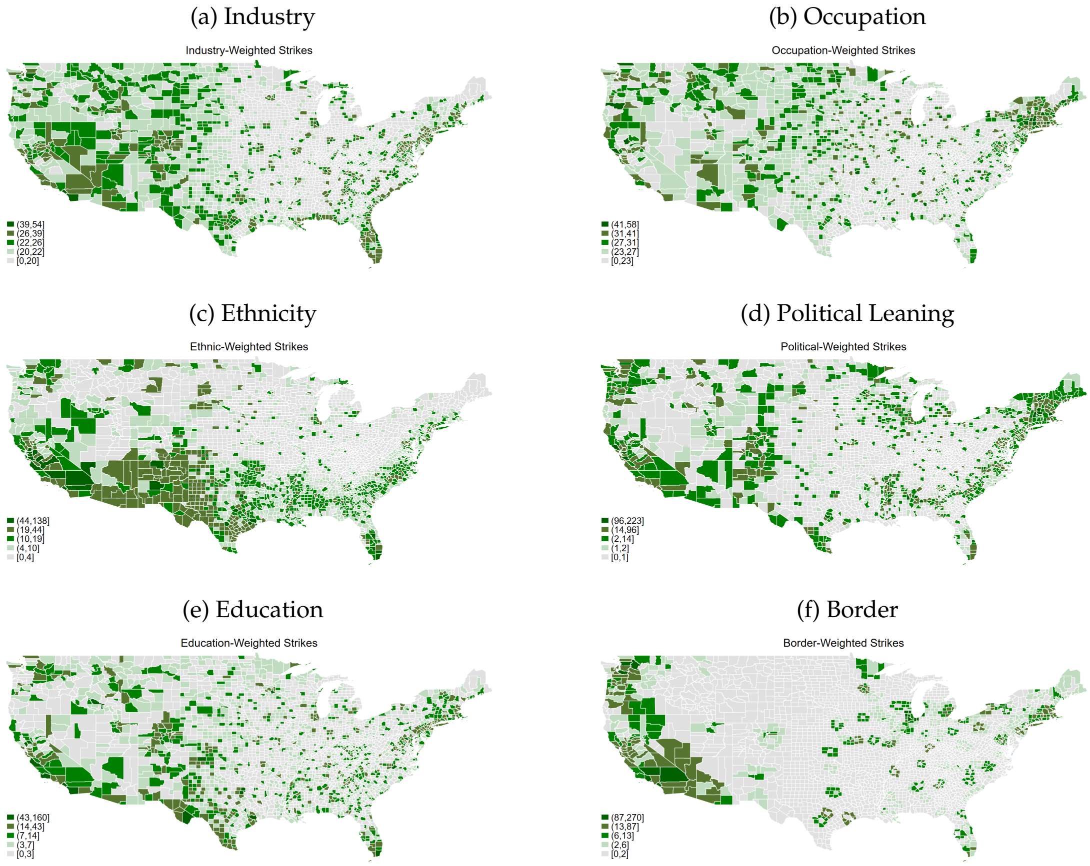
*Inverse-document-frequency weighting.* Nearly every county hosts locals of the largest unions, so two counties sharing one of them are barely distinguished from any other pair. We therefore weight each shared affiliation by its inverse document frequency, $\log(N/n_a)$, where $N$ is the number of counties in the network and $n_a$ the number in which affiliation $a$ has a local. Sharing a rare, specialized union then contributes more to the index than sharing a ubiquitous one. The weighting is applied before the population weighting and row normalization used for every proximity network. The un-weighted variants are also constructed and estimated; they yield the same sign pattern and similar magnitudes with wider standard errors, and the contemporaneous PCI coefficient is unaffected by the choice.

## G Additional Figures

Unless a figure's note states otherwise, every figure in this appendix that describes the strike data is built from the monthly, county-level panel constructed from the Cornell ILR Labor Action Tracker over 2021–2024, the paper's full sample period. We therefore state the window once here rather than repeating it beneath each figure.

The contextual figures (Figures A3–A8), discussed in Appendix A, are the exception: they draw on external sources—the BLS Work Stoppages program, the Federal Mediation and Conciliation Service, JOLTS, and Google Trends—over the longer historical windows indicated in each panel and note, precisely in order to place the 2021–2024 study period in perspective.

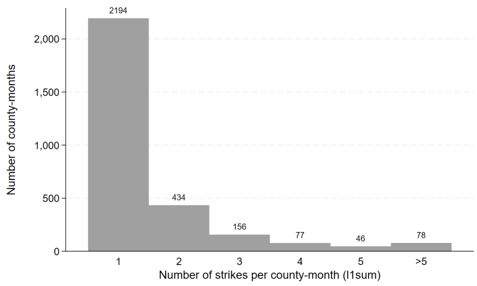
Figure A1: Strikes by Different Exposure Types



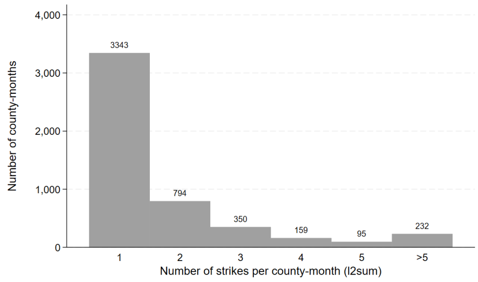

*Note*: This figure compares strikes based on various distance exposures: industry, occupation, ethnicity, political leaning, education, and border. Deeper map shades correspond to higher strike counts. Given that most US counties saw no strikes, darker shades begin at the 75th percentile. Data is sourced from the ILR Labor Action Tracker.

Figure A2: Distribution of Cumulative Strikes over Previous Months

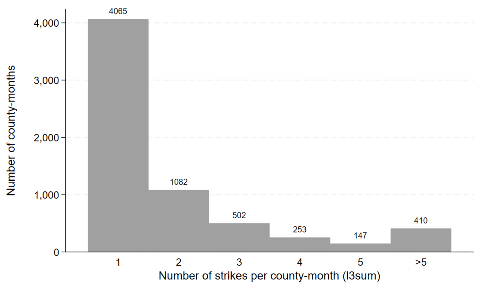
(a) Strikes from Last Month



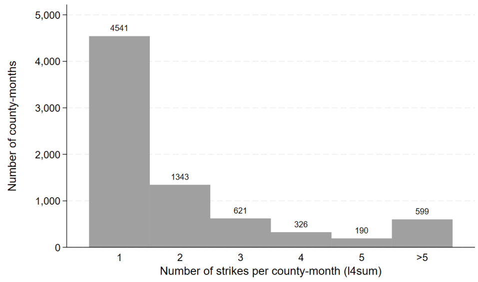

(b) Strikes from Last 2 Months



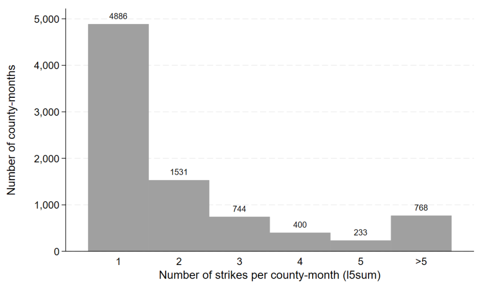

(c) Strikes from Last 3 Months



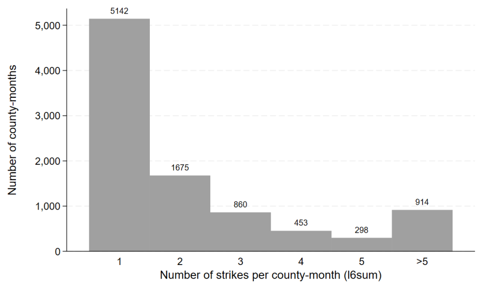

(d) Strikes from Last 4 Months




(e) Strikes from Last 5 Months



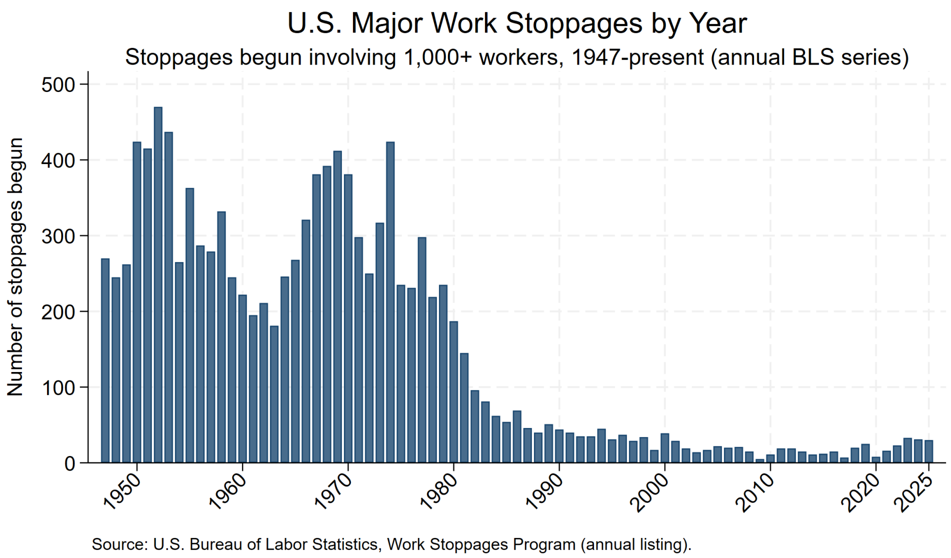

(f) Strikes from Last 6 Months



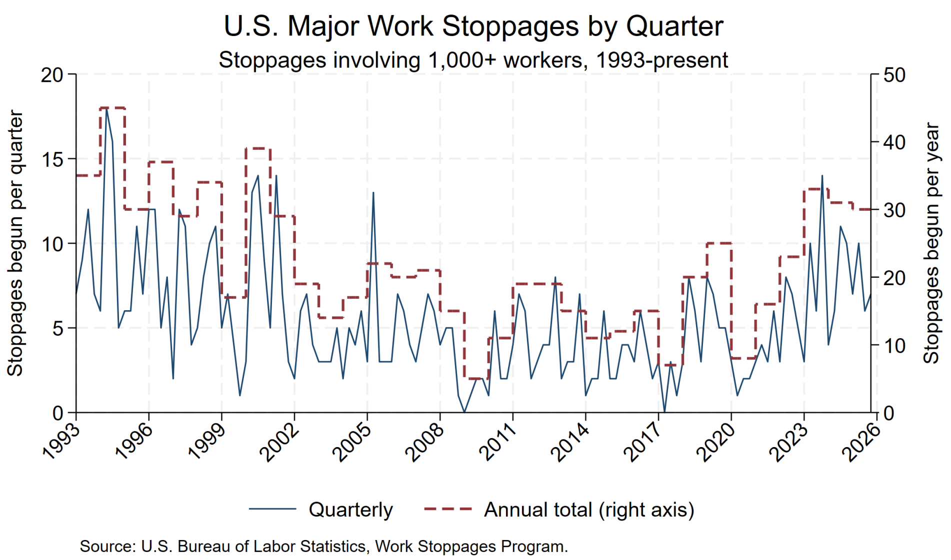

*Notes*: Each panel shows the distribution of strikes per county-month, calculated as the cumulative number of strikes over the previous 1 to 6 months. The data are sourced from the ILR Labor Action Tracker.

Figure A3: Major Work Stoppages (1,000+ Workers), BLS

(a) By year, 1947–2025

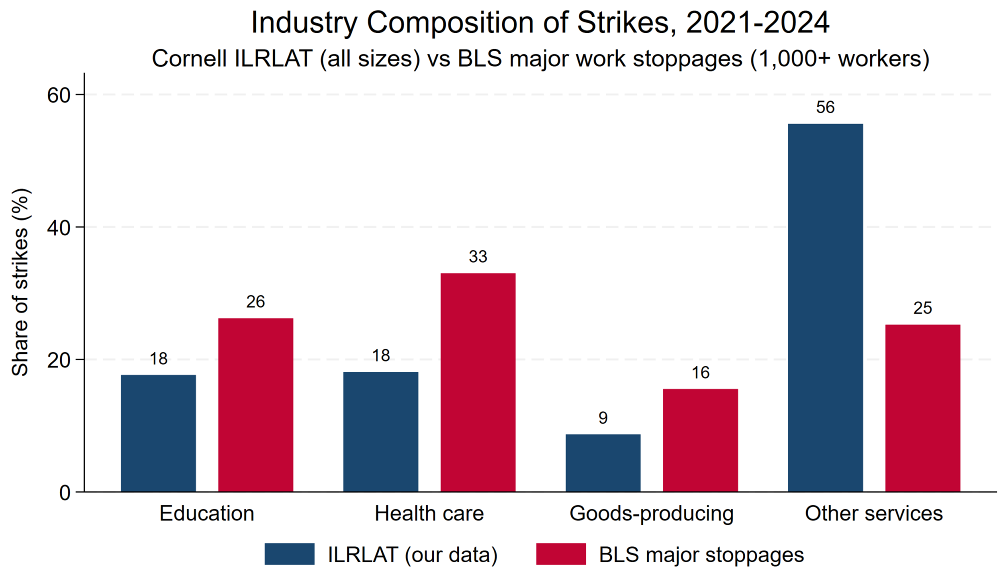



(b) By quarter, 1993–2025




*Note*: Counts of work stoppages involving 1,000 or more workers that began in each period. Panel (a) uses the BLS annual listing (1947–present); panel (b) the BLS monthly listing (1993–present), aggregated to quarters, with annual totals overlaid as a dashed step line on the right-hand axis. Source: U.S. Bureau of Labor Statistics, Work Stoppages program.

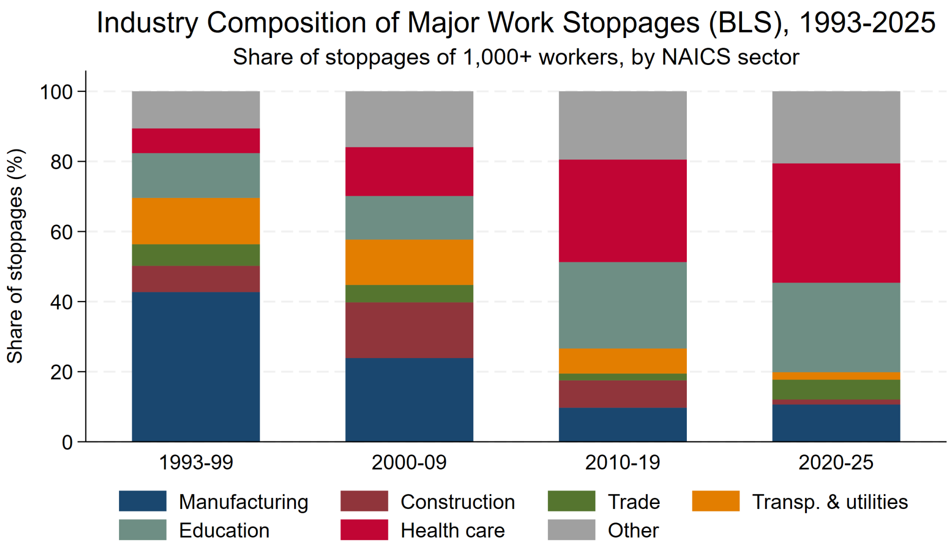
Figure A4: Industry Composition of Strikes, 2021–2024: ILRLAT vs. BLS



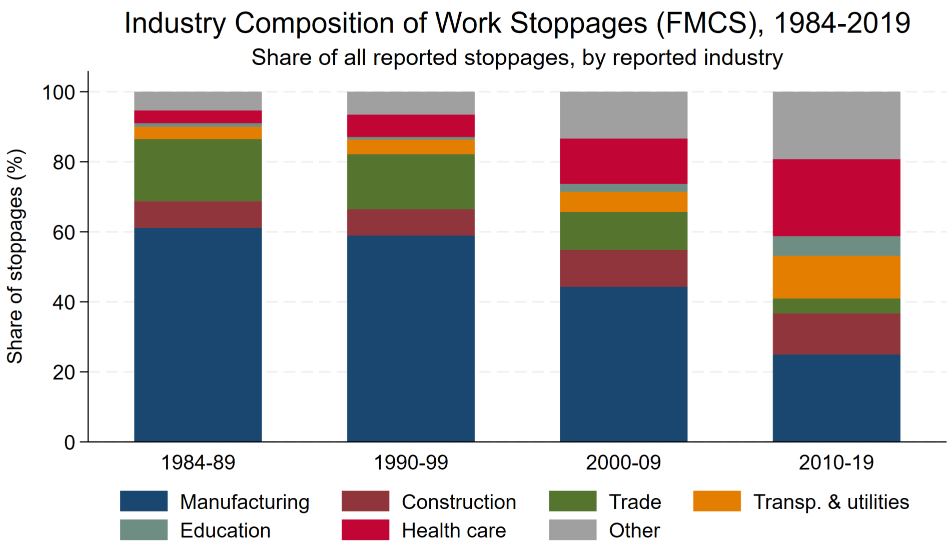

*Note*: Percent share of strikes in each broad sector for the Cornell ILR Labor Action Tracker (all sizes) and the BLS Work Stoppages program (major stoppages of 1,000+ workers), 2021–2024. Sectors follow the paper's grouping: education, health care, goods-producing (agriculture, construction, manufacturing, mining), and other service-providing. BLS stoppages are classified by NAICS sector from the monthly listing. Sources: Cornell ILRLAT; U.S. Bureau of Labor Statistics, Work Stoppages program.

Figure A5: Industry Composition of Strikes over Time

(a) BLS major stoppages, 1993–2025

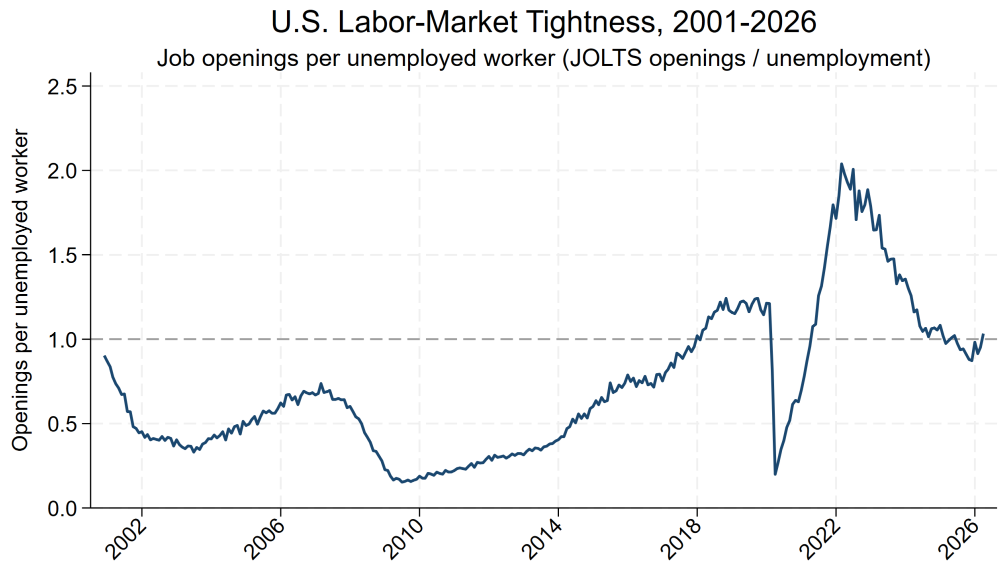



(b) FMCS all stoppages, 1984–2019



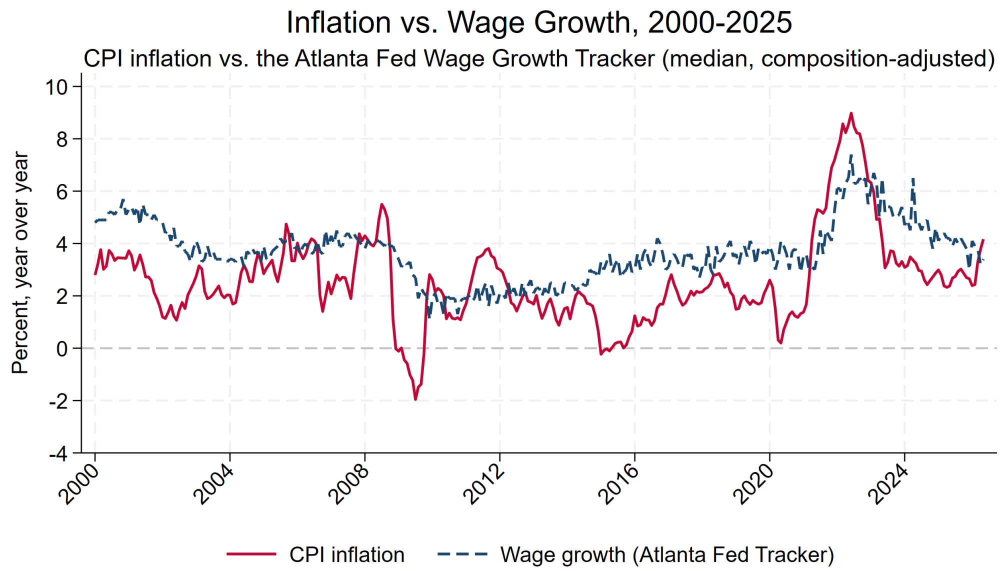

*Note*: Each bar gives the share of work stoppages in a period accounted for by each broad sector. Panel (a) classifies BLS major stoppages (1,000+ workers) by NAICS sector from the monthly listing; panel (b) classifies FMCS stoppages by their reported industry. "Other" collects remaining sectors (information, accommodation and food, mining, public administration, etc.). Education is sizable in panel (a) but small in panel (b) because FMCS does not cover most public-sector (e.g., public-school) employees. Sources: U.S. Bureau of Labor Statistics, Work Stoppages program; Federal Mediation and Conciliation Service work stoppages file.

Figure A6: U.S. Labor-Market Tightness, 2001–2026



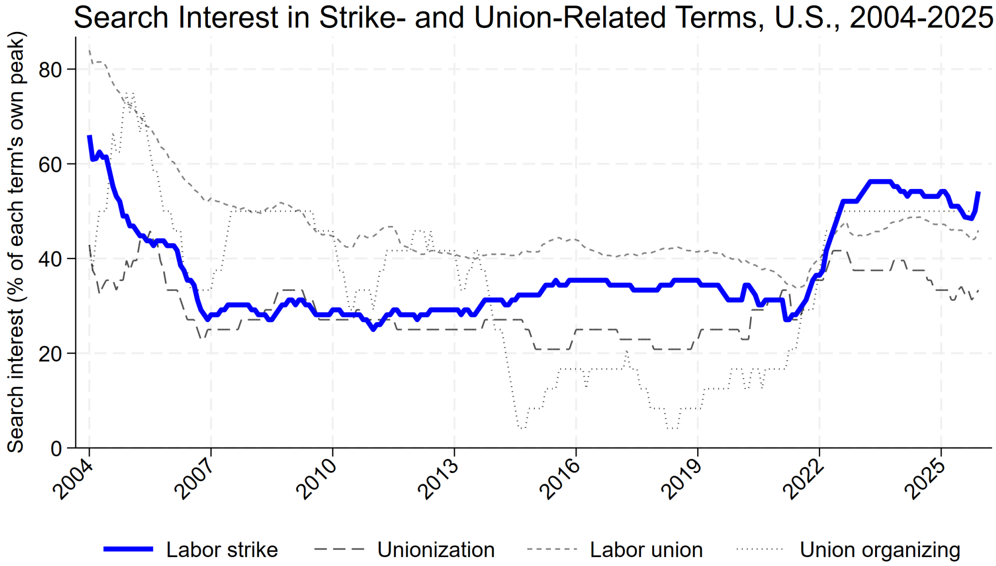

*Note*: Ratio of job openings to unemployed workers (JOLTS job openings divided by the number of unemployed). Values above one indicate more openings than unemployed workers; the ratio peaked near 2.0 in 2022, a record for the JOLTS era (which begins in December 2000). Source: U.S. Bureau of Labor Statistics, Job Openings and Labor Turnover Survey and Current Population Survey, via FRED.

Figure A7: Inflation vs. Wage Growth, 2000–2025




*Note*: Year-over-year consumer-price inflation and the Atlanta Fed Wage Growth Tracker (median 12-month wage growth for matched individuals, which is composition-adjusted). When inflation exceeds wage growth, the median worker's real wage falls. Sources: BLS Consumer Price Index and Federal Reserve Bank of Atlanta Wage Growth Tracker, via FRED.

Figure A8: Search Interest in Strike- and Union-Related Terms, United States, 2004–2025




*Note*: Monthly Google Trends search interest (United States) for "labor strike" (solid blue), "unionization," "labor union," and "union organizing" (grey, dashed/dotted), 2004–2025. Each series is rescaled to its own sample maximum (= 100), so the figure is informative about the timing of changes within each term but not about differences in volume across terms; each line is a 12-month centered moving average. Source: Google Trends.

## H Additional Tables

Table B2: Sensitivity of Strike Incidence to Alternative Lag Structures of Own-County Strikes

| | (1) L1 | (2) L2 | (3) L3 | (4) L4 | (5) L5 | (6) L6 |
|---|---|---|---|---|---|---|
| Own-County Strikes, Lags 1–1 Sum | -0.03** | | | | | |
| | (0.01) | | | | | |
| Lagged Total Strikes in Own County, 2-Month Sum | | -0.02* | | | | |
| | | (0.01) | | | | |
| Own-County Strikes, Lags 1–3 Sum | | | -0.02** | | | |
| | | | (0.01) | | | |
| Own-County Strikes, Lags 1–4 Sum | | | | -0.04*** | | |
| | | | | (0.01) | | |
| Own-County Strikes, Lags 1–5 Sum | | | | | -0.04** | |
| | | | | | (0.02) | |
| Own-County Strikes, Lags 1–6 Sum | | | | | | -0.06*** |
| | | | | | | (0.02) |
| PCI-Weighted Strikes of Other Counties, Same-Month | 0.16*** | 0.16*** | 0.16*** | 0.16*** | 0.16*** | 0.16*** |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Lagged PCI-Weighted Strikes of Other Counties, 4-Month Sum | 0.07 | 0.07 | 0.07 | 0.08 | 0.07 | 0.07 |
| | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) |
| N | 27236 | 27236 | 27236 | 27236 | 26230 | 25284 |

*Notes:* PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B3: Sensitivity of Strike Incidence to Alternative Lag Structures of Social Network Exposure

| | (1) L1 | (2) L2 | (3) L3 | (4) L4 | (5) L5 | (6) L6 |
|---|---|---|---|---|---|---|
| Lagged Total Strikes in Own County, 2-Month Sum | -0.01* | -0.02* | -0.02* | -0.02* | -0.02* | -0.03** |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| PCI-Weighted Strikes of Other Counties, Same-Month | 0.16*** | 0.15*** | 0.15*** | 0.16*** | 0.16*** | 0.16*** |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| PCI-Weighted Strikes, Lags 1–1 Sum | 0.03 | | | | | |
| | (0.03) | | | | | |
| PCI-Weighted Strikes, Lags 1–2 Sum | | 0.07* | | | | |
| | | (0.04) | | | | |
| PCI-Weighted Strikes, Lags 1–3 Sum | | | 0.08* | | | |
| | | | (0.05) | | | |
| Lagged PCI-Weighted Strikes of Other Counties, 4-Month Sum | | | | 0.07 | | |
| | | | | (0.05) | | |
| PCI-Weighted Strikes, Lags 1–5 Sum | | | | | 0.06 | |
| | | | | | (0.05) | |
| PCI-Weighted Strikes, Lags 1–6 Sum | | | | | | 0.05 |
| | | | | | | (0.06) |
| N | 29210 | 29210 | 28215 | 27236 | 26230 | 25284 |

*Notes:* PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B4: OLS Estimates of Changes in Strike Incidence

| | (1)<br>All counties | (2)<br>Ever-strike counties |
|---|---|---|
| $\Delta$ Own-County Strikes, 1-Month Lag | -0.49*** | -0.49*** |
| | (0.01) | (0.01) |
| $\Delta$ PCI-Weighted Strikes, Same-Month | 0.02*** | 0.02*** |
| | (0.00) | (0.01) |
| $\Delta$ PCI-Weighted Strikes, 1-Month Lag | 0.01** | 0.01 |
| | (0.00) | (0.01) |
| N | 144072 | 28980 |

*Notes:* OLS (`reghdfe`) on first-differenced data: the outcome and all right-hand-side variables are expressed as month-to-month changes and standardized. Column (1) uses all counties; column (2) restricts to counties that experience at least one strike over 2021–2024. All columns include labor-market controls, with county and month–year fixed effects; standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B5: Correlation of Own-County and Network Exposure to Strikes, Negative Binomial Estimation

| | (1)<br>NB | (2)<br>+ Controls × Year | (3)<br>NB | (4)<br>+ Controls × Year |
|---|---|---|---|---|
| Lagged Total Strikes in Own County, 2-Month Sum | 0.07** | 0.05*** | 0.20*** | 0.21*** |
| | (0.03) | (0.02) | (0.03) | (0.03) |
| PCI-Weighted Strikes of Other Counties, Same-Month | 0.01 | 0.11*** | 0.20*** | 0.19*** |
| | (0.04) | (0.03) | (0.07) | (0.06) |
| Lagged PCI-Weighted Strikes of Other Counties, 4-Month Sum | -0.54*** | -0.14*** | -0.18** | -0.18** |
| | (0.07) | (0.05) | (0.07) | (0.07) |
| Sample | Ever-strike | Ever-strike | All counties | All counties |
| Observations | 28,072 | 27,852 | 138,204 | 136,928 |

*Notes:* Negative binomial regression (`nbreg`), 2021–2024. The negative binomial likelihood is defined only over non-negative integers, so—unlike the PPML specifications, which are consistent for any non-negative outcome—the dependent variable here must be the raw strike count rather than strikes per 1,000 residents, with county population entering as an exposure offset. County fixed effects do not converge under the negative binomial, so state and month–year fixed effects are used instead, and identification is correspondingly cross-sectional rather than within-county. Columns (1)–(2) restrict the sample to counties with at least one strike, for comparability with the PPML baseline; columns (3)–(4) use the full county panel. Standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B6: Flexible Lag Structure of Own Strikes and Social Network Exposure to Strikes

| | (1) | (2) |
|---|---|---|
| 1-Month Lagged Total Strikes in Own County | -0.03** | -0.03** |
| | (0.01) | (0.01) |
| 2-Month Lagged Total Strikes in Own County | -0.01 | -0.01 |
| | (0.01) | (0.01) |
| 3-Month Lagged Total Strikes in Own County | -0.01 | -0.01 |
| | (0.01) | (0.01) |
| 4-Month Lagged Total Strikes in Own County | -0.05** | -0.05** |
| | (0.02) | (0.02) |
| 5-Month Lagged Total Strikes in Own County | -0.02 | -0.02 |
| | (0.02) | (0.02) |
| 6-Month Lagged Total Strikes in Own County | -0.05*** | -0.05*** |
| | (0.01) | (0.01) |
| PCI-Weighted Strikes of Other Counties, Same-Month | 0.16*** | 0.16*** |
| | (0.03) | (0.03) |
| 1-Month Lagged PCI-Weighted Strikes of Other Counties | 0.01 | 0.00 |
| | (0.04) | (0.04) |
| 2-Month Lagged PCI-Weighted Strikes of Other Counties | 0.05 | 0.06* |
| | (0.03) | (0.03) |
| 3-Month Lagged PCI-Weighted Strikes of Other Counties | -0.01 | -0.01 |
| | (0.03) | (0.03) |
| 4-Month Lagged PCI-Weighted Strikes of Other Counties | 0.03 | 0.04 |
| | (0.04) | (0.04) |
| 5-Month Lagged PCI-Weighted Strikes of Other Counties | 0.01 | -0.00 |
| | (0.04) | (0.04) |
| 6-Month Lagged PCI-Weighted Strikes of Other Counties | -0.02 | -0.01 |
| | (0.05) | (0.05) |
| Labor Market Condition Controls | | x |
| County Fixed Effects | x | x |
| Month–Year Fixed Effects | x | x |
| Observations | 25,284 | 24,948 |

*Notes:* The cumulative lag sums in the baseline are replaced with separate monthly lags (1–6) of own-county strikes and of PCI-weighted strike exposure; column (2) adds contemporaneous and six monthly lags of county unemployment and PCI-weighted unemployment. PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B7: Strike Incidence and Network Exposure to Strike Participation

| | (1) Total | (2) Education | (3) Healthcare | (4) Oth-Serv | (5) Goods |
|---|---|---|---|---|---|
| Lagged Own-County Participation, 2-Month Sum | -0.01<br>(0.01) | | | | |
| PCI-Weighted Participation, Same-Month | 0.05***<br>(0.01) | | | | |
| Lagged PCI-Weighted Participation, 4-Month Sum | 0.04*<br>(0.02) | | | | |
| Lagged Own-County Participation (Education), 2-Month Sum | | -0.03<br>(0.05) | | | |
| PCI-Weighted Participation (Education), Same-Month | | 0.08***<br>(0.02) | | | |
| Lagged PCI-Weighted Participation (Education), 4-Month Sum | | -0.09<br>(0.06) | | | |
| Lagged Own-County Participation (Healthcare), 2-Month Sum | | | -0.02<br>(0.02) | | |
| PCI-Weighted Participation (Healthcare), Same-Month | | | 0.07***<br>(0.03) | | |
| Lagged PCI-Weighted Participation (Healthcare), 4-Month Sum | | | -0.05<br>(0.05) | | |
| Lagged Own-County Participation (Other Service-Providing), 2-Month Sum | | | | -0.01<br>(0.01) | |
| PCI-Weighted Participation (Other Service-Providing), Same-Month | | | | 0.03*<br>(0.01) | |
| Lagged PCI-Weighted Participation (Other Service-Providing), 4-Month Sum | | | | 0.05***<br>(0.02) | |
| Lagged Own-County Participation (Goods-Producing), 2-Month Sum | | | | | -0.09<br>(0.16) |
| PCI-Weighted Participation (Goods-Producing), Same-Month | | | | | 0.06**<br>(0.02) |
| Lagged PCI-Weighted Participation (Goods-Producing), 4-Month Sum | | | | | -0.02<br>(0.07) |
| N | 27236 | 10472 | 10472 | 17072 | 9417 |

*Notes:* The outcome is the number of strikes. Column (1) regresses total strikes on contemporaneous and four-month lagged PCI-weighted total strike participation, controlling for lagged own-county participation. Columns (2)–(5) report the number of strikes in education, healthcare, other service-providing, and goods-producing industries, respectively, on contemporaneous and four-month lagged PCI-weighted participation in the corresponding industry. PPML (`ppmlhdfe`); right-hand-side variables enter in standard deviations; standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B8: Facebook-Follower Reach and Strike Diffusion: Volume/Intensity Decomposition, Overall, and Same versus Cross Industry

| | Decomposition | Overall | Same vs. cross industry | | |
|---|---|---|---|---|---|
| | (1) Vol+Reach | (2) Overall | (3) | (4) | (5) |
| Own-county strikes, 2-month lag | -0.021$^{*}$ | -0.021$^{*}$ | | | |
| | (0.011) | (0.011) | | | |
| Standard PCI exposure (volume), same-mo | 0.165$^{***}$ | | | | |
| | (0.030) | | | | |
| Standard PCI exposure (volume), 4-mo lag | -0.000 | | | | |
| | (0.081) | | | | |
| Mean follower presence (intensity), same-mo | -0.073 | | | | |
| | (0.100) | | | | |
| Mean follower presence (intensity), 4-mo lag | -0.298 | | | | |
| | (0.508) | | | | |
| Volume $\times$ reach, same-mo | 0.001 | | | | |
| | (0.023) | | | | |
| Volume $\times$ reach, 4-mo lag | 0.206 | | | | |
| | (0.168) | | | | |
| Follower-weighted PCI exposure, same-mo | | 0.166$^{***}$ | | | |
| | | (0.039) | | | |
| Follower-weighted PCI exposure, 4-mo lag | | 0.049 | | | |
| | | (0.055) | | | |
| Same-industry follower-wtd exposure, same-mo | | | **0.149**$^{***}$ | **0.149**$^{***}$ | **0.141**$^{***}$ |
| | | | **(0.028)** | **(0.027)** | **(0.028)** |
| Same-industry follower-wtd exposure, 4-mo lag | | | **0.049** | **0.011** | **-0.002** |
| | | | **(0.034)** | **(0.035)** | **(0.039)** |
| Cross-industry follower-wtd exposure, same-mo | | | 0.011 | 0.014 | 0.034 |
| | | | (0.035) | (0.032) | (0.035) |
| Cross-industry follower-wtd exposure, 4-mo lag | | | 0.032 | 0.067 | 0.075 |
| | | | (0.049) | (0.047) | (0.054) |
| $N$ | 27236 | 27236 | 108325 | 47652 | 47433 |
| County $\times$ industry FE | | | | Yes | Yes |
| County FE | Yes | Yes | Yes | | |
| Month FE | Yes | Yes | | Yes | |
| Industry $\times$ month FE | – | – | Yes | | Yes |

*Notes:* Follower reach is measured with the log of one plus the number of Facebook followers of the leading union in each strike (the log tames the extreme skew of raw counts). Column (1) decomposes the follower-weighted exposure into *volume* (standard PCI-weighted strike exposure) and *intensity* (the exposure-weighted average follower reach of the strikes one is exposed to, computed as follower-weighted exposure divided by follower-covered exposure), plus their interaction; volume, intensity, and the interaction each enter same-month and as four-month lagged sums. Column (2) is overall strikes on the follower-weighted PCI exposure. Columns (3)–(5) stack each county–month into four industry-group observations and split the follower-weighted exposure into same-industry versus cross-industry (the sum of the other three groups' follower-weighted exposure; raw sums standardized by their pooled standard deviations), under three fixed-effects schemes (indicated in the table). PPML (`ppmlhdfe`); right-hand-side variables enter in standard deviations; standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B9: Correlations of Own County and Social Network Exposure to Strike Outcomes

| | (1) Strikes | (2) Deals |
|---|---|---|
| Lagged Own-County Deals, 2-Month Sum | -0.007 | -0.156*** |
| | (0.013) | (0.035) |
| PCI-Weighted Deals of Other Counties, Same-Month | -0.000 | 0.013 |
| | (0.031) | (0.039) |
| Lagged PCI-Weighted Deals, 4-Month Sum | -0.014 | 0.043 |
| | (0.051) | (0.073) |
| Lagged Total Strikes in Own County, 2-Month Sum | -0.020* | 0.150*** |
| | (0.011) | (0.053) |
| PCI-Weighted Strikes of Other Counties, Same-Month | 0.158*** | -0.005 |
| | (0.029) | (0.063) |
| Lagged PCI-Weighted Strikes of Other Counties, 4-Month Sum | 0.075 | 0.030 |
| | (0.052) | (0.086) |
| County Fixed Effects | x | x |
| Month–Year Fixed Effects | x | x |
| Observations | 27236 | 16236 |

*Notes:* A deal is a settlement reached within a year of strike onset, classified from public news coverage and verified (Appendix B). Deal counts are restricted to *single-county* settlements – those whose striking-unit footprint lies in one county – assigned to that county and dated by the settlement month; settlements attributed to legislation or policy are excluded. PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. Right-hand-side variables enter in standard deviations. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B10: Cross-Sector Spillover Analysis: Public vs. Private PCI-Weighted Strike Exposure

| | Outcome: Public strikes | | | | Outcome: Private strikes | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) Both | (2) Own sector | (3) Cross sector | (4) All | (5) Both | (6) Own sector | (7) Cross sector | (8) All |
| PCI-Weighted Strikes of Other Counties, Same-Month | | | | 0.100 | | | | 0.164*** |
| | | | | (0.078) | | | | (0.028) |
| Lagged PCI-Weighted Strikes of Other Counties, 4-Month Sum | | | | 0.139 | | | | 0.073 |
| | | | | (0.108) | | | | (0.057) |
| PCI-Weighted Public Strikes, Same-Month | 0.092*** | 0.106*** | | | -0.000 | | 0.014 | |
| | (0.033) | (0.030) | | | (0.029) | | (0.026) | |
| Lagged PCI-Weighted Public Strikes, 4-Month Sum | -0.031 | -0.009 | | | 0.039 | | 0.061 | |
| | (0.080) | (0.079) | | | (0.046) | | (0.045) | |
| PCI-Weighted Private Strikes, Same-Month | 0.001 | | 0.008 | | 0.155*** | 0.156*** | | |
| | (0.082) | | (0.079) | | (0.024) | (0.024) | | |
| Lagged PCI-Weighted Private Strikes, 4-Month Sum | 0.153* | | 0.193** | | 0.057 | 0.067 | | |
| | (0.089) | | (0.087) | | (0.054) | (0.051) | | |
| Lagged Own-County Public Strikes, 2-Month Sum | -0.111*** | -0.110*** | -0.111*** | -0.111*** | | | | |
| | (0.029) | (0.029) | (0.030) | (0.030) | | | | |
| Lagged Own-County Private Strikes, 2-Month Sum | | | | | -0.004 | -0.004 | -0.004 | -0.005 |
| | | | | | (0.011) | (0.011) | (0.011) | (0.011) |
| County Fixed Effects | x | x | x | x | x | x | x | x |
| Month–Year Fixed Effects | x | x | x | x | x | x | x | x |
| Observations | 14168 | 14168 | 14168 | 14168 | 22616 | 22616 | 22616 | 22616 |

*Notes:* Cross-sector spillover analysis splitting PCI-weighted strike exposure into its public- and private-sector components. The outcome is the count of public strikes in columns (1)–(4) and of private strikes in columns (5)–(8). Within each block, "both" includes both sector exposures, "own sector" only the same-sector exposure, "cross sector" only the other-sector exposure, and "all" the pooled (undifferentiated) PCI exposure. PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B11: Network Diffusion by Right-to-Work Status: All Strikes, and Private versus Public Sector

| | (1) All: RTW | (2) All: Non-RTW | (3) All: Interaction | (4) Private: Interaction | (5) Public: Interaction | (6) Stacked: Triple |
|---|---|---|---|---|---|---|
| Own-county lagged strikes, 2-month sum | -0.014 | -0.039*** | -0.021* | -0.004 | -0.111*** | -0.016 |
| | (0.012) | (0.015) | (0.011) | (0.010) | (0.029) | (0.010) |
| PCI-weighted strike exposure, same-month | 0.291*** | 0.110*** | 0.150*** | 0.144*** | 0.095*** | 0.144*** |
| | (0.080) | (0.033) | (0.031) | (0.024) | (0.027) | (0.024) |
| × Right-to-work, same-month | | | 0.090 | 0.167** | 0.131 | 0.165** |
| | | | (0.080) | (0.077) | (0.166) | (0.077) |
| × Public sector, same-month | | | | | | -0.050 |
| | | | | | | (0.036) |
| × RTW × Public, same-month | | | | | | -0.040 |
| | | | | | | (0.180) |
| PCI-weighted strike exposure, 4-month lag | 0.309 | 0.004 | 0.078 | 0.074 | -0.024 | 0.076 |
| | (0.223) | (0.053) | (0.049) | (0.051) | (0.074) | (0.052) |
| × Right-to-work, 4-month lag | | | -0.060 | -0.190 | 0.169 | -0.191 |
| | | | (0.196) | (0.191) | (0.236) | (0.193) |
| × Public sector, 4-month lag | | | | | | -0.103 |
| | | | | | | (0.090) |
| × RTW × Public, 4-month lag | | | | | | 0.357 |
| | | | | | | (0.304) |
| N | 12716 | 14520 | 27236 | 22616 | 14168 | 36784 |

*Notes:* Columns (1)–(2) estimate the network-exposure effect separately on right-to-work and non-right-to-work states; column (3) pools them with a PCI × right-to-work interaction (all strikes). Columns (4) and (5) report the same pooled interaction separately for private-sector (nongovernment) and public-sector (government) strikes, using own-sector PCI exposure. Column (6) stacks the two sectors—one observation per county–month–sector—and adds a PCI × right-to-work × public triple interaction, so the difference between the sector-specific right-to-work gradients is estimated directly rather than compared across columns (4) and (5), which are drawn from the same counties. County × sector and month–year × sector fixed effects give each sector the structure it has in its own column, and the right-to-work and public main effects are absorbed. Right-to-work states are the 27 states with right-to-work laws in force during the sample period; the indicator is time-invariant and classifies Michigan, whose repeal took effect in February 2024 (the final eleven sample months), as right-to-work. PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B12: Shapley Decomposition of $R^2$ and Underlying Coefficient Estimates

| | OLS | | LPM ($\times$ 100) | |
|---|---|---|---|---|
| | (1) All | (2) Strike Cos. | (3) All | (4) Strike Cos. |
| *Panel A: Coefficient estimates, full specification* | | | | |
| PCI-Weighted Strikes of Other Counties, Same-Month | 0.038*** | 0.056*** | 1.206*** | 1.560*** |
| | (0.007) | (0.015) | (0.182) | (0.311) |
| Lagged PCI-Weighted Strikes of Other Counties, 4-Month Sum | 0.013 | 0.016 | 0.768*** | 0.780* |
| | (0.009) | (0.018) | (0.264) | (0.437) |
| Lagged Total Strikes in Own County, 2-Month Sum | -0.018*** | -0.021*** | -0.009 | -0.030 |
| | (0.006) | (0.007) | (0.036) | (0.037) |
| Unemployment Rate, 2-Month Lagged Average | -0.005 | -0.041 | 0.046 | -0.006 |
| | (0.006) | (0.041) | (0.064) | (0.466) |
| $\Delta$ Bartik Wage, 2-Month Lag | -0.192 | -3.168 | -0.677 | -10.536 |
| | (0.164) | (2.348) | (1.069) | (12.791) |
| $\Delta$ Bartik Employment, 2-Month Lag | -0.014 | -0.164 | 0.000 | -0.004 |
| | (0.011) | (0.138) | (0.115) | (1.344) |
| County char. $\times$ year interactions | Yes | Yes | Yes | Yes |
| Observations | 135,916 | 27,500 | 135,916 | 27,500 |
| *Panel B: Shapley decomposition (share of full-model R-squared)* | | | | |
| Labor-market controls (own and PCI-weighted) | 0.7% | 2.3% | 1.8% | 2.9% |
| County characteristics $\times$ year | 5.5% | 15.2% | 18.3% | 20.4% |
| PCI-weighted strikes | 3.3% | 1.7% | 10.2% | 8.9% |
| Lagged own strikes | 1.8% | 1.0% | 0.5% | 0.1% |
| County fixed effects | 87.6% | 73.0% | 68.5% | 65.3% |
| Time fixed effects | 1.0% | 6.8% | 0.6% | 2.4% |
| Full-model $R^2$ | 0.040 | 0.031 | 0.310 | 0.256 |

*Notes:* The four columns are OLS (strike count per 1,000 population) and a linear probability model (any-strike indicator multiplied by 100, so coefficients are percentage points), each estimated on all counties and on counties that experience at least one strike over 2021–2024, with county and month–year fixed effects (`reghdfe`); standard errors clustered by county in parentheses. Panel A reports coefficient estimates from the full specification on which the decomposition is based. Panel B reports the Shapley decomposition of the model $R^2$ across six mutually exclusive groups of regressors—labor-market controls (own-county unemployment and Bartik wage/employment shocks, and their PCI-weighted counterparts in socially connected counties), 2020 county-characteristics $\times$ year interactions, PCI-weighted strike exposure, lagged own-county strikes, county fixed effects, and month–year fixed effects—computed from all $2^6$ nested specifications. Group rows report each component's share of the explained $R^2$ (percent, summing to 100 across the six components); the final row reports the full-model $R^2$. Right-hand-side variables enter in standard deviations, except the own-county unemployment rate, which is in percentage points. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## Table B13: Strikes and Deals by Industry: PCI-Weighted Strike and Deal Exposure

| | (1) Edu Strikes | (2) Edu Deals | (3) Health Strikes | (4) Health Deals | (5) Oth-Serv Strikes | (6) Oth-Serv Deals | (7) Goods Strikes | (8) Goods Deals |
|---|---|---|---|---|---|---|---|---|
| PCI-Weighted Deals (Education), Same-Month | **-0.09**$^{**}$ | **-0.05** | | | | | | |
| | **(0.04)** | **(0.06)** | | | | | | |
| Lagged PCI-Weighted Deals (Education), 4-Month Sum | **-0.12** | **-0.12** | | | | | | |
| | **(0.09)** | **(0.08)** | | | | | | |
| PCI-Weighted Strikes (Education), Same-Month | **0.12**$^{***}$ | **0.02** | | | | | | |
| | **(0.04)** | **(0.06)** | | | | | | |
| Lagged PCI-Weighted Strikes (Education), 4-Month Sum | **0.03** | **0.20**$^{**}$ | | | | | | |
| | **(0.13)** | **(0.09)** | | | | | | |
| PCI-Weighted Deals (Healthcare), Same-Month | | | **-0.02** | **0.05** | | | | |
| | | | **(0.05)** | **(0.04)** | | | | |
| Lagged PCI-Weighted Deals (Healthcare), 4-Month Sum | | | **0.02** | **-0.11** | | | | |
| | | | **(0.07)** | **(0.08)** | | | | |
| PCI-Weighted Strikes (Healthcare), Same-Month | | | **0.17**$^{***}$ | **-0.00** | | | | |
| | | | **(0.03)** | **(0.06)** | | | | |
| Lagged PCI-Weighted Strikes (Healthcare), 4-Month Sum | | | **-0.05** | **0.04** | | | | |
| | | | **(0.08)** | **(0.04)** | | | | |
| PCI-Weighted Deals (Other Service-Providing), Same-Month | | | | | **0.03** | **0.11**$^{*}$ | | |
| | | | | | **(0.05)** | **(0.06)** | | |
| Lagged PCI-Weighted Deals (Other Service-Providing), 4-Month Sum | | | | | **0.00** | **0.10** | | |
| | | | | | **(0.06)** | **(0.07)** | | |
| PCI-Weighted Strikes (Other Service-Providing), Same-Month | | | | | **0.10**$^{***}$ | **0.03** | | |
| | | | | | **(0.03)** | **(0.09)** | | |
| Lagged PCI-Weighted Strikes (Other Service-Providing), 4-Month Sum | | | | | **0.11**$^{*}$ | **-0.04** | | |
| | | | | | **(0.06)** | **(0.12)** | | |
| PCI-Weighted Deals (Goods-Producing), Same-Month | | | | | | | **-0.08** | **-0.38**$^{**}$ |
| | | | | | | | **(0.11)** | **(0.18)** |
| Lagged PCI-Weighted Deals (Goods-Producing), 4-Month Sum | | | | | | | **0.12** | **-0.02** |
| | | | | | | | **(0.08)** | **(0.08)** |
| PCI-Weighted Strikes (Goods-Producing), Same-Month | | | | | | | **0.13**$^{**}$ | **-0.05** |
| | | | | | | | **(0.05)** | **(0.09)** |
| Lagged PCI-Weighted Strikes (Goods-Producing), 4-Month Sum | | | | | | | **-0.08** | **-0.15** |
| | | | | | | | **(0.06)** | **(0.10)** |
| N | 10472 | 6380 | 10472 | 5461 | 17072 | 8096 | 9417 | 4480 |

*Notes:* Industry-disaggregated companion to the strikes–deals analysis. A deal is a settlement reached within a year of strike onset, classified from public news coverage and verified (Appendix B); deal counts are restricted to *single-county* settlements – those whose striking-unit footprint lies in one county – assigned to that county and dated by the settlement month; settlements attributed to legislation or policy are excluded. Within each industry, the odd column uses the count of own-industry strikes as the outcome and the even column the count of own-industry deals; both are regressed on that industry's own PCI-weighted strike and deal exposure. All specifications control for own-county lagged strikes and deals (2-month sums). Own-industry exposure coefficients (the on-diagonal blocks) are shown in **bold**. PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B14: Cross-Industry PCI-Weighted Strike Exposure by Right-to-Work Status

| | (1)<br>Edu RTW | (2)<br>Health RTW | (3)<br>OthServ RTW | (4)<br>Goods RTW | (5)<br>Edu Non-RTW | (6)<br>Health Non-RTW | (7)<br>OthServ Non-RTW | (8)<br>Goods Non-RTW |
|---|---|---|---|---|---|---|---|---|
| PCI-Weighted Education Strikes, Same-Month | **0.08** | 0.14 | 0.06 | -0.01 | **0.09**$^{***}$ | 0.03 | -0.00 | 0.12 |
| | **(0.27)** | (0.21) | (0.07) | (0.24) | **(0.03)** | (0.04) | (0.02) | (0.09) |
| Lagged PCI-Weighted Education Strikes, 4-Month Sum | **0.35** | 0.21 | 0.22 | -0.09 | **-0.13** | 0.14$^{*}$ | -0.04 | 0.15 |
| | **(0.30)** | (0.33) | (0.16) | (0.23) | **(0.08)** | (0.08) | (0.07) | (0.11) |
| PCI-Weighted Healthcare Strikes, Same-Month | 0.20 | **0.27**$^{**}$ | 0.11 | -0.14 | -0.06 | **0.18**$^{***}$ | -0.06$^{*}$ | -0.09 |
| | (0.14) | **(0.13)** | (0.13) | (0.18) | (0.07) | **(0.04)** | (0.03) | (0.11) |
| Lagged PCI-Weighted Healthcare Strikes, 4-Month Sum | 0.64 | **0.37** | -0.01 | -0.36 | -0.09 | **-0.09** | 0.02 | -0.11 |
| | (0.43) | **(0.34)** | (0.16) | (0.40) | (0.07) | **(0.09)** | (0.03) | (0.09) |
| PCI-Weighted Other Service-Providing Strikes, Same-Month | 0.44 | 0.03 | **0.26**$^{**}$ | 0.33 | 0.06 | -0.09 | **0.04** | -0.07 |
| | (0.31) | (0.27) | **(0.11)** | (0.25) | (0.06) | (0.07) | **(0.04)** | (0.08) |
| Lagged PCI-Weighted Other Service-Providing Strikes, 4-Month Sum | 0.12 | -1.10$^{*}$ | **0.19** | 0.04 | 0.15 | -0.00 | **0.06** | 0.19 |
| | (0.48) | (0.60) | **(0.21)** | (0.43) | (0.10) | (0.07) | **(0.05)** | (0.15) |
| PCI-Weighted Goods-Producing Strikes, Same-Month | -0.41$^{**}$ | -0.05 | 0.13$^{*}$ | **0.01** | -0.01 | 0.03 | -0.04 | **0.20**$^{***}$ |
| | (0.18) | (0.15) | (0.06) | **(0.06)** | (0.06) | (0.04) | (0.04) | **(0.06)** |
| Lagged PCI-Weighted Goods-Producing Strikes, 4-Month Sum | -0.46 | 0.22 | -0.16 | **0.03** | 0.01 | 0.07 | -0.05 | **0.02** |
| | (0.31) | (0.27) | (0.11) | **(0.10)** | (0.08) | (0.06) | (0.06) | **(0.08)** |
| N | 2975 | 2432 | 8052 | 3478 | 6732 | 7656 | 9020 | 5375 |

*Notes:* Cross-industry extension of the right-to-work analysis. Each column regresses same-sector strike incidence (Education, Healthcare, Other Service-Providing, Goods-Producing) on PCI-weighted strike exposure originating from all four sectors, both same-month and as a four-month lagged sum. Columns (1)–(4) restrict the sample to RTW states; columns (5)–(8) to non-RTW states. Right-to-work states are the 27 states with right-to-work laws in force during the sample period; the indicator is time-invariant and classifies Michigan, whose repeal took effect in February 2024 (the final eleven sample months), as right-to-work. Own-sector (on-diagonal) exposure coefficients are shown in **bold**. PPML (`ppmlhdfe`) with county and month–year fixed effects; standard errors clustered by county in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B15: Worker-Side Announcements of Strike Settlements: Coverage, Sentiment, and Framing

| | Count | Share |
|---|---|---|
| *Panel A: Coverage (all confirmed positive outcomes)* | | |
| Confirmed positive outcomes (deals) | 1,899 | — |
| With a worker/union announcement | 1,763 | 92.8% |
| No announcement located (gap) | 136 | 7.2% |
| *Panel B: Sentiment of the announcement (n=1,763)* | | |
| Very satisfied | 862 | 48.9% |
| Somewhat satisfied | 628 | 35.6% |
| Mixed | 128 | 7.3% |
| Dissatisfied | 38 | 2.2% |
| Not stated | 107 | 6.1% |
| *Panel C: Framing of the outcome (n=1,763)* | | |
| Clear win | 1,163 | 66.0% |
| Partial win | 403 | 22.9% |
| Compromise | 87 | 4.9% |
| Neutral | 103 | 5.8% |
| Defeat / sellout | 7 | 0.4% |

*Notes:* The universe is the 1,899 confirmed positive strike outcomes (settlements/"deals") over 2021–2024. For each, we searched the saved source material for a *protester-side* announcement or reaction—the union, a bargaining-team member, a union officer or spokesperson, or a rank-and-file worker speaking about the agreement—and, where present, classified its expressed *sentiment* toward the terms and its *framing* of the result. A worker-side announcement was located for 1,763 of the 1,899 outcomes (92.8%); the shares in Panels B and C are of those 1,763 located announcements. Sentiment and framing were coded by LLM adjudication of the saved source text (raw sources and coding materials are archived with the replication data).

Table B16: Example Worker-Side Announcements Coded as "Clear Win" and "Very Satisfied"

| Strike (employer; union) | Speaker | Location; start date | Announcement quote |
|---|---|---|---|
| MGM Resorts; Caesars Entertainment; *UNITE HERE* | Bob McDevitt, President, UNITE HERE Local 54 | Atlantic City, New Jersey<br>Jun 1, 2022 | "This is the best contract we've ever had. We got everything we wanted and everything we needed. The workers delivered a contract that they can be proud of for years to come."<br>*Terms:* wages (housekeeping $18/hr immediately, rising to $22 over contract); 4-year deal; ~6,000 workers<br>*Source:* https://www.casino.org/news/atlantic-city-casino-union-claims-victory-as-new-contracts-reached/ |
| United States Maritime Alliance (USMX); *International Longshoreman's Association (ILA)* | ILA President Harold Daggett | South Boston, Massachusetts (multi-site)<br>Oct 1, 2024 | "The ILA stayed strong and unified throughout and successfully won the greatest contract in ILA history and maybe the strongest Collective Bargaining Agreement ever negotiated by any union."<br>*Terms:* 62% wage increase, automation protections, container royalties, MILA health plan<br>*Source:* https://ilaunion.org/rank-and-file-members-of-international-longshoremens-association-at-atlantic-and-gulf-coast-ports-overwhelmingly-ratify-provisions-of-new-six-year-master-contract/ |
| United Parcel Service (UPS); *Teamsters (IBT)* | international union president (Teamsters) | Brooklyn, New York<br>Jul 28, 2022 | "We demanded the best contract in the history of UPS, and we got it"<br>*Terms:* Historic 5-yr TA: +$2.75/hr in 2023 (+$7.50 over contract), PT minimum $21/hr, end 22.4 two-tier, A/C in vehicles, MLK Day holiday, 7,500 new FT jobs, zero concessions<br>*Source:* https://teamster.org/2023/07/weve-changed-the-game-teamsters-win-historic-ups-contract/ |
| The Washington Post; *The NewsGuild - CWA* | union local | Washington, District of Columbia<br>Dec 7, 2023 | "This deal is the best contract Washington Post employees have seen in 50 years."<br>*Terms:* doubles salary floors for lowest-paid; raises across the board; essential rights<br>*Source:* https://cwa-union.org/news/bargaining-update-268 |
| Hunts Point Terminal Market; *Teamsters (IBT)* | union president (Teamsters Local 202) | Bronx, New York<br>Jan 17, 2021 | "It's the largest deal we've ever signed."<br>*Terms:* min 70 cents/hr raise yr1 rising to $1.85 by yr3; no added health-care contribution<br>*Source:* https://jacobin.com/2021/01/hunts-point-workers-strike-victory-teamsters-local-202 |

*continued on next page*

*Table B16 (continued)*

| Strike (employer; union) | Speaker | Location; start date | Announcement quote |
|---|---|---|---|
| National Aerospace Solutions; *Air Engineering Metal Trades Council (AEMTC)* | Jimmy Hart, President, Metal Trades Department AFL-CIO (AEMTC parent body) | Tullahoma, Tennessee<br>Jul 2, 2021 | "The new contract, ratified by a three-to-one majority of the council members on Friday, July 16, represents the best-ever collective bargaining agreement on a prime base contract at Arnold Air Force Base."<br>*Terms:* wages, healthcare cost-sharing, long-term disability insurance, 3-year contract ratified 3-to-1<br>*Source:* https://unionlabel.org/2021/08/09/two-week-ulp-strike-at-arnold-air-force-base-brings-best-primary-base-contract-in-70-year-history/ |
| County of Benton; *American Federation of State, County and Municipal Employees (AFSCME)* | union local president (Dawn Dale, AFSCME Local 2064) | Corvallis, Oregon<br>Sep 1, 2021 | "In the end, we won the best contract that even our long-time members had ever seen."<br>*Terms:* Two added salary steps retroactive; market adjustments; highest COLAs ever (3.5% retro, 3% 2022, 3.25% 2023); bilingual pay; life insurance & retirement health savings<br>*Source:* https://oregonafscme.org/news/bargaining-during-covid-how-benton-county-local-2046-found-success |
| Los Angeles County; *Service Employees International Union (SEIU)* | union (SEIU 721) | Los Angeles, California<br>Mar 31, 2022 | "securing the biggest one-year raise in LA County history and the largest one-year salary increases since 2006"<br>*Terms:* 12% raise over 3 yrs (5.5%+$1,375 bonus 2022, 3.25% 2023, 3.25% 2024), medical/childcare subsidies<br>*Source:* https://www.seiu721.org/2022/07/la-county-members-ratify-new-3-year-contract-with-a-12-raise-and-more.php |
| Illinois Veterans Home; *American Federation of State, County and Municipal Employees (AFSCME)* | INA president (Tori Dameron) | Quincy, Illinois<br>Jun 27, 2023 | "to win the biggest wage increase that state nurses have ever seen"<br>*Terms:* 22.95% raises over 4 yrs, $2,200 bonuses, 12 wks parental leave, remote work<br>*Source:* https://www.ibjonline.com/2023/10/30/illinois-nurses-association-state-of-illinois-agree-on-new-four-year-contract/ |
| Lunds and Byerlys; Kowalski's; Cub Foods; *United Food and Commercial Workers Union (UFCW)* | rank-and-file/bargaining committee member | Minneapolis, Minnesota<br>Mar 25, 2023 | "This contract is truly historic. In the 35 years I've been in the retail grocery industry, I've never seen such a rich contract."<br>*Terms:* historic wage increases FT & PT; union health plan maintained at no added cost; labor-mgmt committees<br>*Source:* https://www.ufcw.org/actions/victories/grocery-workers-in-minnesota-stand-together-for-a-better-contract-2/ |

*Notes:* Ten examples of announcements coded *very satisfied* and *clear win*, drawn from the set summarized in Table B15. Each row lists the struck employer(s) and union, the speaker credited in the source, the strike's recorded location and start date, and a verbatim quote from the located worker-side announcement, with the settlement terms it cites and a source URL containing the quoted text (links verified July 2026). Quotes are reproduced as coded from the saved source text.