Organizing Collective Action: Labor Strife in the U.S. in the 1880s

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This paper evaluates an explanation for the unionization of labor conflict: unions help workers win strikes. I test this hypothesis with data from the U.S. in the early 1880s, when unorganized workers were still responsible for two fifths of all strike activity. Because organized workers might attempt riskier confrontations than the unorganized, I construct an instrument for the involvement of a union in a strike from the location of the assemblies of the Knights of Labor. I estimate that unions raised strikers' success rate by 32 percentage points from a baseline of 38 percent; moreover, they decreased the incidence of job loss by 22 percentage points from a baseline of 56 percent. Although unions increased the probability that employers acceded to strikers' demands, I find no evidence of an impact on the size of those concessions.

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1. Introduction

Look, my comrades, see the union banners waving high:
Reinforcements now appearing, victory is nigh.

— Extract of "Hold the Fort"[1]

Industrial action underlies the bargaining power of labor unions. Without the credible threat of a work stoppage, employers have no reason to recognize unions or offer them concessions. Yet unorganized workers are also capable of collective action: for example, they undertook 38 percent of strikes in the United States in 1900, 42 percent in Austria-Hungary and 61 percent in Germany (U.S. Bureau of Labor, 1906).[2] Why do workers unionize then? How does organization improve on wildcat picketing?

This paper explores one explanation: organization helps workers win strikes. I hypothesize that unions facilitate coordination, helping members deploy a wider tactical inventory. First, they buttress the picket line by raising defense funds, fostering solidary, etc. Second, they weaken employers by calling boycotts, increasing turnout, etc. Third, they negotiate better settlements on the strength of their bargaining experience and reputation. Fourth, they facilitate the exchange of information and decision making through conventions, journals, etc.

It is difficult to test this hypothesis in modern labor markets. Figure 1 shows organized strikes as a percentage of strike activity in the U.S. from 1881 to 1957. As the labor movement matured, wildcat stoppages dwindled: unions participated in 92 percent of walkouts by the time of the National Labor Relations Act of 1935, accounting for 98 percent of idled man-days.[3] As a consequence, there is not enough variation in postwar data to identify the

[1] Published in *Labor Songs Dedicated to the Knights of Labor* (Chicago, IL: J. D. Tallmadge, 1886).

[2] The strike of the freight handlers in Chicago in April 1881 is illustrative of wildcat walkouts. Under an informal leadership, workers discussed their plans at lunch and after work. They circulated a petition for a wage raise for several days, which they presented to the railroad companies. After the employers denied their request, workers struck the Illinois Central Railroad. Turnout was mixed elsewhere. Most companies promised to match concessions by the Illinois Central Railroad and other lines if employees did not quit work. Strikebreakers were hired, but they were inexperienced and suffered intimidation. Although newspapers repeatedly announced the imminent defeat of the strikers, the railroads offered an unconditional concession after five days of negotiations. (This account is based on daily reports in the *Chicago Tribune* and the *Inter Ocean*.)

[3] Wildcat strikes decreased first in relative and later in absolute terms (Peterson, 1938). There were fluctuations. For example, many unions disbanded in recessions, increasing the proportion of unorganized strikes. For context, Friedman (1999) puts the unionization rate among American industrial workers at 3.75 percent in 1880, 9.68 percent in 1890, 6.35 percent in 1899 and 16 percent in 1909. Comparable series for other countries are scarce. According to second-hand data from the U.S. Bureau of Labor (1906), the percentage of organized strikes increased from 27 in 1894 to 55 in 1905 in Austria-Hungary and from 58 in 1899 to 75 in 1905 in Germany.

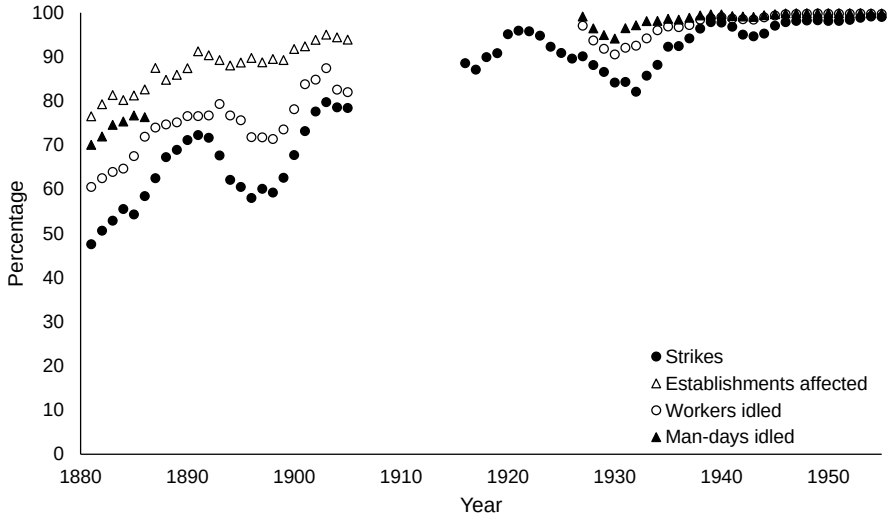

Figure 1: Organized strikes as a percentage of strike activity in the U.S. by measure

Notes: The figure shows three-year moving averages. For years 1881 to 1905, it shows strikes ordered by labor organizations as a percentage of all strikes. For years 1916 to 1957, it shows stoppages involving unionized workers as a percentage of strikes and lockouts. No data were collected between 1906 and 1915.
Sources: 1881 to 1905: U.S. Bureau of Labor (1888, 1906). 1916 to 1957: various editions of the *Monthly Labor Review*, by the U.S. Bureau of Labor Statistics.

impact of unionization on industrial conflict.

Historical data overcome this deficiency. I borrow rich microdata from the earliest nationwide sample of work stoppages in the U.S., the *Third Annual Report of the Commissioner of Labor* (U.S. Bureau of Labor, 1888). My analysis encompasses 2172 unorganized and 3191 organized strikes, ranging from 1881 to 1886. Like Card and Olson (1995) and Friedman (1988), I focus on the impact of organization on the success rate – i.e. the probability that strikers extract concessions from management.

Endogeneity poses a second challenge. The probability of success of a strike comprises a baseline rate and an organization effect (if applicable). Striking is a strategic decision (Hayes, 1984): workers walk out when victory seems likely enough. For instance, they might feel that an employer is vulnerable on account of perishable inventory or outstanding orders. To the extent that the organization effect offsets worse baseline odds, unionized workers might undertake riskier strikes than the unorganized and confront stronger employers. In other words, bargaining strategies are endogenous. Therefore, organized strikes might exhibit a lower baseline success rate on average than the wildcat, which would downward bias estimates of the organization effect.

I exploit an instrumental variable for identification: the existence of a local assembly

of the Knights of Labor (KOL) in the locality of the dispute.[4] The KOL constituted the foremost labor society of the 1880s (Friedman, 1988; Voss, 1993), peaking at a fifth of the industrial workforce in 1886. Their presence indicates that the local workforce had unionized and that union officers operated in the community. Hence, it should correlate with union involvement in strikes. I justify the exclusion restriction on four accounts: (1) assembly creation depended on recruitment opportunities and the proximity to existing offices rather than strike prospects; (2) the instrument incorporates a lag between observations and changes in the location of the Knights, so it is orthogonal to the dynamics of conflict outcomes; (3) my results are robust to balance adjustments, sample restrictions and variations in the instrument; and (4) the instrument passes the test of the exogeneity condition of Machado, Shaikh and Vytlacil (2018).[5]

I estimate that union sponsorship increased the probability of success by 31 percentage points from a baseline rate of 40 percent, rationalizing the preponderance of organized strikes in modern industrial relations. Ordinary regression yields a lower estimate, 12 percentage points, which suggests that unionized workers responded to the higher success rate by undertaking riskier walkouts. The difference is statistically significant. Furthermore, unions reduced the probability of job loss by 22 percentage points from a baseline rate of 56 percent. On the other hand, there is no evidence that successful organized workers achieved larger wage gains or hour cuts than successful wildcat strikers. I find no change in the duration of standoffs either.

This paper pertains to three literatures. First, it furthers the scholarship on labor unions. Economists have long debated their impact on the wage structure and firm performance, from Freeman and Medoff (1984) to recent work by Card, Lemieux and Riddel (2004), Collins and Niemesh (2018), DiNardo and Lee (2004), Farber et al. (2018) and Lee and Mas (2012). I investigate an explanation for the differential bargaining power of organized labor, which underlies their estimates. Second, I add to the literature about strikes. Gramm and Schnell (1994) show that union officers affect the chances of strikebreaking, which supports my hypothesis. Card and Olson (1995) and Geraghty and Wiseman (2008) estimate attrition models from a subset of my sample. Their framework is useful in interpreting my parameters. Other empirical research has mostly focused on the interplay between work stoppages, wage outcomes and market conditions.[6] Third, this paper contributes to

[4] See Enflo and Karlsson (2018) for a related identification strategy.

[5] This test is based on the intuition that the correlation between the instrument and outcomes should be neither too small nor too large if the instrument only affects outcomes through the treatment. See Subsection 6.2 for further discussion.

[6] For surveys, see Card (1990), Cramton and Tracy (2003), Kennan (1986) and Kennan and Wilson (1989).

the historiography of the American labor movement in the 1880s. This decade saw an unprecedented experiment in radical mass unionism by the KOL, culminating in the strike wave of May 1886. Their subsequent collapse entrenched conservative craft unionism in the U.S. Historians have partly blamed this reversal on a string of defeats of the KOL (Friedman, 1988; Kaufman, 2001; Perlman, 1918; Voss, 1993). This paper nuances this view: I argue that the Knights were successful strike leaders, but they could not impose discipline on the rank and file. Kremer and Olken (2009) propose a similar explanation in the context of an evolutionary model of unionization. I complement recent research on environmental constraints on labor activism, such as the government (Currie and Ferrie, 2000; Friedman, 1988), employers (Schmick, 2018; Voss, 1993), market integration (Ansell and Joseph, 1998) and rival associations (Kaufman, 2001).

The paper continues as follows. Section 2 summarizes the historical background. Section 3 describes the data. Section 4 discusses identification. Section 5 explains the econometric strategy. Sections 6 and 7 present the results. Section 9 concludes.

2. Historical background

2.1. Origins of the American labor movement

Labor unrest was sporadic in the United States in the early decades after independence (Saposs, 1918).[7] The first known wildcat strike implicated journeymen printers in New York in 1776. Cordwainers pioneered the organized strike in Philadelphia in the 1790s. Printers and shoemakers went on to establish associations across the northeast, but other trades did not organize until the late 1810s. Evidence of incipient working-class consciousness dates to 1827 (Saposs, 1918; Sumner, 1918), when trade societies agitated for the ten-hour day in Philadelphia. The campaign led to the creation of a citywide federation of labor unions and a labor party. Solidarity crossed occupational lines as workers learned to articulate their common grievances.

Early unions restricted membership to skilled craftsmen. As workshops gave way to factories, artisans blamed mechanization and the division of labor for a perceived loss in autonomy, competency and status (Katz and Margo, 2014; Voss, 1993). Organization was their response. They yearned for a republic of independent producers (Hallgrímsdóttir and Benoit, 2007; Voss, 1993): claiming moral superiority over the "subordinate laborer" as well

[7] There were work stoppages in colonial times. For example, bakers struck in New York in 1741. However, Saposs (1918) argues that these disputes pitted master artisans against local authorities, rather than employees against employers.

as the "idle classes", craftsmen maintained that "wage slavery" was incompatible with a free citizenry. Unions translated ideology into collective action: party politics, collective bargaining, worker cooperatives, mutual insurance, industrial education, etc.

The labor movement foundered as a recession took hold in the 1830s (Mittelman, 1918). It would slowly rebuild and evolve. The earliest national trade associations emerged in the 1850s, as rail links and the telegraph stimulated market integration (Andrews, 1918; Ansell and Joseph, 1998). They grew in importance after the Civil War, overshadowing local unions. Industrialization had so realigned interests by the late 1860s that labor leaders took the first steps to organize the "subordinate laborer" (i.e. blacks, women and the unskilled). In one such effort, Uriah S. Stephens founded the Noble and Holy Order of the Knights of Labor in Philadelphia in 1869.

2.2. The Knights of Labor

Stephens blamed the degradation of labor on internal divisions (Grob, 1958; Wright, 1887). Capital tended toward concentration, through which it accumulated bargaining power and political influence, whereas workers fragmented into uncoordinated trade unions. Stephens saw strength in numbers: if wage earners coalesced into a single organization, they would have the clout to defend their common interests against combined capital. He envisioned a more integrated labor movement as well as a wider constituency.

The KOL combined traits of labor unions, fraternal brotherhoods and political parties (Birdsall, 1953; Kaufman, 2001). They advocated incremental progress toward the abolition of the wage system (Grob, 1958; Wright, 1887). Their agenda included such intermediate goals as the eight-hour week, equal pay for equal work, a ban on child labor, graduated income taxation, antitrust law, public ownership of utilities and the creation of labor bureaus. As Stephens proposed, the order sought to mobilize a critical mass toward socioeconomic reform. In consequence, it adopted a distinctively inclusive admission policy. It recruited unskilled laborers as well as craftsmen, irrespective of nationality, religion or occupation. It would extend affiliation to blacks in 1878 and women in 1882.[8]

The KOL assumed a dual role (Birdsall, 1953; Wright, 1887). As educators, they commended gradualism and nurtured solidarity. As coordinators, they encouraged collective action in three forms. First, political activism would win reform at the ballot box.[9] Second, worker cooperatives would offer an alternative to wage employment. Third, organization

[8] There were limits to its inclusiveness: e.g., the Knights rejected politicians, liquor distributors, lawyers and the Chinese.

[9] The Knights did not support a specific party. They occasionally endorsed candidates and members could run for office, though they did not affiliate professional politicians.

would help workers bargain for better work conditions. On the other hand, they were ambivalent about industrial conflict. National officers warned that strikes should be a last resort, recommending arbitration instead, but local cadres had much freedom to interpret those guidelines and the ranks were keen to strike. (For further discussion, see Section 7.)

At their first constitutional convention in 1878, the Knights structured their government in three tiers on a territorial basis (Birdsall, 1953). The local assembly (LA) was the basic unit of organization. Its size ranged from a minimum of ten members to the thousands (Garlock, 1982). Each defined its own jurisdiction, which could span from a single establishment to a large city. Some restricted admission further: for instance, LA 5327 recruited wood workers in East Boston and LA 8072 affiliated Germans in Holyoke. The second level was the district assembly, though local assemblies did not necessarily belong to one. Supreme authority lied with the General Assembly.[10] However, the national executive board wielded little power over lower assemblies in practice. They played two main roles: settling disputes between districts and discouraging strikes from the bully pulpit. As a federation of autonomous assemblies of varying scopes, the order sacrificed a coherent national strategy for the flexibility to accommodate its diverse membership (Birdsall, 1953; Grob, 1958; Voss, 1993).

The KOL faced headwinds in their early years (Kaufman, 2001; Wright, 1887), including a downturn in the 1870s and opposition from the Catholic Church. These challenges prompted change. Stephens ceded the headship to Terence V. Powderly in 1879, clearing the way for the elimination of secret oaths in stages by 1882.[11] In an additional effort to boost recruitment, the General Assembly created the organizer in 1878. Organizers received a paid commission to found new assemblies. Because existing locals could be jealous of their constituencies and districts had a right of oversight within their jurisdiction (Birdsall, 1953; Voss, 1993), organizers had an incentive to operate in unclaimed territory, which stimulated the geographic expansion of the KOL.

The order was the third national labor federation in the United States.[12] While it had much in common with its predecessors (e.g., the emphasis on political action and independent producers), it innovated in embracing all wage earners. Europe underwent a similar transition from craft movements to radical mass unionism at the end of the 19th century

[10] This hierarchy became more complex with the advent of state assemblies in 1883 and national trade assemblies in 1884. See Birdsall (1953) and Grob (1958).

[11] The KOL kept strict secrecy until 1879. They were partly motivated by the fear of retaliation and partly influenced by a fascination with the freemasonry. However, the Catholic Church was no friend of secret societies, which proved problematic. See Wright (1887) and Kaufman (2001).

[12] Its predecessors were the National Trades' Union (1834–37) and the National Labor Union (1866–73). The International Workingmen's Association (1864–72) maintained local branches in the United States too.

(Friedman, 1988; Voss, 1993). Like the KOL, a moderate strand espoused gradualistic politics, as the British Fabians exemplify. Others preached revolution, such as the French Confédération Générale du Travail.

2.3. Labor at a crossroads

The Panic of 1873 triggered a severe recession. Labor activism withered amid high unemployment and pay cuts, but deteriorating work conditions sowed the seed of recovery (Voss, 1993). Workers' discontent found dramatic expression in the Great Railroad Strike of 1877 (Lloyd, 2009), when wildcat protests spread nationwide after the B&O Railroad reduced wages for a third time in six months. The working class demonstrated mobilization potential and latent solidarity, which unions set out to cultivate.

Friedman (1999) estimates that total union membership rose from 168,000 in 1880 to 500,000 in 1885. The unionization rate reached 4.6 percent.[13] The Knights grew from nine thousand in 1878 to a hundred thousand in 1885 (Perlman, 1918). This expansion owed much to the subsumption of independent unions and the reorganization of extinct ones. It involved considerable turnover: for instance, 18,104 workers joined the KOL in 1880, but 10,056 quit. Labor strife affected an yearly average of 2630 establishments and 176,513 workers between 1881 and 1885 (U.S. Bureau of Labor, 1888).

In October 1884, the Federation of Organized Trades and Labor Unions (FOTLU) announced that the eight-hour day should become standard by May 1, 1886. The plan energized the labor movement (Kemmerer and Wickersham, 1950; Perlman, 1918). Further momentum built after impressionistic press reports about the KOL and successful stoppages of the railroads. As a strike wave loomed, union membership attained 1.2 million in the spring of 1886 (Friedman, 1999), including one in five industrial workers. The Knights increased sevenfold between new recruits and readmissions (Perlman, 1918), surpassing 700,000 members and becoming the largest labor organization in the United States. More than 300,000 protesters marched on May Day. Pickets continued into the following weeks, but the campaign lost impetus after the Haymarket Affair (the fatal bombing of Chicago police on May 4). Notwithstanding concessions from individual employers, activists yielded without achieving the statutory eight-hour day as the violence caused a backlash in public opinion against the strikers.[14]

May 1886 transformed organized labor. The Knights entered rapid decline (Oestreicher, 1984), dwindling to twenty thousand by 1900. In December 1886, Samuel Gompers

[13] Friedman (1999) speculates that these figures understate union membership and overstate growth.
[14] See Biggs (2002) for a study of multistage bargaining in the context of May 1886.

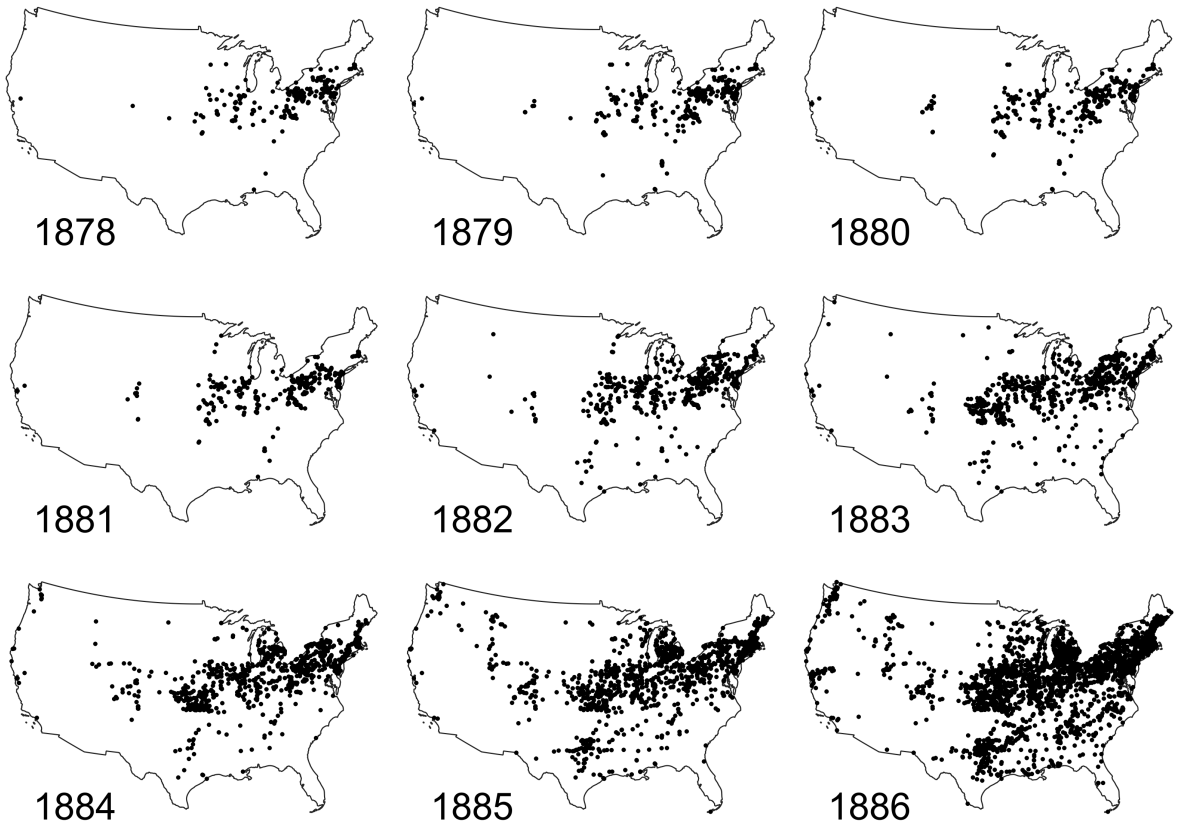

Figure 2: Local assemblies of the Knights of Labor by year (select years)

Source: Garlock (1982, 2009).

forged the American Federation of Labor (AFL) from the FOTLU. The AFL was a league of conservative craft unions (Friedman, 1988; Grob, 1960), which eschewed social reform to focus on workplace issues. It would soon dominate the American labor movement, whereas the unskilled remained mostly unorganized until the 1930s. By contrast, radical industrial unions recovered from early setbacks in Europe. This divergence is a topic of ongoing debate. Recent research suggests that environmental constrains limited the effectiveness of mass unionism in the U.S. (Ansell and Joseph, 1998; Friedman, 1988; Kaufman, 2001; Voss, 1993). This paper nuances this view: the Knights and other unions helped workers win strikes in fact, yet they failed to impose discipline on the rank and file. As Kremer and Olken (2009) argue, weak leadership may have imperiled their survival on the long run. For further discussion, see Sections 7 and 9.

3. Data

This section describes the data sources. For further detail about the construction of the sample, see Appendix A. Table 1 shows summary statistics.

3.1. Strikes and lockouts

I use strike data from the *Third Annual Report of the Commissioner of Labor* (U.S. Bureau of Labor, 1888).[15] The report was the second nationwide survey of work stoppages in the United States,[16] following the postal inquiry for the *Tenth Census* by Weeks (1886), and covers the period from 1881 to 1886.[17] Agents collected data in two stages. They first compiled a list of strikes and lockouts from newspapers, trade journals and other sources. They then visited each locality on the list to interview managers, workers and union officers. These inquiries were not limited to the episodes in the initial list. The only exclusion criterion was a minimum duration of one day. This investigation yielded detailed information about each dispute, including: localities, industry, dates, causes and outcome; involvement of unions or employers' associations; affected workers, their occupation and average wages; affected establishments, their size, average wages and weekly hours; and

[15] This project exploits the full sample. The data incorporate the subset of Card and Olson (1995), Currie and Ferrie (2000) and Geraghty and Wiseman (2008). Friedman (1988) and Rosenbloom (1998) used a different subsample.

[16] Statewide surveys took place in Ohio (1878), Massachusetts (1879) and Pennsylvania (1882). Massachusetts' commissioner was Carroll D. Wright at the time, who became the federal commissioner in 1885.

[17] The *Tenth Report* (U.S. Bureau of Labor, 1896) covers the period between 1886 and 1894. I do not use this sample because the unit of observation changes and no wage information is available. Later reports contain only aggregate data, as does Weeks (1886). Their original schedules could not be located.

establishment closures, idled hands and financial losses.[18] The report gives employment, wages and hours before and after the conflict as well as by gender.

The final sample consists of 5363 observations. I exclude lockouts (358 rows), incomplete strikes (4 rows), general strikes (48 rows) and strikes in imprecise localities (36 rows). To account for price differences across time and regions, I adjust wages by the monthly price index of Warren and Pearson (1932) and the state price index of Haines (1989). I impute hours for 23 observations with irregular workweeks. I chose 60 hours, the mode in both the *Third Report* and the Census of Manufactures (Atack and Bateman, 1992).

My primary outcome is an indicator of success. In accordance with contemporaneous practice (Card and Olson, 1995), the report classifies walkouts as successes (43 percent), compromises (9 percent) or failures (49 percent). Since workers' initial demand is a strategic variable, I combine compromises and successes, following Friedman (1988). Hence, success consists in extracting concessions from employers for my purposes. For future reference, successful strikes constitute 51.5 percent of the sample. My treatment is union sponsorship. In the words of the *Third Report*, treated units were "ordered by a labor organization". It is unclear whether the Bureau abided by this restrictive label. It may also have flagged unauthorized stoppages by unionized workers, especially when the union aided its members in some form.[19] For future reference, organized strikes constitute 59.5 percent of the sample.

The *Third Report* has two shortcomings. First, the unit of observation is inconsistent. Because general strikes can often be difficult to delimit (Peterson, 1938), the Bureau of Labor planned separate entries for each affected establishment. This design proved overly ambitious. Therefore, the Bureau aggregated related stoppages instead, provided that they coincided in industry, dates and resolution. A row may thus represent either a strike or part of one. Following the literature,[20] I treat each line as an observation, which has the advantage of underweighting outliers, but my results are robust to the choice of weighting scheme.[21] I account for the correlation between rows in inference (see Subsection 5.2). Second, Bailey (1991) shows that the Bureau undercounted stoppages: local newspapers record twice as many disputes in Terre Haute. He could not identify systematic differences between included and excluded stoppages.[22] As Card and Olson (1995) argue, missing data

[18] The report gives the number of new employees at affected establishments after the conflict. This series does not include temporary replacement workers. Currie and Ferrie (2000) and Rosenbloom (1998) use it as a proxy for the number of strikebreakers, though it is a lower bound.

[19] The line between authorized and unauthorized strikes had become so unclear by the 1920s that the Bureau preferred to record the mere involvement of unionized workers instead (Peterson, 1938). This distinction might have been ambiguous in the 1880s as well.

[20] Contemporaneous authors weighted rows by establishments or employees (U.S. Bureau of Labor, 1888).

[21] Table 11 in Appendix C explores the sensitivity of my main estimate to weighting by establishments.

[22] I attempted to replicate the exercise for three cities: Chicago, Decatur and Milwaukee. I focused on the first

do not jeopardize statistical analysis so long as they are random.

3.2. Assemblies of the Knights of Labor

Garlock (1982, 2009) gathered information on the local assemblies of the Knights of Labor from primary sources. Two official publications cover my sample period: the *Journal of United Labor* (from 1880 to 1885) and the proceedings of the General Assembly (from 1879 to 1885). Therefore, his list should be exhaustive or nearly so. The data include location, years of operation and fragmentary membership statistics.[23]

My instrument is the existence of an assembly in the locality of the strike in the year before the strike. I use a lag for two reasons. First, it ensures that locals were not chartered post factum, given that I do not observe exact organization dates. Second, it allays concerns about endogenous entry decisions in anticipation of a standoff. Section 4 discusses identification in greater detail. To test the exogeneity condition, Section 8 considers two alternative instruments: the existence of an assembly in the locality of the strike in 1880 and the log distance to the nearest assembly outside the locality.

It is sometimes unclear whether all sources distinguished between two localities. For example, the *Third Report* contains both Knoxville and Knoxville Junction (IA), whereas Garlock (1982, 2009) could only find mention of Knoxville in the files of the KOL. I treat each pair in question as one place. When an assembly or a strike spanned multiple localities (23 and 103 observations, respectively), I base the instrument on the first entry. If the locality of the strike is a county (20 observations), I consider whether a local existed anywhere within the county. My estimates are robust to these choices.

3.3. Market conditions

To account for heterogeneity across local labor markets, I draw aggregate statistics from the *Tenth Decennial Census* of 1880 and other sources. These variables are all measured at the county level.

I construct demographic statistics from the full-count microdata of the population census (Ruggles et al., 2018). I restrict the sample to industrial workers. This subset is more

six months of 1881. I believe that omissions are most likely in this early period, since the data collection took place between 1886 and 1887. I found six additional strikes in Chicago (against 46 in the report), none in Decatur (vs. 3) and three in Milwaukee (vs. 3). Hence, it appears that 50 percent is an upper bound on the omission rate.

[23] Locals ought to submit quarterly membership figures, which appear in abridged form in the annual proceedings of the General Assembly, but this duty was often neglected. According to Kaufman (2001), water leaks destroyed the original forms.

representative of potential strikers, since labor activism was marginal in agriculture, trade and services.[24] My control set includes: industrial workers as a percentage of the total labor force; urban workers as a percentage of the industrial workforce; and gender, ethnic and trade fragmentation indexes. I construct average firm sizes and average daily wages from the tables of the census of manufactures (Haines and ICPRS, 2010).[25] I adjust wages by the state price index of Haines (1989). These covariates help me address the correlation between unionization, industrialization and urbanization. Note that they do not vary over time. Moreover, I borrow data from Atack (2016) to compute the ratio of railways to land area. Transportation links facilitated access to replacement workers, which could influence labor strife. Finally, I construct an indicator of past labor strife from the *Third Report*, viz. the occurrence of a successful stoppage in the previous year in the pertinent sector and county. This variable is not available for 1881, as the report does not include data for 1880. My results are robust to alternative specifications, e.g., considering all strikes rather than successful strikes or using data for 1880 at the state level from Weeks (1886). This indicator proxies for unobserved determinants of strike incidence and conflict outcomes.

4. Identification

To estimate the causal impact of union intervention on conflict outcomes, one must account for endogeneity in bargaining strategies. This section presents my identification strategy. To simplify the exposition, I focus on the success rate and ignore the nature of the payoff in dispute.

Striking is a strategic decision (Hayes, 1984; Kennan, 1986): workers walk out if victory seems likely enough. The probability of success is the sum of a baseline rate and an organization effect. The baseline rate reflects the circumstances of each confrontation. For example, Newark leather workers faced worse odds after their employers committed in 1886 to pay a fine to the industry association if they should offer concessions in any future standoff (Voss, 1993). The organization effect applies if a union defends the strikers. Because it raises the probability of victory (hence, expected payoffs), it might influence the likelihood of a breakdown in negotiations: union members might strike when the unorganized would rather accommodate. In other words, bargaining strategies are endogenous.

[24] For my purposes, industry comprises mining, construction, manufacturing, transportation and utilities. Agriculture, trade and services represent 46 observations in my sample but over two thirds of the labor force. I use the industrial classification of the 1950 Census, imputed by IPUMS (Ruggles et al., 2018).

[25] I do not adjust estimates for differences in capital stock, child labor or unemployment incidence. Although my results are robust to such adjustments, these variables are poorly measured, so I omit them. Additional estimates are available upon request.

Therefore, unions have a direct effect on the probability of success (given a baseline rate, they increase the total rate) and an indirect effect (they enable strikes with lower baseline rates). This indirect impact lowers the average success rate of organized walkouts, which would downward bias estimates of the organization effect.[26] It is important to distinguish these two channels: if I estimate the organization effect to be zero, is it because unions did not help workers win or because their indirect effect offset the direct one?

I use an instrument to establish causality: the existence of an assembly of the KOL in the locality of the dispute in the preceding year. It should correlate with union intervention for two related reasons: it shows that the local workforce had unionized to some extent and it indicates that workers had easy access to union officers. As Imbens and Angrist (1994) note, it identifies effects on compliers, for which union support depended on the presence of the KOL in the community. The identification is mostly due to geographic variation: I compare the success rate in localities with an assembly to the rate in localities without them. A longitudinal analysis is inviable because of the incidence of labor strife is so low in most towns that I do not observe stoppages before the entry of the Knights and afterwards.

A valid instrument must satisfy the exclusion restriction: it must only correlate with outcomes through the treatment. My instrument incorporates a lag between observations and the creation of new assemblies. This construction ensures that it is orthogonal to the baseline success rate so far as its determinants change over time. For instance, the Knights could not anticipate in 1885 that Newark leather manufacturers would later associate. My results are robust to the choice of lag (see Section 8). Two threats to the exogeneity condition remain. First, the presence of a local assembly might correlate with static covariates. The KOL had an incentive to win strikes, which could help them attract and retain members. Therefore, they might have targeted communities where strikers were most likely to succeed, causing upward bias. There is no anecdotal evidence though that they took strike outcomes into consideration as they expanded. Recall from Subsection 2.2 that the Knights pursued membership growth in the hope of advancing labor legislation via the ballot box. They did not direct organizers' efforts, which were mainly constrained by recruitment opportunities and the location of existing assemblies. Still, the presence of an assembly might unintentionally correlate with determinants of conflict outcomes. For instance, locals were more common in urban areas (due to the abundance of manufacturing workers). I allay this concern by reweighting the sample for imbalances in strike characteristics and

[26] Card and Olson (1995) raise a different concern. Unions were not keen on industrial action, since a defeat could trigger defections and imperil the organization. (See also Friedman (1988) and Kaufman (2001).) If unions avoided endorsing riskier strikes, naive estimates would be upward biased. There is plausible anecdotal evidence in support of this hypothesis, but my results suggest otherwise.

market conditions (see Section 5). These corrections do not affect my results. Second, union members might not be representative of strikers at large. Selection should work against me though in that a weak bargaining position gives workers an incentive to unionize before striking. The Knights were especially susceptible to negative selection, as they did not restrict admissions by skill.

Machado, Shaikh and Vytlacil (2018) propose a statistical test of the exclusion restriction. As Subsection 6.2 shows, I reject the null hypothesis of an invalid instrument at any conventional significance level.

In addition to the exclusion restriction, Abadie (2003) establishes three identification conditions: the existence of compliers, monotonicity and common support. The first condition means that the instrument should correlate with the treatment, which I can easily assess through the first-stage F-statistic. The monotonicity assumption states that the existence of an assembly must not reduce the probability of union intervention, which is plausible. The support condition requires that we observe all covariate values for both values of the instrument. We cannot otherwise separate the sources of variation in the treatment.[27]

For further insight, I examine covariates. Table 1 displays covariate averages by KOL presence and organization status. It presents coefficients from logistic regressions as well. I find similar patterns for the instrument and the treatment. As the logistic analysis shows, the instrument improves balance in such idiosyncratic characteristics as industry and firm size. Discrepancies remain in the proportion of strikes against multiple establishments and in wages at affected establishments. On the other hand, it aggravates imbalances in market conditions. It is particularly associated with industrialized urban communities with a history of successful walkouts. This correlation arises for two reasons: first, KOL assemblies and labor strife were both concentrated in manufacturing centers; second, there is little variation in the instrument within localities (unlike the treatment). As Subsections 6.2 and 7.1 show, individual circumstances influence conflict outcomes more than market conditions. Therefore, the instrument seems helpful.

In addition, Table 1 characterizes compliers.[28] Compliers may differ from the population even if the instrument is valid, which could distort my estimates. I find that these strikes are more likely to be offensive, to involve demands for fewer hours and to occur in urban areas. The Mideast is overrepresented, at the expense of New England and the Midwest, as is the construction industry, at the expense of mining and food, drink and tobacco. Nonetheless, compliers are broadly similar to other observations. Hence, my estimates might plausibly

[27] Common support is equivalent to the assumption of full rank or no collinearity in linear models.

[28] I characterize compliers via Abadie's (2003) weighting method. I reweight the sample for one covariate at a time. The weights are based on a local polynomial estimate of the conditional probability of KOL presence.

Table 1: Covariate means and logistic analysis of KOL presence and organization

	All	Compliers	KOL present No	KOL present Yes	Organized No	Organized Yes	Coefficients from logit model of KOL presence	Coefficients from logit model of Organization
Strike characteristics								
Strike of generic employees (%)[a]	26.981	32.194	22.945	28.181	26.565	27.264	–0.038 (0.142)	0.266 (0.144)
Women as a share of the workforce (%)[a]	8.822	8.295	10.126	8.434	11.049	7.306	–0.290 (0.324)	–0.774 (0.290)***
Strike against multiple establishments (%)[a]	20.380	23.182	14.972	21.988	12.569	25.697	0.492 (0.145)***	0.896 (0.130)***
Average size of affected establishments (log)	4.226	3.839	4.550	4.130	4.695	3.907	–0.015 (0.044)	–0.145 (0.048)***
Average wage at affected establishments (log)	0.706	0.816	0.567	0.747	0.588	0.786	0.773 (0.225)***	2.017 (0.261)***
Weekly hours at affected establishments – 60	0.152	0.085	0.457	0.062	0.430	–0.036	–0.012 (0.008)	0.001 (0.008)
Defensive strike (%)[a]	25.340	18.627	27.746	24.625	26.703	24.412	0.152 (0.128)	0.101 (0.117)
Cause: compensation (%)[a]	68.525	62.119	76.810	66.062	75.184	63.992	–0.230 (0.123)	–0.039 (0.157)
Cause: hours (%)[a]	15.961	27.032	5.370	19.110	7.689	21.592	0.202 (0.267)	0.738 (0.250)***
Cause: union rights (%)[a]	7.533	2.253	5.370	8.176	2.302	11.094	–0.146 (0.218)	1.600 (0.238)***
County characteristics								
Industrial workers (percentage, all workers)	34.756	35.233	31.186	35.818	33.986	35.281	0.027 (0.017)	0.009 (0.013)
Urban industrial workers (percentage, all workers)	25.821	30.018	15.171	28.987	22.344	28.187	0.053 (0.014)***	–0.016 (0.007)***
Gender fragmentation (index, industrial workers)	26.708	30.762	23.823	27.565	23.859	28.646	–0.007 (0.006)	0.021 (0.005)***
Ethnic fragmentation (index, industrial workers)	67.480	73.034	58.181	70.244	63.783	69.996	0.001 (0.006)	0.001 (0.005)
Trade fragmentation (index, industrial workers)	92.700	95.355	89.178	93.747	90.920	93.911	0.022 (0.013)	0.021 (0.012)
Average establishment size (log, manufacturing)	2.723	2.783	2.446	2.805	2.621	2.792	–0.340 (0.215)	–0.407 (0.172)***
Average daily wage (log, manufacturing)	0.142	0.221	–0.039	0.196	0.069	0.191	0.963 (0.432)***	1.575 (0.377)***
Railroad tracks (km / km^2)	0.203	0.274	0.108	0.231	0.163	0.231	4.135 (1.710)***	1.416 (0.672)***
Past labor conflict								
Successful strike in previous year (%)[a]	53.387	59.414	31.300	59.294	43.810	59.651	0.342 (0.148)***	0.241 (0.128)

Continues...

Table 1: Covariate means and logistic analysis of KOL presence and organization (continued)

	All	Compliers	KOL present No	KOL present Yes	Organized No	Organized Yes	Coefficients from logit model of KOL presence	Coefficients from logit model of Organization
Period								
1881 (%)[a]	11.635	8.827	18.633	9.555	13.720	10.216		
1882 (%)[a]	10.517	9.037	17.738	8.370	12.155	9.401		
1883 (%)[a]	11.281	7.549	12.693	10.861	11.188	11.344	0.730 (0.287)***	–0.090 (0.201)
1884 (%)[a]	10.554	14.470	8.788	11.079	10.727	10.436	1.202 (0.307)***	–0.205 (0.206)
1885 (%)[a]	14.954	14.932	16.762	14.417	15.976	14.259	0.863 (0.285)***	–0.209 (0.195)
Before May, 1886 (%)[a]	12.381	14.047	9.032	13.377	13.812	11.407	1.690 (0.327)***	–0.307 (0.226)
After May, 1886 (%)[a]	28.678	30.331	16.355	32.342	22.422	32.936	1.195 (0.316)***	–0.263 (0.178)
Region								
New England (%)[a]	11.710	6.321	23.190	8.297	16.114	8.712		
Mideast (%)[a]	42.812	52.473	30.513	46.468	36.648	47.007	0.839 (0.366)***	0.527 (0.281)
Great Lakes (%)[a]	29.778	19.623	23.515	31.640	28.039	30.962	1.220 (0.440)***	0.653 (0.331)***
Plains (%)[a]	8.913	9.356	13.181	7.644	12.155	6.706	0.852 (0.510)	–0.418 (0.368)
South (%)[a]	4.643	3.400	8.055	3.628	5.157	4.293	1.481 (0.517)***	1.313 (0.435)***
West (%)[a]	2.144	2.680	1.546	2.322	1.888	2.319	2.188 (0.546)***	1.121 (0.454)***
Sector								
Mining and quarrying (%)[a]	15.626	7.455	29.699	11.442	22.744	10.780		
Construction (%)[a]	9.621	23.136	5.858	10.740	6.860	11.501	0.089 (0.258)	–0.408 (0.239)
Food, drink and tobacco (%)[a]	12.922	5.482	10.334	13.691	3.269	19.492	0.304 (0.225)	2.317 (0.307)***
Light manufacturing (%)[a]	34.253	37.384	30.757	35.293	32.505	35.443	0.015 (0.237)	0.266 (0.213)
Heavy manufacturing (%)[a]	19.746	16.581	17.575	20.392	21.501	18.552	–0.140 (0.253)	–0.246 (0.228)
Services (%)[a]	7.831	4.857	5.777	8.442	13.122	4.231	–0.034 (0.309)	–1.610 (0.332)***
McFadden's pseudo R^2							0.311	0.274

[a] Regression coefficients were divided by a hundred.

Notes: The means of covariates among compliers are estimated via reweighting (Abadie, 2003). Data about past strikes are not available for 1881. Both regressions include an intercept and exclude observations from 1881 (for a total of 37 parameters and 4739 observations). Standard errors, in parentheses, are robust to correlation across time and space (see Subsection 5.2).

Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent.

generalize to the entire sample.

For comparison, I estimate the organization effect under selection on observables too (Rosenbaum and Rubin, 1983). The identification framework is similar to Abadie's (2003): the treatment should satisfy conditional independence and common support. These estimates are only consistent if unionization does not affect the probability of a stoppage.

5. Empirical strategy

5.1. Estimation

I am interested in the impact of union involvement on strike outcomes – in particular, the success rate. Because my primary outcome is binary, linear regression is inconsistent. Hence, I adopt a two-stage weighting approach instead. I draw on Abadie (2003), who shows that valid instruments identify the entire marginal distributions of compliers' potential outcomes, and Frölich and Melly (2013), who apply this insight to the estimation of unconditional treatment effects.[29]

For each observation i, write y_i for the outcome of interest, d_i for union sponsorship (the treatment), z_i for the presence of an assembly of the KOL (the instrument) and $\boldsymbol{x}_i$ for the control vector. For concreteness, suppose that y_i is an indicator of success in the following.

In the first stage, I construct estimation weights w_i. The weighting scheme depends on the identification assumptions. Under selection on observables, I use inverse probability weighting (Hirano, Imbens and Ridder, 2003):

$$w_i = 1/\,\mathrm{P}(d_i \mid \boldsymbol{x}_i).$$

In the case of endogenous selection, the weighting scheme is due to Abadie (2003) and Frölich and Melly (2013):

$$w_i = (2d_i - 1)(2z_i - 1)/\,\mathrm{P}(z_i \mid \boldsymbol{x}_i).$$

These weights have two key properties. First, they overweight underrepresented observations in each instrument group, which improves balance in covariates. Second, they are negative when d_i differs from z_i, which helps us recover the treatment effect on compliers

[29] Abadie (2003) develops a similar estimator of conditional effects. I follow Frölich and Melly (2013) for three reasons: first, unconditional effects are easier to interpret; second, his estimator requires that I model expected outcome values; and, third, his estimator seems imprecise, as his empirical illustration attests. Clarke and Windmeijer (2012) and Lewbel, Dong and Yang (2012) discuss the relative merits of alternative estimators of the effect of endogenous treatments on binary outcomes.

by cancelling the contribution of noncompliers.[30] This first step requires an estimate of the conditional probabilities $\mathrm{P}(d_i \mid \boldsymbol{x}_i)$ and $\mathrm{P}(z_i \mid \boldsymbol{x}_i)$. I use a logistic specification. Alternative parametric estimators yield similar results. My sample is too small for more flexible models.

In the second stage, I regress y_i on d_i and an intercept:

$$\left(\hat{\alpha}, \hat{\beta}\right) = \arg\min_{\alpha,\beta} \left\{\hat{\mathrm{E}}\left[w_i(y_i - \alpha - \beta d_i)^2\right]\right\}.$$

The constant $\hat{\alpha}$ estimates the average baseline success rate – i.e. the success rate of wildcat walkouts. The coefficient $\hat{\beta}$ gives the average treatment effect. It is equal to the difference in weighted mean outcomes between unorganized and organized strikes. Under selection on observables, $\hat{\alpha}$ and $\hat{\beta}$ pertain to the entire sample; under endogenous selection, to compliers.[31]

How does weighting improve on linear regression? Weighting does not require parametric assumptions in principle; hence, it readily accommodates binary responses. Fully nonparametric estimation is difficult in practice though because conditional probabilities are subject to the curse of dimensionality. The choice of a model for $\mathrm{P}(d_i \mid \boldsymbol{x}_i)$ and $\mathrm{P}(z_i \mid \boldsymbol{x}_i)$ is important, as one must forecast individual probabilities in constructing the weights w_i. On the other hand, linear regression imposes linearity on the conditional expectation of outcomes. While ordinary least squares give the best linear approximation to the average effect of conditionally exogenous treatments, this property does not extend to instrumental regression under endogenous selection (Abadie, 2003; Lewbel, Dong and Yang, 2012). Note that these methods yield the same treatment effect without covariates (i.e. $\boldsymbol{x}_i = 1$).[32]

5.2. Inference

Correct inference must account for correlated errors. My sample features dependence by design: as Subsection 3.1 explains, the *Third Report* could present a single strike against multiple establishments as several rows. Local shocks and dynamic bargaining may also induce intrinsic correlation over time and space. For example, Biggs (2002) analyzes the

[30] Hirano, Imbens and Ridder (2003) develop an early application of inverse probability weighting to treatment evaluation. See also Firpo and Pinto (2016). Note that the two weighting schemes coincide when the instrument is the treatment itself: Frölich and Melly (2013) thus generalize inverse probability weighting in the same sense as two-stage least squares generalizes ordinary least squares.

[31] In terms of averages, $\hat{\alpha} = \hat{\mathrm{E}}[(1-d_i)w_i y_i]/\hat{\mathrm{E}}[(1-d_i)w_i]$ and $\hat{\beta} = \hat{\mathrm{E}}\left[d_i w_i \left(y_i - \hat{\alpha}\right)\right]/\hat{\mathrm{E}}(d_i w_i)$.

[32] Inverse probability weighting and ordinary least squares yield the same intercept as well (equal to the raw average success rate of unorganized strikes). On the other hand, Frölich and Melly (2013) do not compute the same intercept as two-stage least squares: weighting gives compliers' average baseline outcome, whereas regression estimates a mixture of compliers' and never-takers' (Abadie, 2003).

marches of May 1886 in Chicago as a sequence of interactions, spanning preemptive concessions, violent pickets and uneasy truces.

My approach is based on Conley (1999). I assume that the maximum possible residual correlation between observations decreases with distance in time and space. In other words, I allow for arbitrary correlation between two stoppages if they begin in the same place on the same day, but distant episodes must be effectively independent. It seems plausible that labor strife in New York would be more likely to spill over to Brooklyn than San Francisco.

By Theorem 6.1 of Newey and McFadden (1994), my estimators are asymptotically normal. Their limit variances take the form $\mathrm{E}(v_{ij}\boldsymbol{h}_i\boldsymbol{h}_j^\top)$ for some weights v_{ij} and some vector function $\boldsymbol{h}_i$. The weights v_{ij} capture the residual correlation between observations. (See Appendix B for the formula for $\boldsymbol{h}_i$.) Write $r_t(i,j)$ for the difference in start dates and $r_s(i,j)$ for the spatial distance between observations i and j.[33] Let k be a kernel function. Let b_t and b_s be bandwidths. My variance estimator is:

$$\hat{\mathrm{E}}\left[k\left(\sqrt{[r_t(i,j)/b_t]^2 + [r_s(i,j)/b_s]^2}\right)\hat{\boldsymbol{h}}_i\hat{\boldsymbol{h}}_j^\top\right].$$

The kernel term $k(...)$ bounds v_{ij} in absolute value. Other than regularity conditions, consistency requires that b_t and b_s increase with the sample size at an appropriate rate, relaxing the bound on v_{ij}.

I set b_t to one year, so the bound on the correlation between two observations in the same locality is no lower than 0.75 if they start within three months of each other. I set b_s to 380 km, so the bound on the correlation between two observations in the same county is no lower than 0.75 if they start on the same date. I use the Parzen kernel for k. My findings are robust to these choices.[34]

6. Impact of the KOL on labor strife

6.1. Effect on strike incidence

This subsection investigates the impact of unionization on the incidence of labor strife. Recall from Section 4 that organized workers might undertake riskier strikes than the

[33] The start date is incomplete for 13 observations, in which case I impute the first day of the month. If a strike affected multiple localities (110 observations), I base distances on the first entry. Since the *Third Report* does not seem to list localities in a logical pattern, I assume that the first entry was the main theater of events. If the locality is a county, I base distances on the coordinates of its centroid.

[34] The standard error on my benchmark estimate of the average effect of union sponsorship on the success rate is 0.095. If I set b_s to 570 km and b_t to 547 days (an increase of fifty percent), the standard error becomes 0.094. Additional results are available upon request.

Table 2: Impact of KOL assemblies on strike incidence

	(1)	(2)	(3)	(4)	(5)
KOL effect					
Two-stage least squares	0.451***	0.217***	0.223***	0.156***	0.147***
	(0.025)	(0.026)	(0.034)	(0.029)	(0.049)
Ordinary least squares	0.328***	0.160***	0.173***	0.105***	0.060***
	(0.016)	(0.011)	(0.014)	(0.010)	(0.013)
Fit					
First-stage F-statistic	441.194	186.050	166.670	149.084	121.862
Adjusted R^2 (OLS)	0.198	0.378	0.349	0.422	0.536
Controls					
Lagged outcome		×		×	×
County characteristics			×	×	
County effects					×
Region effects		×	×	×	
Year effects		×	×	×	×
Sample					
Counties	2519	2519	2519	2519	2519
Years	5	5	5	5	5
Parameters	2	12	19	20	2525

Notes: See Subsection 6.1 for details. Standard errors, in parentheses, are clustered at the county level. The first-stage F-statistic tests the effect of the instrument on the treatment in a linear specification.
Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent.

unorganized if union support increases their probability of success. This possibility suggests that unionization could increase the frequency of conflict. I cannot properly test this hypothesis because I do not observe unionization rates in the 1880s. I can however address a related question: whether strike incidence correlates with the presence of the KOL.

To that end, I construct a panel of counties from 1882 to 1886. The outcome is an indicator of the occurrence of a walkout. The treatment is an indicator of the existence of a local assembly of the KOL in the previous year. To address attenuation bias from measurement error, I construct two instruments: an indicator of the existence of assemblies in neighboring counties in the previous year and the fraction of the labor force in neighboring counties with assemblies in the previous year. Table 2 shows my estimates. Since I combine two instruments and one is continuous, I use linear regression in lieu of reweighting. Standard errors are clustered at the county level.

The first specification does not include covariates. KOL presence is associated with an increase in strike incidence from 1 to 46 percent per year. This effect decreases considerably once I correct it for differences in economic development between counties, which

correlates with both unionization and striking. The fourth specification adds controls for past stoppages, market characteristics, region and year. KOL presence is now associated with a shift in strike incidence from 6 to 22 percent. The last specification includes county effects. The coefficient drops from 17 to 15 percentage points.

These estimates are not causal, so one should not take them at face value. Nonetheless, they provide circumstantial evidence in favor of my hypothesis. Moreover, it seems that my covariates are a good proxy for market conditions so far as they do not change over time.

6.2. Effect on union sponsorship and the success rate

This subsection explores the impact of the KOL on work stoppages. First, I estimate their effect on the probability of union intervention. This exercise is analogous to the first stage of linear regression. Second, I estimate their effect on the success rate, which is the reduced form of my main specification. Unlike the previous subsection, the unit of observation is the strike. Table 3 presents my results. Table 10 in Appendix C shows their linear counterparts.

The existence of an assembly in the locality of the dispute increased the probability of union involvement by 26.1 percentage points from a baseline of 39.4 percent to 65.5 percent. This estimate is robust to balance adjustments. The third specification is the exception. It takes idiosyncratic strike characteristics into account, which absorb much of the variation in union support, so the coefficient falls from 26.1 to 16.8 percentage points. These results are precise, so the instrument should satisfy the correlation condition (see Section 4).

The Knights had a more modest impact on the success rate. Their presence raised it by 8.6 percentage points from a baseline of 44.9 percent to 53.5 percent. This estimate is robust to balance corrections as well, though I lose precision if I adjust it for differences in market characteristics. As the coefficient of determination demonstrates, covariates exert little influence on the success rate.[35]

Machado, Shaikh and Vytlacil (2018) develop tests of the exclusion restriction when the instrument, the outcome and the treatment are all binary. It exploits the fact that exogeneity bounds the coefficient from the reduced form: if the instrument only affects outcomes through the treatment, the correlation between the instrument and the outcome should be neither too small nor too large. I implement their test of the null hypothesis of an invalid instrument under the monotonicity assumption (see Section 4). Because

[35] This finding might seem surprising, as covariates explain much of the variation in conflict incidence and union support. To understand it, consider the following schematic model. Workers draw a probability of success, which is either 0.4 or 0.6. Covariates influence their probability of drawing 0.4 or 0.6. Workers strike if it is 0.6. Then, there is no variation in the success rate for covariates to explain, but they influence the likelihood of a strike.

Table 3: Impact of KOL assemblies on organization and success rates

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome: organization						
KOL effect	0.262***	0.223***	0.168***	0.234***	0.269***	0.223***
	(0.032)	(0.029)	(0.029)	(0.052)	(0.034)	(0.028)
Baseline probability	0.393***	0.409***	0.448***	0.376***	0.392***	0.413***
	(0.021)	(0.023)	(0.024)	(0.044)	(0.023)	(0.025)
Adjusted R^2 (OLS)	0.050	0.159	0.282	0.202	0.050	0.166
Outcome: success						
KOL effect	0.086***	0.089***	0.065***	0.093***	0.112***	0.110***
	(0.024)	(0.022)	(0.023)	(0.040)	(0.025)	(0.024)
Baseline probability	0.449***	0.439***	0.461***	0.428***	0.418***	0.409***
	(0.017)	(0.020)	(0.023)	(0.043)	(0.018)	(0.023)
Adjusted R^2 (OLS)	0.005	0.045	0.074	0.049	0.008	0.049
Controls						
Period, region & sector		×	×	×	×	×
Strike characteristics			×			
County characteristics				×		
Past labor conflict					×	×
Sample						
Sample size	5363	5363	5363	5363	4739	4739
Year 1881	×	×	×	×		
Parameters	2	18	28	26	2	18

Notes: See Section 3 for information about the data. Column (6) excludes observations from 1881 for lack of strike microdata for 1880. Unless noted, the table shows reweighted estimates (see Subsection 5.1). Standard errors, in parentheses, are robust to correlation across time and space (see Subsection 5.2).
Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent.

the procedure uses bootstrapped critical values, I cannot follow my preferred inference strategy (see Subsection 5.2). I cluster critical values at the county level instead. I reject the null hypothesis at any conventional significance level: the test statistic is 3.58, which is comfortably larger than the critical value of 2.36 at the one-percent level. This result provides additional evidence in favor of my identification strategy.

7. Impact of union sponsorship on labor strife

7.1. Effect on the success rate

Table 4 presents my main results: the average effect of union sponsorship on the probability of success of a strike. The first two rows contain estimates by the weighting method for endogenous treatments of Frölich and Melly (2013). The first row shows the average treatment effect and the second row shows the baseline success rate. The following two rows contain analogous estimates by inverse probability weighting for conditionally exogenous treatments (Hirano, Imbens and Ridder, 2003). The fifth row displays the m-statistic of Hausman (1978), which tests the estimates of the union effect for equality. The sixth row gives the coefficient of determination. The seventh row shows the first-stage partial F-statistic.[36] The control set changes across columns. The last specification excludes observations from 1881 for lack of stoppage data for 1880. For comparison, the fifth column shows estimates under the benchmark specification without observations from 1881 as well. Table 10 in Appendix C shows their linear counterparts.

Column (1) is my benchmark. It ignores imbalances in covariates. Wildcat strikers' mean success rate was 37.8 percent across compliers (second row) and 44.2 percent across the entire sample (fourth row). These estimates are similar, which suggests that compliers form a representative subsample of the population. They confirm the intuition that workers walked out when they stood a reasonable chance of winning (Biggs, 2002). The average causal effect of organization was 32.7 percentage points (first row), which implies that union intervention increased the probability of victory from 38 to 71 percent. In line with Card and Olson (1995) and Friedman (1988), the naive estimate is 12 percentage points (third row). These two coefficients are statistically different at the five-percent level: the m-statistic is 2.436 (fifth row).

Columns (2) through (6) take covariates into account. The second specification balances

[36] The m-statistic is asymptotically normally distributed. Given a linear regression of the treatment on the instrument and covariates, the first-stage partial F-statistic is the squared t-statistic for the zero null hypothesis. It follows a χ^2_1 distribution asymptotically. It is a measure of instrument strength and relates to the share of compliers in the sample (cf. Section 4).

TABLE 4: EFFECT OF UNION SPONSORSHIP ON THE STRIKE SUCCESS RATE

	(1)	(2)	(3)	(4)	(5)	(6)
With instrument						
Union effect	0.327***	0.397***	0.364***	0.286	0.414***	0.446***
	(0.095)	(0.106)	(0.155)	(0.820)	(0.102)	(0.127)
Baseline rate	0.378***	0.365***	0.431***	0.255***	0.340***	0.347***
	(0.053)	(0.078)	(0.108)	(0.061)	(0.056)	(0.108)
No instrument						
Union effect	0.122***	0.108***	0.109***	0.099***	0.123***	0.096***
	(0.026)	(0.022)	(0.026)	(0.024)	(0.027)	(0.024)
Baseline rate	0.442***	0.447***	0.441***	0.450***	0.432***	0.444***
	(0.014)	(0.017)	(0.022)	(0.019)	(0.014)	(0.017)
Fit						
Hausman m-statistic	2.436	2.844	1.642	0.227	3.163	2.819
First-stage F-statistic	68.462	57.804	27.225	16.362	63.185	46.679
Adjusted R^2 (OLS)	0.014	0.049	0.077	0.053	0.014	0.051
Controls						
Period, region & sector		×	×	×		×
Strike characteristics			×			
County characteristics				×		
Past labor conflict						×
Sample						
Sample size	5363	5363	5363	5363	4739	4739
Year 1881	×	×	×	×		
Parameters	2	18	28	26	2	18

Notes: See Section 3 for information about the data. Column (6) excludes observations from 1881 for lack of strike microdata for 1880. Unless noted, the table shows reweighted estimates (see Subsection 5.1). Standard errors, in parentheses, are robust to correlation across time and space (see Subsection 5.2). The m-statistic tests the difference between estimates of the union effect (Hausman, 1978). The first-stage F-statistic tests the effect of the instrument on the treatment.
Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent.

the instrument across periods, regions and sectors. The causal organization effect rises from 32.7 to 39.7 percentage points. I obtain similar numbers after including controls for idiosyncratic characteristics and market conditions, but I lose precision. The adjusted coefficients are not statistically different from the benchmark.[37] The last specification includes an indicator of successful past walkouts in the same sector and county. The average treatment effect becomes 43.8 percentage points. Note that this specification excludes observations from 1881, since I lack strike data for 1880. This restriction explains most of the change in the causal estimate: if I compute the organization effect without controls (like Column (1)) and without observations from 1881 (like Column (6)), I obtain 41 percentage points (Column (5)).

These results support the hypothesis that unions help workers win strikes. This advantage rationalizes unions' growing role in industrial conflict (cf. Figure 1). Moreover, I find evidence of downward bias in the naive estimate of the organization effect, which suggests that workers adjust their bargaining strategies in response to the availability of union support and confront stronger employers on average than the unorganized. In other words, unionization expands their tactical inventory, enabling them to strike in less favorable circumstances. (See Subsection 7.3 for a discussion of possible mechanisms.)

Unions were actually ambivalent about industrial action in the 1880s. Besides the financial toll, stoppages were fraught with danger: job loss, blacklisting, violence against picket lines, jail terms and more (Currie and Ferrie, 2000; Rosenbloom, 1998). Officers worried that a defeat might threaten the survival of the association (Kremer and Olken, 2009), as it could depress morale and wreck leaders' prestige. Competition for members among associations was fierce (Kaufman, 2001). Moreover, industrial disruption antagonized sympathetic employers and public authorities (Friedman, 1988; Voss, 1993). Therefore, union executives had reason to avoid conflict (especially if victory was uncertain).[38] However, these fears clashed with the interests of the rank and file. Workers unionized to maximize their own welfare by increasing wages, decreasing hours and improving work conditions (Eichengreen, 1987; Kremer and Olken, 2009). Unions could hardly disavow strikers, lest it weaken their appeal to existing members and potential recruits (Perlman, 1918). Local

[37] As Column (4) shows, my estimate is exceptionally imprecise if I reweight the sample for differences in county characteristics. The additional noise is due to the overlap between assemblies' location and urbanization (cf. Table 1), which threatens the assumption of common support. Unlike other specifications, this estimate is moreover sensitive to the choice of urbanization measure and of the model of $P(d_i \mid \boldsymbol{x}_i)$ (see Subsection 5.1). The table shows my lowest and noisiest estimate.

[38] Postwar commentators made the opposite argument: for personal and ideological reasons, union leaders were more belligerent than the rank and file. This view motivated legislation to condition industrial action on secret ballots. For example, see Moore (2013, 2016) or Olofsgård (2012). This difference may be due to the institutionalization of union rights in the 20th century.

officers were particularly willing to endorse unauthorized picketing (Card and Olson, 1995; Kremer and Olken, 2009). My results imply that unions were not an effective moderating force in this period: workers were able to impose facts on the ground and extract support for difficult confrontations.

There is evidence of such tension within the KOL. For example, the leading article of the *Journal of United Labor* of June 1882 bemoaned that the mechanic hung on "to the *old* barbarous, clumsy, unyielding, and treacherous system, known as strike, for his own personal benefit [emphasis in the original]".[39] It went on to berate the cost and uncertain benefits of work stoppages. The *Journal* later quoted the *Chicago Express*: "The striking mania among the workers has partly yielded to judicious counsel. Organization is regulating it, and will presently control it fully [...]. Strikes are voted down as disorderly and leading to bloodshed." Yet this very edition contains an appeal for aid from embattled miners in Maryland. It begins: "Whilst I have condemed [sic] without stint the strike system, it is not without purpose, or to no good in all cases, when I witness the action of capital in demanding of their employees that they work twelve hours for a day's work." It is clear that local assemblies paid lip service to official guidelines against walkouts. At the General Assembly of 1882, Grand Master Powderly declared: "One cause for the tidal wave of strikes that has swept over my Order comes from the exaggerated reports of the strength of the Order, numerically and financially, given by many of my organizers. Such a course may lead men into the Order, but by a path that leads them out again [...]" (Wright, 1887). Nonetheless, assembly representatives seized the occasion to legalize strike relief. They reversed this position in 1884.[40] Nor were these disagreements exclusive to the KOL. Chicago's *Inter Ocean* reported the following resolution in May 1881: "The Tinners and Sheet-iron Workers' Union, No. 1, of Chicago, are not organized in the spirit of a strike; [...] There is a spirit of discontent prevalent among the different branches of our trade; therefore, [...] the Union will not hold itself responsible for the acts of individual members."

Note that my findings do not support theories of asymmetric information between officers and the membership. Ashenfelter and Johnson (1969) and Olofsgård (2012) argue that union leaders have access to private information (e.g., the company's books), which could help them forecast conflict outcomes. Organized workers should then learn that certain disputes

[39] The *Journal of United Labor* was the official journal of the KOL. It circulated between 1880 and 1889. This edition is the second number of the third volume, published by Robert D. Layton in Pittsburgh (PA).

[40] On this occasion, Powderly observed: "[...] many new Assemblies are deceived on being organized; they are told by the Organizer that the assistance fund is laying idle [...]. These members, thinking that they are entitled to this fund, become obnoxious and troublesome to their employers [...];the result is a lock-out and trouble" (Powderly, 1884). Although he refers to lockouts, this quote shows that workers could become bellicose if they believed that union support was forthcoming.

TABLE 5: EFFECT OF UNION SPONSORSHIP ON STRIKE PAYOFFS

	Job loss (1)	Weekly hours (2)	Daily wage (3)	Daily wage (4)
With instrument				
Union effect	–0.226***	–0.984	0.018	–0.025
	(0.070)	(3.070)	(0.024)	(0.054)
Baseline outcome	0.561***	–8.344***	0.129***	–0.101***
	(0.051)	(2.595)	(0.013)	(0.041)
No instrument				
Union effect	–0.057**	–2.558***	0.018***	–0.003
	(0.023)	(1.163)	(0.006)	(0.010)
Baseline outcome	0.502***	–5.295***	0.122***	–0.128***
	(0.016)	(0.899)	(0.003)	(0.006)
Fit				
Hausman m-statistic	–2.486	0.578	0.019	–0.443
First-stage F-statistic	68.462	15.115	37.126	6.803
Sample				
Cause	Any	Hours	Wage raise	Wage cut
Result	Any	Success	Success	Defeat
Sample size	5363	381	1498	367

Notes: See Section 3 for information about the data. Wage and hours regressions exclude strikes after which all strikers lost their jobs. Wage regressions use log wages. Unless noted, the table shows reweighted estimates (see Subsection 5.1). Standard errors, in parentheses, are robust to correlation across time and space (see Subsection 5.2). The m-statistic tests the difference between estimates of the union effect (Hausman, 1978).
Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent.

are hopeless, so their baseline success rate would be higher than wildcat strikers' and naive estimates, upward biased. Information asymmetries must thus have been relatively unimportant in the 1880s, though they may have grown with the institutionalization of collective bargaining in the 20th century.

7.2. Effect on payoffs

The previous subsections analyzed the probability of success. This subsection focuses on payoffs. Table 5 presents my findings.

Workers did not always return to their jobs at the end of hostilities (Currie and Ferrie, 2000; Rosenbloom, 1998). Some found alternative employment during the standoff. Others were permanently replaced by strikebreakers. Some employers refused to reinstate strike leaders in particular, though they might offer concessions to other workers. Dis-

charged employees were often blacklisted as well. Column (1) investigates unions' influence over dismissals. The *Third Report* does not specify job losses, but it gives the change in firm size and the number of new employees by gender, which allows me to approximate the incidence of layoffs. As the baseline rate shows, there were layoffs in more than half of all disputes. Organization offered workers protection: the causal estimate implies that unions decreased the incidence of job loss by 22.6 percentage points or nearly half. This coefficient is four times greater than the naive estimate, which conforms with the hypothesis that organized labor took more risk than the wildcat.

Column (2) examines the change in weekly hours. Following Card and Olson (1995), I restrict the sample to successful strikes for a shorter workweek. Unorganized compliers achieved an average reduction of 8.3 hours. Unionization had no significant impact. Column (3) reports similar findings for the change in daily pay after a successful stoppage for a wage raise: the mean baseline increase is 12.9 percent and the organization effect is insignificant. On the other hand, the naive estimates are significant (–2.6 hours and 1.8 percentage points, respectively), which suggests that officers may have been sensitive to pressure from the ranks over the terms of settlement as well as the decision to strike.[41]

I find no significant organization effect on the payoff of successful stoppages. Note however that there was a significant effect on the expected outcome of a walkout, since it depends on the probability of success in addition to the realized payoff. Moreover, a rough estimate indicates that the benefits outweighed the costs of organized strikes on average. The KOL charged $15 to charter a new assembly. The minimum membership was ten workers, so suppose that my hypothetical worker contributed $1.5. There were also an induction fee ($1) and a quarterly membership fee ($0.25). Suppose that they struck after a year. Unionization cost $3.5. Its benefit is a higher success rate by 0.33, times a wage raise of 15 percent, times a mean initial daily wage of $2 for male strikers – i.e. 9.6 cents per day or $7.2 per quarter.[42]

The last column considers the decrease in daily wages after an unsuccessful stoppage against a wage cut. Daily pay fell by 10 percent on average among compliers. Neither estimator yields a significant organization effect. This result is unsurprising: employers announced wage cuts before workers struck, so union intervention should only affect payoffs through the probability of victory.

[41] Note that layoffs may induce spurious changes in wages and hours, which I cannot account for.

[42] This calculation ignores the impact of organization on strike duration. As Subsection 7.3 shows, I find no significant effect on duration.

TABLE 6: EFFECT OF UNION SPONSORSHIP ON STRIKE DEVELOPMENT

	With instrument		No instrument		Hausman
	Baseline	Union effect	Baseline	Union effect	m–stat.
Entire workforce on strike	0.179***	0.506***	0.298***	0.111***	3.333
(indicator)	(0.053)	(0.135)	(0.029)	(0.033)	
New workers from other places	0.204***	–0.202***	0.121***	0.010	–3.543
(indicator)	(0.030)	(0.065)	(0.009)	(0.017)	
New workers after strike	0.093***	0.096***	0.115***	0.028***	1.864
(w.r.t. initial workforce)	(0.023)	(0.039)	(0.008)	(0.010)	
Shutdown of affected firms	0.701***	–0.280***	0.591***	–0.013	–2.398
(indicator)	(0.064)	(0.120)	(0.031)	(0.027)	
Financial assistance (indicator)	0.098***	0.398***	0.084***	0.331***	0.816
	(0.032)	(0.101)	(0.010)	(0.028)	
Duration (log days)	2.329***	–0.186	1.965***	0.542***	–2.943
	(0.158)	(0.255)	(0.048)	(0.070)	

Notes: See Section 3 for information about the data. Each row presents presents estimates for a different outcome. The table shows reweighted estimates (see Subsection 5.1). Standard errors, in parentheses, are robust to correlation across time and space (see Subsection 5.2). The m-statistic tests the difference between estimates of the union effect (Hausman, 1978).
Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent.

7.3. Mechanisms

Subsection 7.1 argued that unions raised the probability of success of a strike. This subsection explores the mechanisms behind it. Table 6 reports my results.

I would ideally quantify the contribution of different channels to the organization effect. This exercise is infeasible though because strikers' tactics are endogenous. Consider for example financial assistance. A naive regression would implausibly have us believe that it lowered the success rate. Selection bias is the likely culprit: for instance, unions may have prioritized the most difficult confrontations in allocating funds. An accurate decomposition would thus require a separate instrument for each mechanism of interest. Given the limitations of my data, I adopt a simpler approach and compute the impact of unionization on the course of each dispute.

Association was partly an answer to such challenges to collective action as coordination failures and free riding. First, unions fostered solidarity through lectures, meetings, parades, songs, etc., which helped workers internalize their contribution to others' welfare and increased the social fallout of crossing the picket line. Second, officials could leverage their experience and the threat of expulsion to impose discipline, improve coordination and overcome mistrust. They could also accumulate bargaining expertise, which helped them negotiate better settlements. Thirdly, there were logistical advantages: for example, labor journals expanded the reach of boycotts.

As the first row of Table 6 shows, organization succeeded in boosting turnout: nearly seventy percent of union strikes involved the entire workforce of affected establishments, against 18 percent of the wildcat. Unions had a more complex impact on strikebreaking. As Subsection 7.2 noted, layoffs were rarer in organized stoppages. Unions were especially effective against outside replacement workers (second row). However, firms hired more permanent replacements if they hired them at all (third row): new employees represented a fifth of the initial workforce on average, against a tenth for unorganized stoppages. This difference may reflect increased participation in union strikes. Furthermore, affected establishments were less likely to close by 28 percentage points despite higher turnout (fourth row), which suggests that employers procured either temporary strikebreakers or help from other firms.

Few strikers had enough savings for a prolonged standoff. Some found alternative employment during stoppages, but many relied on outsiders for financial relief. Labor societies were the main providers, building resistance funds in peacetime and pooling risks across branches. When unions' resources proved insufficient, they coordinated donations (e.g., KOL assemblies pleaded for aid on the *Journal of United Labor*). As the fifth row of Table 6 shows, half of authorized walkouts received financial assistance, against a tenth of the unorganized. The difference between estimators is not significant, which is interesting in that strikers exerted little influence over aid.

The last row examines duration. Most disputes were short in this period: a quarter ended within three days and half ended within ten days. I find no causal effect on duration. This result is surprising to some extent: one would think that organization helped workers endure longer stoppages (through financial relief, for instance). However, employers might concede defeat earlier if they expect greater resistance from unionized workers. Therefore, the organization effect is ambiguous a priori.

7.4. Subgroup effects

Friedman (1988) studies the impact of union intervention on industrial conflict across two phases of the American labor movement: the radical experiment (1881–86), under the aegis of the KOL, and the return of craft unionism (1887–94), led by the AFL. He finds an increase in the organization effect after 1886, which he attributes to a change in strategy. The Knights sought the strength in numbers to bully employers into submission, he argues, but they could not provide strikers with adequate assistance or win the sympathy of hostile public authorities. By contrast, craft unions restricted membership to skilled workers and increased fees. Their walkouts were fewer, smaller, better planned and better funded –

hence, more successful.

Table 7 presents estimates by strike size and skill level. The first column divides the sample according to the average workforce of affected establishments. As Friedman (1988) argued, organized workers had a lower baseline success rate against large employers in the early 1880s. However, union intervention was significantly more effective, compensating the lower intercept. Large strikes were probably susceptible to coordination problems, which unions could mitigate. I obtain the same pattern if I classify observations according to the number of strikers instead of firm size (second column). The last two columns are based on proxies for strikers' skill level: whether the *Third Report* specified their trade and whether their mean wage was higher than $2 before the conflict.[43] Differences between estimates are neither significant nor consistent. We find little evidence overall in support of Friedman (1988).

8. Robustness tests

Subsection 7.1 examined the robustness of the estimate of unions' effect on the success rate to different control sets. This section explores additional robustness tests.

Table 8 investigates sample restrictions. The first row reduces the sample to strikes whose locality had not suffered stoppages in the previous year. It addresses the concern that the Knights may have targeted areas with a higher success rate. If a locality experienced few strikes in the past, it would have been harder for them to predict conflict outcomes. The organization effect is here equal to 45.5 percentage points. As this sample excludes observations from 1881, the relevant benchmark is Column (5) in Table 4, 41 percentage points.[44] The second row excludes localities which the KOL had not organized by 1886. The estimate becomes 30 percentage points, against 32.7 for my benchmark. The last two rows split the sample into a subsample without assemblies (third row) and a subsample with assemblies (fourth row). They indicate that unorganized strikes had similar success rates across instrument values. The naive organization effect is lower in locations without assemblies by four percentage points, but this estimate is imprecise, so the difference is not statistically significant.

Table 9 considers two alternative instruments. Because one is continuous, this table uses linear regression in lieu of reweighting. The first two columns use the existence of

[43] In microdata from the *Census of Manufactures* (Atack and Bateman, 2016), $2 is the median average wage of skilled workers and the 99th percentile of the average wage of unskilled labor.

[44] Note that this test is a more stringent version of Column (6) in Table 4, in which we reweight observations for the incidence of successful past strikes in the same sector and locality. Note too that this subset excludes most large cities (cf. Footnote 37).

TABLE 7: EFFECT OF UNION SPONSORSHIP ON THE STRIKE SUCCESS RATE BY SUBGROUP

	Estab. size above median	Strike size above median	Strike of generic workers	Strikers' wage above $2
With instrument				
Union effect if outside subgroup	0.174	0.253***	0.349***	0.366***
	(0.103)	(0.099)	(0.098)	(0.114)
Baseline rate if outside subgroup	0.470***	0.417***	0.412***	0.309***
	(0.058)	(0.068)	(0.062)	(0.073)
Union effect if in subgroup	0.544***	0.418***	0.308***	0.311***
	(0.163)	(0.148)	(0.146)	(0.152)
Baseline rate if in subgroup	0.243***	0.332***	0.277***	0.497***
	(0.099)	(0.073)	(0.084)	(0.088)
Equality test (union effect)	1.918	0.926	–0.233	–0.287
Equality test (total probability)	1.008	0.564	–1.309	0.871
No instrument				
Union effect if outside subgroup	0.134***	0.150***	0.121***	0.127***
	(0.035)	(0.030)	(0.027)	(0.030)
Baseline rate if outside subgroup	0.458***	0.419***	0.457***	0.437***
	(0.019)	(0.016)	(0.017)	(0.015)
Union effect if in subgroup	0.100***	0.090***	0.128***	0.108***
	(0.027)	(0.032)	(0.048)	(0.038)
Baseline rate if in subgroup	0.430***	0.470***	0.402***	0.458***
	(0.016)	(0.020)	(0.022)	(0.028)
Equality test (union effect)	–0.754	–1.396	0.129	–0.395
Equality test (total probability)	–1.603	–0.241	–1.089	0.037
Share of sample in subgroup	0.492	0.491	0.270	0.395

Notes: The table shows reweighted estimates for each subgroup (see Subsection 5.1). For example, the first two estimates in the first column concern establishments whose size is below the median. Medians are taken within regions and sectors. The equality test is the t-statistic for the difference between subgroups. Standard errors, in parentheses, are robust to correlation across time and space (see Subsection 5.2).
Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent.

TABLE 8: ROBUSTNESS OF THE UNION EFFECT TO SAMPLE RESTRICTIONS

	Sample size	Instrument	Baseline rate	Union effect
Localities without strikes in the previous year	1459	Yes	0.436***	0.455***
			(0.139)	(0.222)
Localities with a KOL assembly by 1886	5035	Yes	0.364***	0.300***
			(0.061)	(0.104)
Localities without KOL assemblies	720	No	0.436***	0.083
			(0.027)	(0.053)
Localities with KOL assemblies	4643	No	0.444***	0.125***
			(0.015)	(0.027)

Notes: See Section 3 for information about the data. The table shows reweighted estimates (see Subsection 5.1). The first row excludes observations from 1881 for lack of strike microdata for 1880. Standard errors, in parentheses, are robust to correlation across time and space (see Subsection 5.2).
Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent.

an assembly in the locality of the dispute in 1880 (instead of the preceding year). This specification should decrease the correlation between the instrument and the baseline success rate so far as its determinants change over time. The first column does not take covariates into account. The resulting estimate is 31 percentage points, against 32.7 for my benchmark. The second column uses the main instrument as a control, which should further diminish any residual correlation with determinants of the probability of success. The coefficient remains similar at 28.1 percentage points. The last two columns replace the instrument with the log distance between the locality and the nearest assembly in 1880.[45] This formulation should lower the correlation between the instrument and the success rate to the extent that its determinants are specific to each locality. The estimates are somewhat larger than the benchmark: 40.9 and 44.1 percentage points. Because these instruments have less power, these coefficients are less precise and less robust than the benchmark. Nonetheless, they provide additional evidence in support of the exclusion restriction.

9. Conclusion

This paper explored the effect of unionization on strike outcomes in the United States in the early 1880s. To identify causal effects, I constructed an instrument from the location of the assemblies of the Knights of Labor. Organized strikers were significantly more successful than wildcat strikers: union sponsorship increased the probability of success of

[45] Assemblies in the locality in question are ignored. Assemblies are also ignored if they were founded after an assembly was established in the locality.

TABLE 9: ROBUSTNESS OF THE UNION EFFECT TO ALTERNATIVE INSTRUMENTS

	(1)	(2)	(3)	(4)
Union effect				
Two-stage least squares	0.310***	0.281	0.409	0.441
	(0.115)	(0.203)	(0.295)	(0.409)
Ordinary least squares	0.122***	0.111***	0.122***	0.111***
	(0.026)	(0.025)	(0.026)	(0.025)
Fit				
Hausman m-statistic	1.813	0.868	0.989	0.816
First-stage F-statistic	39.469	9.967	9.823	6.781
Adjusted R^2 (OLS)	0.014	0.016	0.014	0.016
Controls				
KOL presence in year of strike		×		×
Instrument				
KOL presence in 1880	×	×		
Distance to nearest assembly in 1880 (log)			×	×
Sample				
Sample size	5363	5363	5363	5363
Parameters	2	3	2	3

Notes: See Section 3 for information about the data. Standard errors, in parentheses, are robust to correlation across time and space (see Subsection 5.2). The m-statistic tests the difference between estimates of the union effect (Hausman, 1978). The first-stage F-statistic tests the effect of the instrument on the treatment in a linear specification.
Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent.

a strike by 32 percentage points from a baseline rate of 38 percent. This result rationalizes unions' leading role in collective bargaining in the postwar period. Organization reduced the probability of job loss as well. On the other hand, I found no effect on the payoff of successful walkouts.

Because wildcat stoppages are so few today, strike theory has not paid much attention to the interaction between unions and workers.[46] Most models assume pairwise bargaining between a firm and a union. Empiricists evaluate their predictions about duration or the impact of aggregate shocks (Card, 1990). This paper provides theorists with additional empirical evidence. Organization is a twofold shock: it lowers the cost of a standoff to workers (through financial assistance, etc.) and increases its cost to firms (by reducing strikebreaking, etc.). My results are consistent with an attrition model (Card and Olson, 1995; Geraghty and Wiseman, 2008; Kennan and Wilson, 1989). In this framework, firms and workers dispute a known indivisible surplus.[47] They pay a fixed delay cost per period of stoppage. They know their own costs, but not each other's. This model captures the effect of organization on the success rate as well as the lack of an effect on the payoff of successful strikes or duration.

This paper sheds new light on the American labor movement in the 1880s. This decade saw an unprecedented experiment in radical mass unionism under the aegis of the KOL. The KOL entered rapid decline in 1886, which entrenched conservative craft unionism and the American Federation of Labor, whereas radical inclusive unions rebounded from similar setbacks in Europe at the end of the century. This divergence is a topic of ongoing debate. Recent research has emphasized environmental constraints in the U.S. (Ansell and Joseph, 1998; Friedman, 1988; Kaufman, 2001; Voss, 1993). I find evidence that organized workers undertook riskier confrontations than the unorganized, which indicates a discipline problem within unions. Kremer and Olken (2009) make a similar point in the context of an evolutionary model of unionization. They argue that democratic unions are evolutionarily disadvantaged because they focus on maximizing members' welfare instead of their own survival. The Knights of Labor were a loose federation of nearly autonomous assemblies, whereas the American Federation of Labor centralized power. Greater discipline may therefore elucidate the triumph of craft unionism in the U.S.

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[46] Exceptions include Ashenfelter and Johnson (1969) and Olofsgård (2012).
[47] The surplus need not be indivisible: see the behavioral model of Abreu and Gul (2000), for example.

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A. Sample construction

The *Third Report* has 5809 rows. I exclude lockouts (358 rows), unfinished strikes (4 rows), general strikes (48 rows) and strikes in imprecise localities (36 rows). The general strikes are: the strike of the Amalgamated Association of Iron and Steel Workers of 1882, the nationwide strike of the Brotherhood of Telegraphers of 1883 and the Great Southwest Railroad Strike of 1886. The telegraphers' strike appears as one full row (in New York) and 44 empty rows (in other states). The imprecise localities are: Jersey Meadows (1 row), Hocking Valley (3 rows) and Western Pennsylvania (28 rows).

I follow these definitions in constructing covariates from the *Third Report* and the *Tenth Census*:

- *Strikes over pay:* mostly for a wage raise (70 percent) or against a wage cut (22 percent), but also over effective compensation (change of screen, payment in script, etc.).
- *Generic employees:* "employés", laborers or helping hands (labels from the *Third Report*).
- *Fragmentation index:* $100 \times (1 - \sum_i s_i^2)/(1 - 1/N)$, where s_i is the share of group i in the industrial workforce and N is the number of groups. Following census reports, I distinguish eight ethnic groups: Black American, White American, British, Canadian, Irish, German, Nordic and other. (German includes Austrians and the Swiss.) I use the trade classification of the *Tenth Census*.
- *Incidence of unemployment:* the percentage of workers that experienced at least one month of unemployment in the twelve months before the census.
- *Successful strike in previous year:* the occurrence of a successful strike in the same sector and county in the preceding year.

I construct two variables from the tables of the census of manufactures: the average establishment size and the average daily wage.[48] Because the census reports more establishments than employees for a few counties, I add one employee to each establishment before taking averages. If there were fewer than five establishments in a county, I substitute state figures. To compute railways per square kilometer, I calculate the land area of each county from boundary files (Manson et al., 2018).

I divide the sample into seven periods. The first five are yearly (1881–85). Following Card and Olson (1995), I divide the eight-hour campaign of 1886 into two stages: January to April (buildup) and May to December (fallout). I aggregate states into six regions. Four correspond to the definitions of the Bureau of Economic Analysis (BEA): the Great Lakes, the Mideast, New England and the Plains. The South includes the BEA region of the same name, Oklahoma and Texas. The West combines two BEA regions, Far West and Rocky Mountain, Arizona and New Mexico. I aggregate industries into six sectors: mining and quarrying (coal, ice, metal and stone), construction (building trades, public ways and public works), food, drink and tobacco (agriculture, food, drink and tobacco), light manufacturing (ceramics, clothing, leather, paper, printing, rubber, textiles, wood and other manufacturing), heavy manufacturing (chemicals, coke, gas, machinery, metals and transportation equipment) and services (communications, government, services, trade and transportation).

B. Variance of weighting estimators

My parametric implementation of the weighting estimators of Hirano, Imbens and Ridder (2003) and Frölich and Melly (2013) are asymptotically normally distributed by Theorem 6.1 of Newey and McFadden (1994). Their limit variances take the form $\mathrm{E}(v_{ij}\boldsymbol{h}_i\boldsymbol{h}_j^\top)$ for some weights v_{ij} and some vector function $\boldsymbol{h}_i$. The weights v_{ij} capture the residual correlation between observations.

I use the notation of Section 5. Since the weights of Frölich and Melly (2013) simplify to inverse probability weighting when z_i is d_i, I focus on the more general case. Let $\Lambda(\cdot) \equiv \exp(\cdot)/[1+\exp(\cdot)]$ be the logistic function. Recall that I set $\mathrm{P}(z_i \mid \boldsymbol{x}_i) = \Lambda(\boldsymbol{x}_i^\top\boldsymbol{\gamma})$ for some vector $\boldsymbol{\gamma}$. Define $\boldsymbol{d}_i \equiv (1, d_i)$ and $\boldsymbol{\beta} \equiv (\alpha, \beta)$.

Newey and McFadden (1994) give the formula for $\boldsymbol{h}_i$:

$$\mathrm{E}(w_j\boldsymbol{d}_j\boldsymbol{d}_j^\top)^{-1}\left(w_i\boldsymbol{d}_i(y_i - \boldsymbol{d}_i^\top\boldsymbol{\beta}) - \mathrm{E}\left[\boldsymbol{d}_j(y_j - \boldsymbol{d}_j^\top\boldsymbol{\beta})\,\mathrm{D}_{\boldsymbol{\gamma}^\top} w_j\right]\mathrm{E}(\boldsymbol{x}_j\boldsymbol{x}_j^\top)^{-1}\boldsymbol{x}_i[d_i - \Lambda(\boldsymbol{x}_i^\top\boldsymbol{\gamma})]\right),$$

[48] To compute average daily wage, I divided total yearly wages across manufacturing firms in each county by the number of employees times 300.

where $\mathrm{D}_{\gamma^\top} w_i = -\boldsymbol{x}_i(2d_i - 1)\Lambda(\boldsymbol{x}_i^\top \boldsymbol{\gamma})[1 - \Lambda(\boldsymbol{x}_i^\top \boldsymbol{\gamma})]\left(z_i/\Lambda(\boldsymbol{x}_i^\top \boldsymbol{\gamma})^2 + (1 - z_i)/[1 - \Lambda(\boldsymbol{x}_i^\top \boldsymbol{\gamma})]^2\right)$.

C. Additional results

Table 10 shows linear estimates of the treatment effects in Tables 3 and 4. These estimates are given for completeness, though they are inconsistent. Linear regression yields larger coefficients than the weighting method of Frölich and Melly (2013), partly because it does not restrict outcomes to the unit interval and partly because it is more sensitive to limited overlap (Imbens, 2015). For example, the third specification yields 205 fitted values outside the unit interval (3.82 percent of observations); the fourth, 479 (8.93 percent); the fifth, 625 (13.19 percent). Ordinary least squares and inverse probability weighting give similar estimates of the union effect on success rates. Weighted estimates of KOL effects are more stable across specifications than their linear counterparts.

Table 10: Linear estimates of the effects of KOL presence and union sponsorship

	(1)	(2)	(3)	(4)	(5)
KOL effect on organization (OLS)	0.262***	0.184***	0.111***	0.103***	0.173***
	(0.032)	(0.024)	(0.021)	(0.025)	(0.025)
KOL effect on success rate (OLS)	0.086***	0.078***	0.065***	0.063***	0.087***
	(0.024)	(0.019)	(0.023)	(0.020)	(0.021)
Union effect on success rate (TSLS)	0.318***	0.410***	0.576***	0.704**	0.483***
	(0.096)	(0.106)	(0.183)	(0.303)	(0.128)
Union effect on success rate (OLS)	0.122***	0.100***	0.094***	0.084***	0.087***
	(0.026)	(0.021)	(0.021)	(0.021)	(0.022)
Hausman *m*-statistic	2.307	2.998	2.634	2.054	3.147
Controls					
Period, region & sector		×	×	×	×
Strike characteristics			×		
County characteristics				×	
Past labor conflict					×
Sample					
Sample size	5363	5363	5363	5363	4739
Parameters	2	18	28	26	18

Notes: See Section 3 for information about the data. Column (5) excludes observations from 1881 for lack of strike microdata for 1880. Standard errors, in parentheses, are robust to correlation across time and space (see Subsection 5.2). The *m*-statistic tests the difference between estimates of the union effect (Hausman, 1978).
Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent.

As Subsection 3.1 notes, the unit of observation is ambiguous in the *Third Report*: each row may represent an entire strike or a subset of the affected establishments. Following the literature, I treat each line as an observation. This approach has the advantage of underweighting outliers. Moreover, unions may strategically strike additional establishments to put pressure on recalcitrant employers; therefore, weighting estimates by establishments could introduce endogeneity bias. Nonetheless, Table 11 investigates the sensitivity of my benchmark specification for completeness. For reference, a row may represent up to 1500 establishments, 80 percent represent a single establishment and 99 percent represent 50 or fewer. My findings are qualitatively robust: the union effect is large and downward biased. The coefficients are sensitive to the bound on the weights. Unreported results show that they are more stable if I account for imbalances across periods, regions and sectors. They are then close to the unweighted estimate in Column (2) of Table 4, 0.368.

TABLE 11: SENSITIVITY OF MAIN ESTIMATES TO WEIGHTING BY ESTABLISHMENTS

	(1)	(2)	(3)	(4)	(5)
KOL effect on organization (IPW)	0.262***	0.324***	0.329***	0.333***	0.364***
	(0.032)	(0.035)	(0.047)	(0.050)	(0.053)
KOL effect on success rate (IPW)	0.086***	0.088***	0.138***	0.145***	0.098
	(0.024)	(0.034)	(0.050)	(0.056)	(0.077)
Union effect on success rate (FM)	0.327***	0.273***	0.418***	0.435***	0.269
	(0.095)	(0.104)	(0.149)	(0.165)	(0.214)
Union effect on success rate (IPW)	0.122***	0.162***	0.219***	0.235***	0.183***
	(0.026)	(0.031)	(0.037)	(0.040)	(0.066)
Fit					
Hausman m-statistic	2.436	1.166	1.389	1.267	0.492
First-stage F-statistic	68.462	84.457	49.439	45.108	46.672
Weighting scheme					
Bound on weights	1	10	50	100	
Sum of weights	5,363	10,550	15,612	17,514	21,593

Notes: See Section 3 for information about the data. Standard errors, in parentheses, are robust to correlation across time and space (see Subsection 5.2). The m-statistic tests the difference between estimates of the union effect (Hausman, 1978). The first-stage F-statistic tests the effect of the instrument on the treatment in a linear specification.
Legend: Stars denote significance: *, at the 10 percent level; **, 5 percent; ***, 1 percent. Row labels distinguish estimators: "FM" refers to Frölich and Melly (2013); "IPW", to inverse probability weighting.