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Information Networks and Collective Action: Evidence from the Women's Temperance Crusade[†]

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How do social interactions shape collective action, and how are they mediated by networked information technologies? We answer these questions studying the Temperance Crusade, a wave of anti-liquor protest activity spreading across 29 states between 1873 and *1874. Relying on exogenous variation in network links generated by railroad accidents, we provide causal evidence of social interactions driving the diffusion of the movement, mediated by rail and telegraph information about neighboring activity. Local newspaper coverage of the crusade was a key channel mediating these effects. Using an event-study methodology, we find strong complementarities between rail and telegraph networks in driving the movement's spread.* (*JEL* D83, J16, L92, L96, N31, N41, N71)

To organize and exercise collective action, disenfranchised groups require effective coordination. Effective coordination, in turn, requires information. Do the types of communication technologies available shape how information impacts the ability of groups to solve the collective action problem? Internet-based social media platforms, for example, played a key role in fostering the Arab Spring (Acemoglu, Hassan, and Tahoun 2018; Tufekci and Wilson 2012). They may become ineffective, however, if fake news become prevalent online. Similarly, television and radio were key instruments for the organization of the civil rights movement in the United States (Andrews and Biggs 2006, Morris 1984). Indeed, governments seeking to

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undermine collective action are well aware of the threat posed by access to communication technologies.[1]

In this paper, we contribute to the literatures on social interactions and collective action by studying the female-led Temperance Crusade movement, which swept the United States on a wave of protest activity against liquor dealers between 1873 and 1874. In this context, we study social interactions by estimating how women's decisions to participate in the Temperance Crusade depended upon the collective action decisions of women in neighboring towns. We do this by tracing how information about protest activity was mediated through the railroad and telegraph networks, the two key information transmission technologies in the second half of the nineteenth century.

The Temperance Crusade is an ideal historical setting to investigate these issues for a variety of reasons. Despite the very different technological characteristics of rails and telegraphs, their geographic distribution appears to be closely correlated with the spread of the crusade. Although it was a precursor movement of subsequently organized collective action by women in the United States (together with the Seneca Falls Convention of 1848), the protests in each town were quite parochial in their aims. Besides a core group of leaders focused on spreading the movement across communities, shutting down local bars and saloons was the main aim of the crusading women. Resistance to these protests was also purely local. Moreover, in 1874 women in the United States were still disenfranchised in all states except for Wyoming and Utah. Women's ability to exercise collective action was their only direct source of political power, allowing us to abstract away from alternative channels of political influence as potential omitted variables. The historical setting also restricts the number of potential communication technologies we must consider, justifying our emphasis on railroad and telegraph networks. Finally, we have access to detailed, daily variation in the occurrence and type of crusade-related events, and daily variation in railroad accidents. Crucially for our empirical strategy, the time-series variation in these disruptions to the information transmission infrastructure is likely unrelated to other shocks driving women's collective action decisions. Moreover, our detailed knowledge of the geographic diffusion of the crusade allows us to provide evidence of the importance of local newspapers as channels of information transmission.

A vast literature in economics studies social interactions in the adoption of behaviors and activities. A similarly large body of work in the social sciences examines the determinants of collective action. We bring these areas of inquiry together by providing causal evidence of social interactions in a collective action setting, and by tracing the roles that alternative communication technologies play in allowing groups to aggregate and use information. We do so using a variety of methodological approaches. Estimating whether crusade-related protests in a community had a causal effect on the subsequent crusading decisions of neighboring communities presents an array of empirical challenges first emphasized by Manski (1993). The potential for unobserved correlated effects is particularly serious in our context

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[1] In his *History of the Russian Revolution*, for example, Trotsky points out that a priority of the Revolutionary government was to control all forms of communication technologies: "The soviet seized all the post and telegraph bureaus, the wireless, all the Petrograd railroad stations, all the printing establishments, so that without its permission it was impossible to send a telegram, to leave Petrograd, or to print an appeal." (Trotsky 2008 [1932], p. 120)

because towns are embedded in several communication networks.[2] We tackle this challenge with a panel instrumental variables (IV) strategy relying on exogenous variation in network links caused by railroad accidents during the months of the crusade. To the best of our knowledge, this is the first observational study to identify social interaction effects exploiting exogenous variation in whether links in an underlying network are active or not over time.[3] Related exercises in the context of market integration are the work by Koudjis (2016), who uses weather disruptions in the English Channel to identify the effect of information on stock prices in eighteenth-century Amsterdam, Steinwender (2018), who exploits the establishment of the transatlantic telegraph cable in 1866 to measure information frictions in the cotton trade, and Jensen (2007), who tracks how the introduction of mobile phones in Kerala reduced price dispersion and increased allocative efficiency in the fisheries industry. These studies, however, abstract away from network considerations.

Our IV strategy allows us to separately identify the effects of information transmission along railroad and telegraph networks on crusading activity.[4] After establishing the central role played by railroad and telegraph-mediated information flows in the diffusion of the crusade, we ask whether these information technologies complemented or substituted each other in fostering the crusade's spread. To do so, we rely on an event-study methodology that exploits cross-sectional variation in access to railroad and telegraph networks across towns. Studying short time windows after women in a town have undertaken a protest, we compare the relative likelihood of subsequent Temperance Crusade events between neighboring towns that vary in their rail and telegraph access within narrow spatial clusters. The time-series variation in protest activity allows us to control for spatially correlated and town-specific unobservables, making the comparison of towns with varying types of network access quite reasonable.

We find positive and precisely estimated average social interaction effects mediated through the railroad network. During the phase of the fastest spread of the crusade, one additional crusade event among neighboring towns linked by rail led to a sixfold increase in the probability of holding a crusade event in the following ten days. Our estimates for the average effect of information transmission through the telegraph network are larger. Consistent with the higher efficiency of information transmission along railroads and telegraph lines, our findings indicate that alternative channels of information transmission had delayed effects on neighboring protest activity. We then find a strong complementarity between railroad and telegraph networks: telegraph connections boosted the responsiveness of neighboring towns when railroad links were present. Conditional on a neighboring town experiencing

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[2] In a setting with simultaneous networks, information flows traveling along one unmodeled network would constitute correlated unobservables when estimating the effects of information traveling along other observed networks.

[3] Discussing the challenges of identification in social network settings, Breza (2016) argues: "Because the social network encodes patterns of interactions of individuals in real life, it is often extremely hard, if not impossible to find sources of exogenous variation in network structure … The possibility of using exogenous network change to better understand causal links between network shape and other real outcomes is exciting. However, such an exercise would require that the underlying change to the network not be directly correlated with the outcome of interest …" (p. 22).

[4] Incidentally, our methodology illustrates that in a context with parallel communication networks, as long as there is sufficient overlap of nodes across them, an exogenous source of variation in link activity of just one of the networks is sufficient to identify the effects along the different networks.

a crusade event, the average probability of holding a crusade event in the following two weeks was 10 percentage points larger for towns with both a rail link and telegraph access compared to towns with only one of the technologies. These results are very precisely estimated, and highlight the importance of network complementarities in social learning and adoption settings. Moreover, as would be expected in a networked information-transmission setting, we find a clear pattern of decay in the effectiveness of signals over increasing distances.

We also provide an array of robustness exercises and tests of the validity of our empirical strategy, including specification tests and placebo exercises. Our results are very similar when we vary the way in which we construct our railroad link instruments, when we vary the number of days within periods in our panel, when we vary the lag structure of our models, and when we vary the definition of a link in the railroad network. Additionally, we show that the first stage relationship between railroad accidents and neighboring protest activity is very strong, and robust to the use of alternative subsets of instruments. Results are similar when we use generalized method of moments (GMM) instead of IV. In settings with multiple endogenous regressors and overidentification, assessing the presence of weak instruments is challenging. Thus, we also compute weak instrument-robust Anderson-Rubin-type confidence intervals that fully agree with our main results, and suggest our instruments are strong. The difficulties inherent in collecting historical information raise the possibility that our data on crusade activity may be incomplete, and that this form of measurement error may be correlated with network access, posing a serious empirical challenge. We explore this possibility in detail. Besides undertaking an exhaustive verification of the original sources of our data, we rely on a battery of empirical strategies exploiting newspaper coverage of the crusade to bound the plausible extent of misreporting, and its consequences for our estimates. Overall, we find little evidence of systematic misreporting.

Our results contribute to the literature on social interactions in adoption settings (Bikhchandani, Hirshleifer, and Welch 1992; Banerjee 1992).[5] Parallel to these theoretical contributions, there is a growing empirical literature interested in identifying social interactions in the adoption of behaviors, from hybrid corn adoption (Griliches 1957) to bank panics (Kelly and Ó Gráda 2000) and physician drug prescription practices (Iyengar, Van den Bulte, and Valente 2011). Economists have been keen on studying social learning, for example in the contexts of technology adoption by farmers (Foster and Rosenzweig 1995, Conley and Udry 2010, Bandiera and Rasul 2006), movie attendance (Moretti 2011), or voting in primaries (Knight and Schiff 2010). Experimental studies such as Kremer and Miguel (2007) and Duflo and Saez (2003) also have studied social learning about deworming medicine uptake or retirement savings decisions, just to name a few. Distinguishing social learning from other forms of social interactions such as contagion or imitation requires observing both adoption decisions and outcomes (Young 2009). We do not

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[5] These models allow for informational cascades and inefficient herding despite Bayesian behavior. More recent theoretical contributions to social learning focus on agents interacting in networks, establishing the relationship between network topology and long-run learning under various behavioral assumptions (Bala and Goyal 1998, Golub and Jackson 2010). These models extend the popular DeGroot-type models where agents are embedded in networks but aggregate information in simple, non-Bayesian ways (DeGroot 1974). For a recent experimental application distinguishing social learning from endorsement, see Banerjee et al. (2013).

observe the outcomes of protest activity at the local level—e.g., whether crusaders managed to close the saloons in their towns. We do observe, however, different collective action events, and information about them moving along different communication networks. This allows us to provide suggestive evidence of social learning by exploiting differences in the kinds of protest activities adopted by crusading women in towns with differential access to telegraph and rail connections.

Our paper also relates to a literature studying the diffusion of behaviors in online social networks (Aral, Muchnik, and Sundararajan 2009; Aral and Walker 2012; Gruhl et al. 2004; Lerman, Ghosh, and Surachawala 2012; Bakshy et al. 2012). These papers rely on observational and experimental data to document contagion in a variety of online activities such as news consumption or app adoption, made possible by their ability to map the social networks and trace the information flows in detail. In a different and historical setting, we are able to undertake a similar exercise by mapping the railroad and telegraph networks, and by observing each instance of information generation. Conveniently, the relatively short time span of the Temperance Crusade allows us to take these communication networks as fixed and abstract away from endogenous network formation considerations. This is a major empirical difficulty in online social network studies because correlations in behavior across agents can be driven by selection into friendships.

Our paper also contributes to the literature on collective action and political mobilization. Beginning with Olson (1965), early work by political scientists emphasized characteristics such as group size and group heterogeneity as important determinants of successful collective action. In his classic study on collective action, Tilly (1978) highlights four key dimensions: interests, organization, mobilization, and opportunity. Beginning with Granovetter (1978, 1973), sociologists in turn have emphasized the importance of group identity, social ties, and preferences for conformity in galvanizing collective action. These ideas have been applied to various settings such as worker strikes, the diffusion of trade unions, and political unrest (Biggs 2003, Hedström 1994, Opp and Gern 1993, Gould 1991). Relative to this literature, our results illustrate that even in settings where underlying grievances are present, access to information can be an essential ingredient for protest activity to effectively get organized and take place.

Economists also are increasingly interested in understanding collective action. Leon, Aidt, and Satchell (2020) study the English Swing riots of 1830 to 1831, and emphasize the importance of communication constraints and economic fundamentals as drivers of their diffusion. González (2020) studies how high school classmate networks drove a recent protest movement in Chile. We are unaware, however, of other studies focused on the dynamics of collective action in a setting with competing networks, and on the complementary roles of alternative communication networks in driving protest diffusion. Other recent and related empirical studies estimate how the spatial rollout of new information technologies such as internet-based social media platforms and cell phone coverage impact the likelihood of collective action (Enikolopov, Makarin, and Petrova 2020; Christensen and Garfias 2018; Pierskalla and Hollenbach 2013).[6] In contrast, we directly trace how information transmitted

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[6] Also related is the literature studying the role of social networks such as Twitter in shaping political communication (Halberstam and Knight 2016). Other recent work studying the nature and consequences of collective action and protest activity from a political economy perspective includes Cantoni et al. (2019) and Madestam et al. (2013).

along established communication networks leads to collective action responses, and point out how information flows operating through alternative communication networks can have distinct effects.

The paper proceeds as follows. In Section I we provide a historical overview of the Temperance Crusade, and a discussion of the role of railroads and telegraphs in relation to it. In Section II we describe the data. In Section III we discuss our empirical strategy to identify the effects of network-mediated information flows on protest diffusion. In Section IV we turn to the estimation of technological interaction effects between rails and telegraphs. We conclude in Section V. Online Appendices A–D contain additional results and a more detailed description of our sources and data.

I. Historical Overview of the Temperance Crusade

The Temperance Crusade was striking in the speed and scope of its diffusion. In less than a year, disenfranchised women mobilized and took to the streets in hundreds of towns across 29 states around a single cause: to demand the closure of saloons and liquor stores. Almost 150,000 women joined it, making it one of the largest social movements involving political action in the nineteenth century (Blocker 1985). Other major social movements of this period such as abolition and temperance societies reached larger enrollments, but few engaged in active militant action (Bordin 1981, Tyrrell 1979, Degler 1981).[7] In contrast to other reform movements, the crusade was truly grassroots. More strikingly, it happened two years before Alexander Graham Bell invented the telephone, five years before Thomas Edison invented the light bulb, and when much of the West was still frontier territory.

Historical accounts of the crusade agree that communication technologies were key to its diffusion. By the early 1870s, both the railroad and the telegraph networks had expanded considerably across the United States. The first transcontinental railroad, linking California to the eastern states, had already opened, and close to 45,000 miles of track had been laid (Stover 1999). Trains were by far the main mode of transportation of travelers and freight. The importance of the railroad for the crusade's diffusion was twofold: it allowed for the movement of leaders across towns, and for the flow of newspapers reporting on crusade activities. Contemporary accounts agree on the importance of "visitors, emissaries, missionaries, and delegates" spreading the word. For example, after Dr. Dio Lewis gave the speech on temperance in Fredonia, NY, that led to the first crusade protest, he traveled to three other towns in New York and Ohio, giving speeches that had the same effect. According to Blocker (1985, pp. 11–12), "… the four actions initiated by Lewis became the forerunners of a national women's movement … Lewis provided the initial impetus for the crusade, but other agencies produced its growth from a local incident to a national movement."

Possibly more important than the role of leaders was the role of local newspapers. As studied recently by Gentzkow, Shapiro, and Sinkinson (2011) and noted early

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[7] The crusade also preceded all other Progressive Era female organizations except the women's suffrage movement begun at Seneca Falls in 1848. The Women's Christian Temperance Union (WCTU) was founded in 1874 as a result of the crusade, while the General Federation of Women's Clubs was founded in 1890, the National Congress of Mothers in 1897, the Women's Trade Union League in 1903, and the National Birth Control League in 1910 (Cooney 2005, Schneider and Schneider 1993).

on by de Tocqueville, nineteenth-century printed newspapers were widespread and central to the political and civic culture of the United States: "… the number of periodicals and occasional publications in the United States exceeds all belief …. scarcely any hamlet lacks its newspaper" (de Tocqueville 2003 [1835], p. 215). Less than a month after the first crusade in upstate New York, newspapers in Columbus, Cleveland, Detroit, Minneapolis, New York City, Baltimore, and Newark were already reporting on it. Newspaper reports of protest activity were read out loud and shared during the organizational meetings, where women discussed whether to undertake protests themselves (Blocker 1985). As we will discuss in Section II, our own newspaper search recovered more than four thousand articles on Temperance Crusade activity, many of them reporting on events taking place in distant locations.

Of similar importance for the diffusion of the crusade was the telegraph network, which by then had reached California as well. To a large extent, it operated in lines running parallel to the railways. Rails and telegraph cables did not, however, completely overlap, as we illustrate in Table 1. The table presents the joint distribution of rail and telegraph access across all 15,971 towns in the 1870 US census, the analogous joint distribution for the 802 towns that experienced crusade activity, and the respective conditional probabilities of collective action. The table makes two points. First, the railroad network had much wider coverage: while two thirds of all towns had rail access, only 6 percent of towns had telegraph access. Second, towns with telegraph were very likely to have rail access as well: 87 percent of towns with a telegraph were also in the rail network.

Despite its much smaller geographic scope, the telegraph was much more efficient at information transmission.[8] As a result, it became central to the operation of the newspaper industry, as local newspapers began relying on telegrams to share news with each other. The telegraph industry had been rapidly expanding starting in the 1840s. It also had experienced intense competition. However, after the Civil War, Western Union managed to consolidate a monopoly of the telegraph cables, controlling 37,000 miles of routes and 2,250 offices (Swindler 1946, Thompson 1947).[9] This gave Western Union a strong bargaining position in relation to the newspaper industry, forcing the Associated Press into a collusive agreement with it: while Western Union was to transmit reports of Associated Press member papers only, the Associated Press was to use Western Union lines exclusively.

Besides the importance of the telegraph for the newspaper industry, crusade leaders also relied on direct telegraph communication to coordinate and spread information. Telegrams were key in generating enthusiasm. In her memories, Mother Stewart, one of the most prominent crusade leaders, provides a fascinating account

[8] Although in previous decades railroads and steamboats had generated large improvements in information transmission speeds (see Kaukiainen 2001), the technological superiority of the telegraph became especially true after the invention in the late 1860s of the automatic repeater, which retransmitted incoming telegraph messages onto the next circuit without the intervention of a human operator, and the invention of the duplex cable, which permitted messages to be sent simultaneously over the same wire from opposite ends (Schwarzlose 1990).

[9] The postal system played a key role in newspaper distribution, but increasingly relied on the rail and telegraph networks as these expanded. In fact, the Pony Express was ended in 1861 when the transcontinental telegraph line was completed. Beginning in 1864, railway post offices started to open, linking the postal system to the railroad network (USPS 2012). For this reason, we do not consider the Postal Service as an independent network in our analysis.

Table 1—Joint Distribution of Rail and Telegraph Access across US Towns, c. 1870 and Crusade Activity

		Distribution of towns				Distribution of crusade activity				$\Pr(\text{crusade} \mid \text{R}, \text{T})$	
		T				T				T	
		0	1			0	1			0	1
R	0	5,538	122	R	0	126	3	R	0	0.022	0.024
	1	9,514	797		1	609	64		1	0.064	0.080

Notes: The leftmost table presents the joint distribution of rail and telegraph access across all 15,971 towns in the 1870 US census. The central table reports the distribution of the 802 crusading towns across the support of the joint distribution of rail and telegraph access. The rightmost table presents the corresponding conditional probabilities of Crusade activity across the support of the joint distribution of rail and telegraph access.

of her role in the movement, illustrating the strong complementarities between the telegraph and railroad networks:

> *The Crusaders in Bucyrus were having a peculiarly hard time with the liquor men and their allies, which were not only the low drunkards, but the city Mayor and his officials also. So they wrote me to come to them …, but my friends at home insisted that I must go with them to Cincinnati. I telegraphed I could not go at that time. Rev. Baltzly telegraphed back: It will be very disastrous to us if you do not come now. It was now twelve o'clock, and the train left at one. I ran to Rev. Mr. Hamma for advice. He said go … while he ran to the telegraph office to notify them that I was coming … I sprang in and was driven a half mile to my home … in time for my train. The sisters still insisted that I must return in time to accompany them to Cincinnati next morning.* (Stewart 1890, p. 316)

Compared to men, women's ability to engage in collective action may have been disproportionately bolstered by the expansion of these communication networks precisely because of their more limited access to political parties, media, unions, or workplaces outside the home. While the communications infrastructure was necessary for the spread of the protest movement, in the absence of local grievances, women in crusading towns would have lacked the motivation to engage in the costly and risky collective action that meetings, petitions, and marches entailed.

As part of the broader temperance movement, crusaders—mostly affiliated with Protestant churches—were religiously motivated. As precursors of the Progressive Era reform movement, many also believed that state and community should be involved in promoting moral and social values (Gusfield 1955). And despite some disagreement among crusaders, many supported the women's suffrage movement (Blocker 1989). Historians, however, disagree on their motivations. Epstein (1981) argues that crusaders were middle class women reacting against working class immigrants and their increasing social influence. For Blocker (1985), in contrast, crusaders' main motivations were the private costs they faced from their male relatives' drinking. The rapid growth of the liquor industry in the decade prior to the crusade is consistent with this view. Between 1864 and 1873, the number of liquor dealers registered as federal taxpayers grew from 80 to 200 thousand, a 17 percent annual growth rate well above the 2.6 percent annual population growth rate of the decade. The geographic distribution of the crusade is also consistent with this view. Both alcohol consumption levels and the number of liquor dealers were highest in the Midwest, where more than 75 percent of the protest activity took place. The alcohol markets were smaller in New England and in the South, where liquor restrictions were more common and had been enforced more strongly (Cherrington 1920). The changing political economy of the Midwest may also have motivated women in that region. Starting in the 1860s, enforcement of Prohibition measures weakened. Several states adopted civil-damage legislation (e.g., the Adair law in Ohio) allowing victims to sue alcohol dealers for damages. These statutes were intended as substitutes for prohibitory measures and may have, therefore, increased drinking.

Saloon visits and sit-ins, referred to as marches, were the most radical but not the only forms of collective action. Crusaders also held organizational meetings, often in churches, and sometimes addressed by traveling crusade leaders. Meetings were well documented in the press, and had the purpose of motivating participants, sharing information, and coordinating further action. There is variation across towns in whether meetings took place before militant action was undertaken. There is also variation across towns in whether meetings led to subsequent petitions or rallies; in some towns, the crusade stopped at the meeting stage. Where further action did happen, there is cross-town variation in the time it took the crusaders to move from a meeting to a formal petition or a march. We also observe variation in whether and how long it took crusading women to move from petitions to marches. Blocker (1985), for example, suggests that in towns experiencing petitions but not subsequent marches, opposition was strong and crusaders concluded marching would have been unproductive. Similarly, he argues that isolation from communication networks explains why many towns holding meetings did not move onto rallies.

Despite the rapid spread of the crusade and its significant geographic reach, its effectiveness in permanently closing saloons was short-lived. The protests themselves may have generated backlash from men at the ballot box. In Ohio, the state with the most crusade activity, the Democratic Party—by then the anti-temperance party—made large gains in the 1874 elections. The crusade's most direct consequence was the creation of the WCTU at a convention in 1874. The WCTU would become a key player in the movements leading to both constitutional Prohibition (García-Jimeno 2016) and women's suffrage forty years later (Gusfield 1955).

II. Data Description

In this section we describe our data collection effort and our main sources of information, and provide summary statistics describing the evolution of the crusade.

Temperance Crusade Activity: Jack Blocker's research, described in his book *Give to the Winds Thy Fears: The Women's Temperance Crusade, 1873–1874* (Blocker 1985), is our source for Temperance Crusade activity. Using his files, we recovered the name of every town where an event related to the crusade took place, as well as the nature of these events, classified as *meetings*, *petitions*, or *marches*.[10]

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[10] Blocker (1985) claims it includes a comprehensive record of towns experiencing crusade-related events. Besides his own newspaper and archival research, he used the record of crusades compiled by Annie Wittenmyer, the

Blocker (1985) and contemporary newspaper sources referred to town hall gatherings as meetings. These were often held in churches, where attendees discussed potential further action. Petitions refer to written requests demanding closure of the stores, addressed to local authorities or saloon owners directly.[11] Marches were public demonstrations, often organized in front and inside targeted saloons, involving prayer, singing, and sit-ins. These were wars of attrition between crusaders and store owners, extending over several days in many documented cases.

We observe the type and beginning date of each event. Moreover, we observe that meetings never took place after petitions or marches, and petitions never occurred after marches. Petitions and marches, however, were not always preceded by meetings. Indeed, there is significant variation in the observed histories across towns. As mentioned above, the first Temperance Crusade event took place on December 14, 1873 in Fredonia, NY. The last march in our dataset took place on July 15, 1874. During this 214-day period, 483 towns held a meeting, 264 towns circulated a petition, and 464 towns staged a march. Panel A in Figure 1 describes the number of events at a daily frequency. Diffusion was very slow early on, picking up speed only around 50 days after the first event. The number of incidents peaked after a hundred days into the protest wave.

Railroad and Telegraph Networks: We constructed our railroad and telegraph networks based on the universe of towns in the 1870 US Census, by first geo-referencing each 1870 town using the 2000 census TIGER/Line shapefiles. We matched each town by GPS coordinates, county, and state.[12] Our final dataset contains 15,971 towns. Our railroad network data comes from Jeremy Atack's archive at Vanderbilt University. We use his 1870 ArcGIS shapefile, which covers all rail lines in the continental United States as of 1870. We represent the railroad network, denoted by $\mathbf{R}$, as an undirected graph, where railroad lines form the edges, and town centroids from the geo-referenced 1870 US census are the vertices. The geographical distances between towns within the railroad line serve as weights. We classify a town to be on the railroad network if its centroid is within ten kilometers (km) of the

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first president of the WCTU, and by historian Susan Dye Lee (Wittenmyer 1878, Lee 1980). Our own newspaper search described below, and a subsequent text analysis of eight books documenting the Temperance Crusade, did not produce any finding of additional crusading towns not already in Blocker's (1985) archive. An important question, however, is whether our dataset does contain all crusade-related events, or whether, to the contrary, our sources missed a subset of events in which case we face misclassification. This issue is particularly concerning in our setting if the likelihood of misclassification depends on other characteristics relevant for the diffusion of collective action, such as network connectivity. In online Appendix B we discuss in detail this possibility and its implications for our findings, and provide evidence suggesting that misclassification in our dataset, if any, is minor. Further, we compute bounds for our main estimates under different assumptions about the extent of misclassification.

[11] This is an example of a petition from the women of Fredonia, NY: "*In the name of God and humanity we make our appeal: Knowing, as we do, that the sale of liquor is the parent of every misery, prolific in all woes in this life and the next, potent alone in evil, blighting every fair hope, desolating families, the chief incentive to crime, these mothers, wives and daughters, representing the moral and religious sentiment of our town, to save the loved members of our households from the strong temptation of drink, … do earnestly request that you will pledge yourself to cease the traffic here in those drinks forthwith and forever …* " (Stewart 1890, p. 87).

[12] We manually verified the cases for which a county had changed its name, or the town became part of a larger settlement. To verify our matching procedure, we used two types of shapefiles: subcounty division and place division. For the observations we were unable to match, we manually verified that the town was not part of a larger metropolitan area, or whether the town had changed counties from 1870 to 2000.

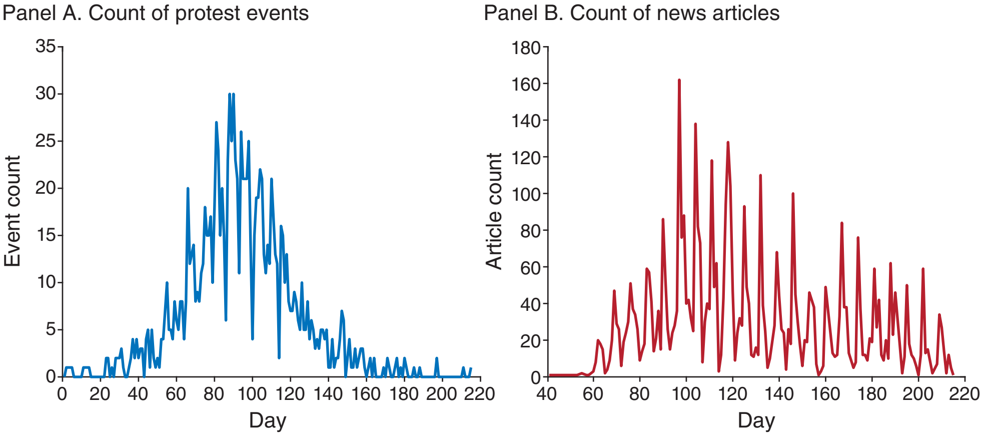

Figure 1. Temperance Crusade Activity and Its Newspaper Coverage, December 1873–July 1874

Notes: Panel A reports the total number of crusade events per day, including meetings, petitions, and marches, based on Blocker (1985). Panel B reports the number of Temperance Crusade-related articles from all newspapers in the *Chronicling America* online newspaper repository of the Library of Congress, based on our text analysis search described in online Appendix D.1, during the period of protest activity.

rail line. Our benchmark rail network classifies a pair of towns as directly linked if they are adjacent along a rail line, and no other towns lie in between.

We next geo-referenced the telegraph network using the 1874 Western Union Telegraph Directory (Western Union 1874). The directory contains maps of US states and territories, depicting the location of telegraph offices and towns with telegraph connections between them.[13] We geo-coded the information in these maps by merging it with the 1870 town-level boundary shape-file. We successfully located 92 percent of telegraph offices from the directory. Using the office coordinates, we constructed the telegraph network as an undirected graph, where each town is a vertex and each telegraph line is an edge. Throughout we denote this network by $\mathbf{\Gamma}$. Figure 2 illustrates our resulting railroad and telegraph networks. The 1870 railroad network from Atack (2013) is on the left, and the 1874 telegraph network from our calculations based on Western Union (1874) is on the right. As expected, the networks are densest in the most populated regions. At a bird's-eye view, the two appear highly correlated, but the figures conceal a great deal of variation across towns at the local level. In our sample, 4.8 percent of towns had access to both railroads and telegraphs, 59.7 percent had access to the railroad only, and 0.7 percent had access to the telegraph only.

Railroad Accidents: Our identification strategy relies on using plausibly exogenous variation in network connectivity across towns over time, induced by railroad-related accidents. We obtained data on railroad accidents from the *Railroad*

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[13] During the period of study, Western Union controlled more than 90 percent of the market share of telegraph communications, making our telegraph network almost completely comprehensive. As an illustration, online Appendix Figure D.2 reproduces the telegraph map for Connecticut and Rhode Island from the directory. Western Union offices are represented by dots, and telegraph cables appear as solid lines.

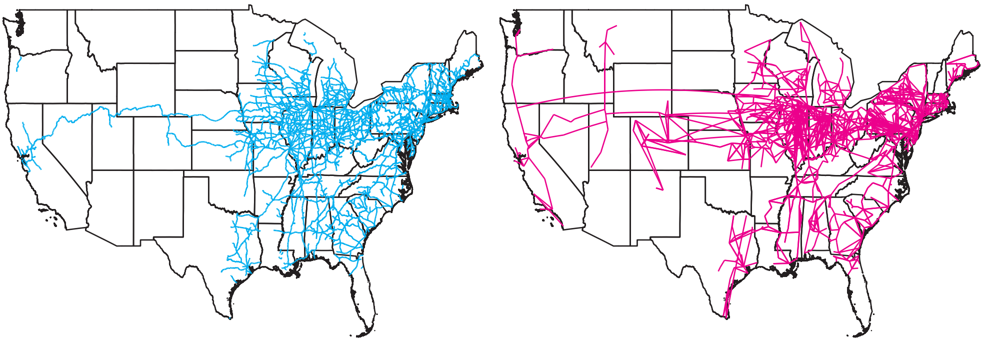

Figure 2. The Railroad and Telegraph Networks, c.1870

Notes: The panel on the left depicts the rail network in 1870, based on Atack (2013). The panel on the right depicts the telegraph network in 1874 based on our own geo-referencing of the maps in Western Union (1874).

Gazette (Wright and Forney 1873–1874), a weekly publishing railroad news about the whole industry. Ideally for our purposes, it includes a monthly compilation of all railroad accidents in the United States, providing details of each accident (explosion, derailment, collision, people involved, its date, and its location). During the period of Temperance Crusade activity, we recorded a total of 471 accidents across the railroad network, an average of 2.2 accidents per day. Panel A of Figure 3 plots the daily count of railroad accidents during the crusade period. We manually matched the location of the accidents to our universe of towns to determine which of them suffered from reduced railroad access over the period of an accident. The *Gazette* is silent about the spatial and temporal extent of the disruption induced by the accidents, requiring us to make some assumptions about which links in the rail network were affected, and for how long. We assume throughout that following an accident, the affected edges remained broken for seven days, and for robustness, compute alternative measures allowing the affected area to include all edges inside either a 50, 80, or 120 km radius from the accident location. Using the disruptions caused by these accidents, we compute a time-varying railroad network $\mathbf{R}_t$. Panel B of Figure 3 plots the "active" number of links in the railroad network at the day level, using the 50 km radius definition for the accidents. It illustrates the substantial time series variation in the network structure induced by these disruptions.

Newspaper Coverage of the Crusade: We also collected data on newspaper coverage of the crusade. This information comes from the *Chronicling America* online newspaper archive of the Library of Congress. Based on a battery of keywords related to the temperance movement, we collected the universe of relevant articles in the years around the beginning and end of the crusade.[14] We used the news article texts to collect data on mentions of specific event types ("meeting," "march," "petition"), by searching for the occurrence of these keywords, as well as the mentions of the towns where the events took place. Our search yielded 4,713

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[14] We used the following keywords: crusade, Dio Lewis, temperance, war on whisky, whisky war, women protest, women's war, ladies league, women movement, and saloon pledge.

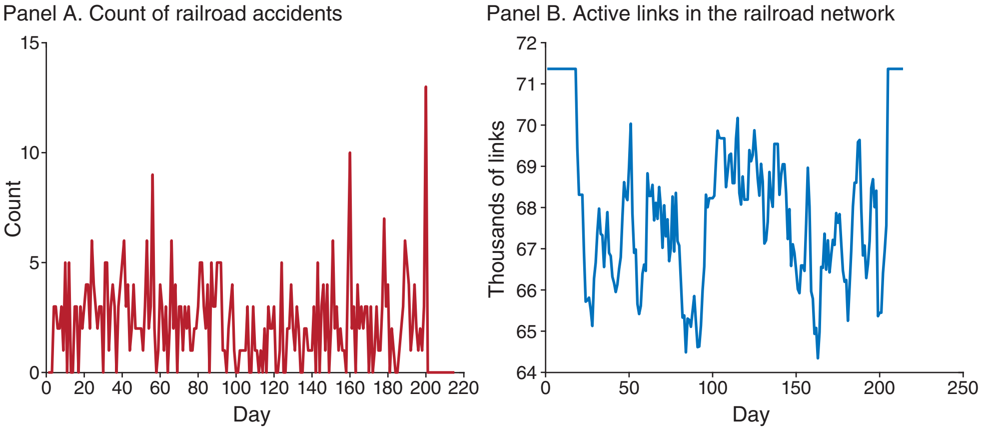

Figure 3. Variation in Railroad Accidents: Number of Accidents and Active Links in the Railroad Network, December 1873–July 1874

Notes: Panel A plots the daily number of railroad accidents in the US rail network. Panel B plots the daily number of active town-to-town links in the railroad network. A link is active if it falls outside a 50 km radius from the accident's location, in a seven-day window. Accidents taken from Wright and Forney (1873–1874) and Vernon (1870).

articles in 190 newspaper titles within the relevant time frame.[15] Panel B in Figure 1 plots the daily number of articles mentioning a crusading event during the relevant period. Comparing it to panel A illustrates the close correlation between protest activity and its newspaper coverage. We organized these data in a network format, coding for each town with a local newspaper, the mentions of other towns' crusade events reported in its newspaper. This allows us to measure both how often a crusading town was mentioned elsewhere, and how much crusade-related information a given town received. We similarly scraped the newspaper archive to recover counts of railroad accident reports, and counts of articles across a variety of other topics. We describe and use these data in online Appendix B to address the possibility of misclassification in our dataset of protest activity. Online Appendix D contains details about the newspaper article search.

Town Characteristics: We collected town-level information from an array of sources. From the 1870 census we obtained demographic information by ethnicity and place of birth, and literacy and school enrollment rates. We collected the female to male ratio and the number of religious sittings (the total seat capacity for each religious denomination) at the county level from the University of Minnesota's *National Historical Geographic Information System*, which we then matched to our universe of 1870 towns. Based on the religious sittings data, we created a Herfindahl index to capture religious heterogeneity. We also collected town-level information on the number of local alcohol vendors (saloons, distillers, wine retailers, wine wholesalers, and breweries) from 46 state business directories covering the years

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[15] For example, a daily from Wayne County, IN, reported on crusade meetings taking place in Shelbyville, Marietta, Waldron, and Fairland: "These meetings and these lectures have done much toward awakening and strengthening a healthy feeling on this important question of Temperance." (*Richmond Palladium*, January 3, 1874, https://lccn.loc.gov/2007618519).

1860–1885. Based on the *N.W. Ayer and Son's American Newspaper* directory, we also collected data on the number of newspapers circulating in each town in 1880. Finally, we collected data on the existence of a US post office in each town. During our period of study, 9,130 towns had a post office.

Online Appendix Table A.1 presents descriptive statistics for these covariates, illustrating the patterns of selection on observables over the course of the crusade. By the end of the protest wave, crusading towns were disproportionately located in the Midwest, significantly less religiously heterogeneous (Herfindahl index of 0.23 compared to 0.27 for non-crusading towns, with a difference of means t-statistic of 11.3), had a much smaller black population share (difference of means t-statistic of 14.1), and were better connected within the railroad network as measured by the betweenness and the degree centrality statistics (t-statistics of -2.5 and -8.3). End-of-crusade differences for the remaining covariates are not statistically significant, although during the first half of the crusade, protesting towns had significantly fewer alcohol vendors per capita. Online Appendix Table A.2 in turn reports summary statistics comparing towns above and below median exposure to railroad accidents. Despite their differences in network centrality, other characteristics are unrelated to railroad accident propensity.

III. Information Technologies and Social Interactions

Our objective is to establish the role played by the main communication technologies of the 1870s, railroads and telegraphs, in mediating the information flows leading to the geographic diffusion of the Temperance Crusade. To do so, we employ several complementary empirical strategies. In this section we rely on a linear model of social interactions, and the exogenous time-series variation in network links induced by railroad accidents, to estimate the effect of information about neighboring crusade events on the likelihood of crusade activity. We find large and precise effects from railroad information flows, and even larger effects from telegraph information flows. We document the importance of newspaper coverage of crusade protest activity as a main channel for these effects.

A. *The Impact of Rail and Telegraph-Mediated Information Flows*

Consider a set of towns $i = 1, 2, \ldots, n$ embedded in several communication networks. At time t each town i is connected by rail to a set $R_t(i)$ of other towns. Because railroad accidents disrupt the network, the set of connected towns changes over time. Similarly, each town is connected by the telegraph to a set $\Gamma(i)$ of other towns. Throughout we will assume that the telegraph network is complete among towns with telegraph access: $\Gamma(i) = \Gamma$ for all towns with telegraph access, and $\Gamma(i) = \varnothing$ for all towns without it. To capture frictions on the information flows that may depend on the length of the links—e.g., longer distances may increase the likelihood that information is degraded, lost, or discounted by its receiver—we will, however, allow for the strength of a link between two towns to depend on distance.[16]

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[16] Our benchmark definition of a link in these network matrices weights them by the inverse distance between each pair of nodes according to $e^{-distance_{ij}}$.

We denote by $\mathbf{R}_t$ and $\boldsymbol{\Gamma}$ the rail and telegraph network matrices. Both are symmetric matrices and their diagonals are zeros. $r_{ij,t} \in [0,1]$ is a typical element of $\mathbf{R}_t$, and $\gamma_{ij} \in [0,1]$ is a typical element of $\boldsymbol{\Gamma}$. Finally, $\mathbf{r}_{i,t}$ and $\boldsymbol{\gamma}_i$ denote the ith row of the rail and telegraph network matrices. Information may travel through alternative means, such as roads and waterways. These constitute latent networks, through which the same information may flow. We capture these latent networks using the (geodesic) distance matrix of all US towns, and call it $\mathbf{D}$, with typical entry d_{ij}. We denote by $\mathbf{d}_i$ the ith row of the distance network.

Technological differences between rails and telegraph cables may lead to differences in both the speed and the nature of the information flowing through them. As a result, the same signal may have effects on neighboring protest activity at various different frequencies. We allow for lagged effects to capture these delays. The lag structure may also differ across networks. Part of our empirical strategy entails estimating the relevant lag structure. To allow for collective action in some towns to generate informative signals about the prospects for collective action in neighboring towns, we consider the following linear probability specification:

$$(1)\quad a_{i,t} = \sum_{\ell=0}^{L_r} \beta_r^{\ell} \mathbf{r}_{i,t-\ell} \mathbf{a}_{t-\ell-1} + \sum_{\ell=0}^{L_\gamma} \beta_\gamma^{\ell} \boldsymbol{\gamma}_i \mathbf{a}_{t-\ell-1} + \sum_{\ell=0}^{L_d} \beta_d^{\ell} \mathbf{d}_i \mathbf{a}_{t-\ell-1} + \mu_i + \xi_t + \varepsilon_{i,t},$$

where $a_{i,t}$ is an indicator of collective action in town i at time t, and $\mathbf{a}_{t-\ell}$ denotes the column vector of these indicators for all towns at time $t-\ell$. Equation (1) allows for up to L_r lags of rail signals, L_γ lags of telegraph signals, and L_d lags of other latent networks, to induce crusade activity in town i. The μ_i are town fixed effects, capturing all time-invariant unobservables that may make women in a given town more or less prone to collective action. The ξ_t are time fixed effects, capturing time-varying shocks affecting all towns in a given period. In practice, this will capture the aggregate time path of the crusade we illustrated in Figure 1. Finally, the $\varepsilon_{i,t}$ captures time-varying unobservables relevant for the collective action decisions of women, possibly including a lagged dependent variable. There is also a potential for interaction effects between networks if, for example, rail-mediated information is useful for protesters especially when additional telegraph-mediated information arrives. Unfortunately, the structure of the telegraph network in place does not allow recovering such technological interaction effects within this estimation framework.[17] As such, we delay our discussion of technological interactions to Section IV.

Thanks to the panel structure of our data, the usual reflection problem common in the estimation of social interactions is not a concern in our setting (Manski 1993). Another recurrent empirical concern in the peer effects literature is the endogeneity of the network structure itself. When links in a network are created based on characteristics that are also correlated with the behavior under study, it is hard to assess whether a correlation in behavior across linked agents is the result of a social

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[17] The spatial distribution of telegraph stations across US towns at the time was highly negatively correlated. Neighboring towns of a town with a telegraph were very unlikely to have a telegraph station themselves. Telegraph companies explicitly followed a strategy that located telegraph stations far apart from each other. As a result, there are very few pairs of towns directly linked by the railroad *and* with access to the telegraph network. Online Appendix Figure A.1 illustrates why our observed network structure does not allow for the identification of interaction effects.

interaction effect, or simply of selection into the friendship. In our setting, unless rails and telegraph cables were laid as a function of the similarity of neighboring towns along characteristics relevant for collective action, this concern will be minor. Population size is important for collective action, and the geographic distribution of both networks is strongly correlated with population density. Our ability to include town-level fixed effects thanks to the panel structure of our data, however, would require that the effect of population size on protest activity be time-varying for this to be a concern. As such, we treat all networks as predetermined. The short duration of the crusade makes this assumption quite reasonable. In a robustness exercise, however, we explore heterogeneity of our social interaction effects along the population gradient.

Estimation of the coefficients in equation (1) is fraught with other econometric challenges. The possibility of persistent unobserved characteristics correlated with the collective action choices of neighboring towns, and relevant for the collective action decisions of women in town i, is particularly serious. This is most obviously the case if we consider the existence of a latent network (roads, waterways, etc.) through which information about *the same* neighboring actions also flows. Were we to leave $\mathbf{d}_i \mathbf{a}_{t-\ell-1}$ inside the error term, even an instrument that generates exogenous variation in information flows $\mathbf{a}_{t-\ell-1}$ would be invalid in equation (1). In a setting with multiple networks transmitting correlated information, an instrumental variables strategy will not be useful if a subset of the networks is left as latent. To our knowledge, this econometric challenge has not been highlighted before. We explicitly include the "distance" network in our econometric specification, as a way to capture alternative channels of information transmission, but go further explicitly allowing for other communication networks as robustness exercises. Even if we can control for all relevant communication networks, residual sources of correlation across neighboring towns that make network information flows endogenous at all relevant lags remain a concern.

Exclusion Restrictions and Identification.—Our strategy to deal with these issues relies on disruptions of the railroad network caused by railroad accidents happening during the months of the crusade. Using this information, we can consider the rail network as time varying, with each link being switched on or off depending on these events. We code $r_{ij,t} = 0$ if despite there being a rail connection between towns i and j, an accident affecting towns i or j took place at time t. Our key identifying assumption is that $\operatorname{cov}\left(r_{ik,s}, \varepsilon_{i,t} \,|\, \mu_i\right) = 0$ for all neighboring towns (i, k) and adjacent time period pairs (s, t). We believe this exclusion restriction is reasonable in our context: disruptions caused by accidents are unlikely to predict time-varying unobservables relevant to the crusaders' protest decisions. The identifying assumption is especially plausible because for a large fraction of our sample, accidents affecting a given pair of towns took place relatively far from the pair. We rely on this assumption to construct valid instruments for all the endogenous variables in equation (1).[18] Breaks in

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[18] A concern in network settings is spatial correlation of the instrument, in which case its cross-sectional variation may pick up some of the variation in the spatially correlated unobservables (see Acemoglu, García-Jimeno, and Robinson 2015). In our setting, railroad accidents affected neighboring towns. Our empirical strategy, however, does not use the cross-sectional variation in railroad disruptions. It only exploits the time-series variation within towns.

the operation of the rail lines affect information flows in two main ways: first, they can reduce the likelihood that a given piece of information generated in town i will reach town j. This can happen either because newspaper circulation across towns is disrupted, or because travelers with information cannot reach their destination. Second, if sufficiently salient or newsworthy, the accidents can reduce the likelihood that news sources or protesters in town j pay attention to crusade-related news originating in town i. As such, we expect (and find) these instruments to be strong predictors of protest activity in neighboring towns.

Consider first $\mathbf{r}_{i,t}\mathbf{a}_{t-1}$, the weighted sum of crusade events of town i's railroad neighbors one period earlier. It varies both because the set of effective rail neighbors of town i, $R_t(i)$, varies *exogenously* over time as railroad accidents take place, and because $\mathbf{a}_{t-1}$ varies *endogenously* over time and across i's railroad neighbors $j \in R_t(i)$. If equation (1) applies for any town, then $a_{j,t-1}$ varies exogenously because the set of effective rail neighbors of town j, $R_{t-1}(j)$, is varying over time. This provides us with a number of valid instruments for $\mathbf{r}_{i,t}\mathbf{a}_{t-1}$: (i) the sum of town i's active railroad links themselves: $\mathbf{r}_{i,t}\boldsymbol{\iota} = \sum_{j \in R_t(i)} r_{ij,t}$;[19] (ii) the sum across i's neighbors, of each of their active railroad links in the previous period: $\mathbf{r}_{it}\mathbf{R}_{t-1}\boldsymbol{\iota} = \sum_{j \in R_t(i)} r_{ij,t} \sum_{k \in R_{t-1}(j)} r_{jk,t-1}$; (iii) following the same idea one neighbor away, the sum across i's active railroad neighbors, of the sum across each of their active railroad links in the previous period, of the sum across each of their active rail links in the period before that: $\mathbf{r}_{it}\mathbf{R}_{t-1}\mathbf{R}_{t-2}\boldsymbol{\iota} = \sum_{j \in R_t(i)} r_{ij,t} \sum_{k \in R_{t-1}(j)} r_{jk,t-1} \sum_{q \in R_{t-2}(k)} r_{kq,t-2}$.

We can construct instruments for the telegraph network and for the distance network information flows following the same idea.[20] For telegraph information flows we use the rail link variation of telegraph neighbors, and the rail link variation of rail neighbors of own telegraph neighbors. Our instruments for $\gamma_i\mathbf{a}_{t-1}$ are thus (i) $\gamma_i\mathbf{R}_{t-1}\boldsymbol{\iota} = \sum_{j \in \Gamma(i)} \gamma_{ij} \sum_{k \in R_{t-1}(j)} r_{jk,t-1}$, and (ii) $\gamma_i\mathbf{R}_{t-1}\mathbf{R}_{t-2}\boldsymbol{\iota} = \sum_{j \in \Gamma(i)} \gamma_{ij} \sum_{k \in R_{t-1}(j)} r_{jk,t-1} \sum_{q \in R_{t-2}(k)} r_{kq,t-2}$. For latent network information flows we use the rail link variation of distance neighbors, and the rail link variation of rail neighbors of own distance neighbors. Our instruments for $\mathbf{d}_i\mathbf{a}_{t-1}$ are thus (i) $\mathbf{d}_i\mathbf{R}_{t-1}\boldsymbol{\iota} = \sum_{j \in D(i)} d_{ij} \sum_{k \in R_{t-1}(j)} r_{jk,t-1}$, and (ii) $\mathbf{d}_i\mathbf{R}_{t-1}\mathbf{R}_{t-2}\boldsymbol{\iota} = \sum_{j \in D(i)} d_{ij} \sum_{k \in R_{t-1}(j)} r_{jk,t-1} \sum_{q \in R_{t-2}(k)} r_{kq,t-2}$. Lags of each of these instruments will be valid instruments for the corresponding lags of the endogenous regressors in equation (1).

Model Selection and Specification.—Our first question relates to the relevant lag structure of equation (1). Information may travel at different speeds along different communication networks. Although telegraph-mediated information flows travel faster than flows along other networks, the nature of such information may also be different, and may thus matter at different frequencies. The distance network, on the other hand, is intended to capture communication taking place, foremost, along roads and rivers. We expect information traveling along these alternative networks to be the slowest.

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[19] $\boldsymbol{\iota}$ represents a column vector of ones. In practice any weighted average of neighbors' active rail links can be used as a valid instrument.

[20] Thus, we do not need exogenous variation in active links along those networks.

Our empirical strategy begins with a formal model-selection statistical test to find the lag structure that most closely approximates the relevant frequencies at which information affected protest diffusion. We rely on Andrews and Lu (2001), who propose a model selection test for panel data and GMM estimation, ideally suited to our setting. The test is based on the *J* statistic for overidentifying restrictions, and incorporates a degrees-of-freedom adjustment that takes into account varying degrees of overidentification across the models being compared. When we estimate a model with the wrong lag structure, the true lags (or a subset of them) are left in the error term. As a result, the instruments will be correlated with the residuals from such a model, leading to a large *J* statistic. In contrast, in a model with the correct lag structure, valid instruments will be uncorrelated with the residual, leading to a small value for the *J* statistic. The test selects the model with the smallest test statistic. Thus, this test does not select the best fitting model; rather, it chooses the model minimizing the correlation between instruments and second stage residuals. We compare a large number of alternative lag structures. To conserve space, in online Appendix Table A.3 we present two-stage least squares estimation results of equation (1) for a subset of competing lag structures.[21]

In all of these specifications, we build the estimation sample panel as follows. First, because the responsiveness to information was likely heterogeneous over time as the crusade spread, we focus attention to the period between day 50 and day 150 after the first crusade event took place. As we illustrated in Figure 1, 95 percent of all activity happened in this interval.[22] We then created synthetic time periods of five contiguous days, within which we aggregated all our variables. In a series of robustness checks discussed below, we alter the definition of a period to include either three days or seven days, and consider as well specifications including the full period from the first to the last observed crusade events. In the benchmark specification, we do not distinguish between types of crusade events. The dependent variable is a dummy for whether any type of crusading event took place (meetings, petitions, or marches) within the five-day period. The regressors include any type of neighboring event as well. We instrument each regressor with the corresponding lag of the instruments described above. Finally, based on our knowledge of the structure of the crusade, we eliminate all time periods after a town experienced a march, leading to a

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[21] Inference in network models based on sampled nodes—even if at random—is challenging. In our setting, however, our estimation sample includes the universe of US towns based on the 1870 census. Spatial correlation in unobservables still remains a challenge for inference in network settings. To address this issue, throughout we compute standard errors that allow for contemporaneous correlation in the residuals across railroad neighboring towns in the spirit of Conley (1999), and allow for arbitrary intertemporal correlation in the errors within a town. Defining **X** to be the matrix of regressors, and **Z** the matrix of instruments, the robust network-correlation corrected variance matrix of the IV fixed effects estimator takes the form

$$(\mathbf{X}'\mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{W}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'\mathbf{X}(\mathbf{X}'\mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'\mathbf{X})^{-1},$$

$$\text{where} \quad \mathbf{W} = \sum_{i=1}^{n}\mathbf{Z}_i'\boldsymbol{\varepsilon}_i\boldsymbol{\varepsilon}_i'\mathbf{Z}_i + \sum_t\left(\sum_{i=1}^{n}\sum_{j\in R_t(i)} z_{it}'\varepsilon_{it}\varepsilon_{jt}z_{jt}\right).$$

[22] Any diffusion process is necessarily nonlinear, whereas we are estimating linear models. It is precisely during the very early and the very late phases where we expect the most heterogeneity in the nature of these processes.

slightly unbalanced panel. After such an event had taken place, no further collective action could occur.

Our exercise pointed us to a parsimonious lag structure that dominates all other models based on the Andrews and Lu (2001) test. In column 1 of online Appendix Table A.3 we begin with the simplest specification, including only the first lag of neighboring crusade activity signals along rail, telegraph, and distance networks. Column 2 presents results for a specification that includes lags of order two instead. Neither first nor second lags of distance-mediated neighboring signals have an effect on the likelihood of collective action. The effect of railroad-mediated signals, in contrast, is highly significant and positive, and very similar in magnitude for the first or second lag models. The coefficient for telegraph-mediated signals is larger, more precisely estimated, and also similar across lags. In column 3 we experiment instead with the third lag of distance-mediated signals. It appears highly significant and positive. Lower order lags of this variable show no effect across specifications, either on their own or in tandem with its third lag. Having established this robust result, subsequent columns fix the third lag of the distance-mediated signals, and explore alternative combinations of lags for rail and telegraph-mediated signals. A pattern quickly emerges suggesting the robustness of the first lags of rail and telegraph-mediated signals, either on their own or simultaneously with other lags. In fact, the last row of the table presents the Andrews and Lu (2001) model selection criterion. The model in column 7 has the smallest test statistic (-21.68), not only across all models we report in online Appendix Table A.3 but also among alternative lag structures we do not show to conserve space. The quantitative implications, however, are similar across the different models that include first lags of rail and telegraph signals. Thus, throughout the rest of the paper we focus on the lag specification favored by the Andrews and Lu (2001) model selection criterion. It has a parsimonious structure, including the first lag of rail-mediated signals, the first and second lags of telegraph-mediated signals, and the third lag of distance-mediated signals.

Online Appendix Table A.3 also reports J statistics with their associated p-values for testing the null hypothesis of the joint validity of our instruments. We cannot reject the null hypothesis that our instrument set is valid across all specifications. We additionally report the p-values for the Kleibergen-Paap Wald test statistics, which similarly suggest no evidence of weak instruments in any of the specifications. In the robustness analysis, however, we will delve deeper on the possibility of weak identification. For completeness, online Appendix Table A.4 presents the R^2 and F -statistics for the corresponding first stages of each of the models in online Appendix Table A.3. These statistics are presented, from top to bottom, in the same order as their corresponding endogenous regressor appears in online Appendix Table A.3. Across all specifications, they show we have strong first stages for all lags of our endogenous regressors.

Main Estimates.—In Table 2 we report estimates from the optimally chosen lag structure model. The first three columns present results under the benchmark five-day period specification. Because the period-size choice is arbitrary, the last three columns present analogous results using three-day periods instead. Column 2 presents our main results. Consistent with our initial priors about the nature of information

Table 2—The Effect of Information along the Rail and Telegraph Networks: Causal Estimates

	Any crusade activity a_{it}—meetings, petitions, marches					
	5 days			3 days		
	OLS (1)	IV (2)	GMM (3)	OLS (4)	IV (5)	GMM (6)
First lag rail	0.004	0.037	0.038	0.005	0.032	0.031
$(\mathbf{r}_{i,t}\mathbf{a}_{t-1})$	(0.001)	(0.013)	(0.013)	(0.001)	(0.014)	(0.016)
First lag telegraph	0.014	0.172	0.163	0.008	0.072	0.076
$(\gamma_i\mathbf{a}_{t-1})$	(0.006)	(0.033)	(0.054)	(0.005)	(0.015)	(0.039)
Second lag telegraph	0.018	−0.068	−0.053	0.011	0.020	0.014
$(\gamma_i\mathbf{a}_{t-2})$	(0.005)	(0.031)	(0.058)	(0.004)	(0.024)	(0.034)
Third lag distance	0.001	0.006	0.006	0.001	−0.001	−0.001
$(\mathbf{d}_i\mathbf{a}_{t-3})$	(0.0001)	(0.002)	(0.002)	(0.0001)	(0.001)	(0.001)
Number of towns	15,934	15,934	15,934	15,950	15,950	15,950
Maximum number of periods	16	16	16	30	30	30
Observations	267,247	267,247	267,247	487,548	487,548	487,548

Notes: The table presents panel estimates of equation (1) for the optimally chosen lag structure model (first order lag for the railroad neighbors' crusade events, first and second order lags for the telegraph neighbors' crusade events, and third order lag for the geographic neighbors' crusade events). The dependent variable is an indicator of crusading activity—meetings, petitions, or marches. All models include period fixed effects and town fixed effects. Standard errors in columns 1, 2, 4, and 5 are robust and allow for spatial correlation between neighboring towns along the railroad network. Standard errors in columns 3 and 6 are robust and clustered at the town level. In columns 1–3 a period is defined as a five-day interval. In columns 4–6 a period is defined as a three-day interval. Columns 1 and 4 report ordinary least squares (OLS) estimates, 2 and 5 report IV estimates corresponding to column 7 in online Appendix Table A.3, and 3 and 6 report GMM estimates using the same set of instruments of columns 2 and 5. Instruments are based on a 50 km radius for the railroad accidents.

flows across the different networks, telegraph-mediated effects (0.17, SE $=$ 0.03) are considerably larger in magnitude than rail-mediated effects (0.04, SE $=$ 0.01). In turn, distance-mediated effects operate at a lower frequency, and although statistically significant, are an order of magnitude smaller than rail-mediated effects (0.006, SE $=$ 0.002). Controlling for the first order lag, the coefficient on the second order lag of telegraph-mediated signals is negative and marginally significant.[23] This second lag, however, is not statistically significant in the three-day period model. First lags of rail and telegraph-mediated signals are quantitatively similar across specifications with varying period-length definitions. The effects implied by the coefficients of this model are large. On average, 50 towns experienced crusade activity every five-day period. In a population of more than 15 thousand towns, this implies a mean for the dependent variable of 0.003. With an average of 2.3 rail neighbors, a coefficient of 0.04 on $\mathbf{r}_{i,t}\mathbf{a}_{t-1}$ means that on average, a crusade in a rail-neighboring town happening one to five days before multiplies the likelihood of undertaking collective action by 5.6 $(= 0.04/0.003)(1/2.3)$.

For completeness, columns 1 and 4 report the corresponding OLS results. The coefficients in these specifications are positive and significant for all network effects.

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[23] One possible interpretation is that signals arriving earlier, by their very nature, may contain little information on, for example, the outcome of the neighboring protest activity, leading to optimism. Signals arriving later, in contrast, may contain more detailed information about, for example, the failure of neighboring crusade activity in closing bars and saloons, leading to a pessimistic response. We thank an anonymous referee for suggesting this possibility.

They are, however, smaller than their IV counterparts, and highlight the importance of instrumenting neighboring protest activity.[24] Columns 3 and 6 present GMM results based on moments constructed using the same set of instruments we employ in our IV specification. The magnitude of the GMM estimates for the first lags of rail and telegraph signals is remarkably close to that of our benchmark IV estimates.

Additionally, in online Appendix Table A.5 we present the first stages corresponding to our main specification. The first four columns report results for the specification in column 2 of Table 2, which uses the 50 km link break radius definition. As column 1 reports, variation in own railroad link disruptions $\mathbf{r}_{i,t}\boldsymbol{\iota}$, as expected, is positively correlated with neighboring railroad signals, $\mathbf{r}_{i,t}\mathbf{a}_{t-1}$: in periods with a higher than average number of active links, towns were more likely to receive neighboring information through the railroad $(0.73, \mathrm{SE} = 0.12)$. More active links of rail neighbors of neighbors $(\mathbf{r}_{i,t}\mathbf{R}_{t-1}\boldsymbol{\iota})$ similarly lead to more signals. Moving to columns 2 and 3, we similarly find active rail links of telegraph neighbors to be strong predictors of telegraph-mediated signals: the coefficient on $\boldsymbol{\gamma}_i\mathbf{R}_{t-1}\boldsymbol{\iota}$ in column 2 is 0.57 $(\mathrm{SE} = 0.03)$, while the coefficient on $\boldsymbol{\gamma}_i\mathbf{R}_{t-2}\boldsymbol{\iota}$ in column 3 is 0.92 $(\mathrm{SE} = 0.03)$. Finally column 4 shows that variation in rail links of distance neighbors $\mathbf{d}_{i,t}\mathbf{R}_{t-3}\boldsymbol{\iota}$ is a strong predictor of the third lag of distance-mediated signals $\mathbf{d}_i\mathbf{a}_{t-3}$ $(0.2, \mathrm{SE} = 0.02)$. As a robustness exercise, the last four columns of online Appendix Table A.5 report analogous first stages using instead the 80 km link break radius definition. The patterns of coefficient magnitudes and significance levels are remarkably close to those using the 50 km definition. Together with the IV diagnostics we discussed previously, these results illustrate the overall strength of the IV first stages for all endogenous regressors at the relevant lags.

Specification Tests and Robustness.—We now discuss a battery of robustness checks, placebo exercises, and specification tests probing the sensitivity of our findings. Online Appendix Table A.6 reproduces the model selection exercise from online Appendix Table A.3, but using a panel based on three-day periods instead of our benchmark five-day period definition. Quantitatively and qualitatively, the results point to the same conclusions we derived in online Appendix Table A.3, and suggest that neither our model selection exercise nor the magnitude and significance of our results are driven by our choice of time-period definition.

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[24] A downward bias of OLS is precisely what we would expect in our setting: if the error term in equation (1) contains a lagged dependent variable and crusading activity is *negatively* autocorrelated, then a positive correlation between own and neighboring protest activity will lead to a downward-biased OLS estimator. To illustrate this point, consider a simplified model where only the first lag of railroad network information has an effect, but where a lagged dependent variable is present and left in the error term,

$$a_{i,t} = \beta_r \mathbf{r}_{i,t}\mathbf{a}_{t-1} + \left(\rho a_{i,t-1} + \varepsilon_{i,t}\right) + \mu_i.$$

For simplicity, suppose the lagged dependent variable is the only source of endogeneity of $\mathbf{r}_{i,t}\mathbf{a}_{t-1}$. Then the probability limit of the OLS estimator of this model will be

$$\beta_r^{OLS} = \beta_r + \rho\frac{\operatorname{cov}\left(\mathbf{r}_{i,t}\mathbf{a}_{t-1}, a_{i,t-1}\right)}{\operatorname{var}\left(\mathbf{r}_{i,t}\mathbf{a}_{t-1}\right)},$$

which is smaller than β_r if $\rho < 0$ and the covariance term is positive. In our setting, the within-town autocorrelation in crusading activity is negative, because periods immediately following an event are very unlikely to exhibit an event as well. The average autocorrelation in $a_{i,t}$ across towns is -0.25.

In Table 3 we move on to a more exhaustive set of robustness checks. All models use the optimal lag structure from online Appendix Table A.3 and our benchmark definition of a railroad link break. In columns 1–5 we use the full sample of towns, and vary the radius used for defining railroad accidents when building our instrument set, and the number of days per period in the panel. Columns 1 and 2 use the five-day period definition, but use 80 km and 120 km radii for the instrument construction. Results are unchanged. In columns 3 and 4 we fix the accident radius at 50 km, but vary instead the panel period definition (three or seven-day periods). Despite the very different number of effective periods, coefficients are once again very similar, although the standard error for the railroad network signals is larger in the seven-day model. An alternative concern is whether spatial correlation in the identifying variation from the closest neighboring towns may pick up other correlated unobservables. In column 5 we present results from a "donut" specification for the instruments, using only rail accidents taking place between 30 and 50 km from each town to construct our instruments, thus excluding the variation coming from the closest links. The estimates are once again indistinguishable from our benchmark effects. In column 6 we change the sample, excluding all towns for which there is no within-town variation in any of the instruments. Although this reduces the sample size considerably, results are unchanged. The overidentification test in this case similarly cannot reject the validity of the instrument set.

In these exercises we considered a pair of towns as linked in the railroad network if a rail line goes through them with no other towns in between. The coefficients we recover, thus, measure average effects of information moving along such town pairs. The definition of a link, of course, is arbitrary. Alternatively, in the spirit of how we defined links in the telegraph network, we can consider a pair of towns as linked in the rail network if there exists a path between them along the railroad, regardless of how many towns lie in between. We refer to this as the "fully connected" rail network, and in online Appendix Table A.8 we explore the robustness of our main findings to varying the rail link definition in this way. Because average distances under this definition will be longer, the relevant lag structure may be so as well. Rather than undertaking a lag specification test, we instead present an array of alternative models, all of which are remarkably consistent with our main findings. Column 3, for example, reports a model with third lags of rail and telegraph effects. The rail coefficient is 0.0037 (with SE 0.0006), which is around a tenth of the magnitude of the rail effect under the benchmark direct link definition. In fact, the average distance between linked towns under the fully connected network definition is also close to an order of magnitude larger than under the direct link definition. The telegraph coefficient is 0.14 (SE 0.6), in close alignment with our main telegraph effect—notice the definition of a telegraph link remained unchanged. Even in column 6, where we include all first three lags simultaneously, we estimate the same qualitative pattern.

An additional concern is the possibility of time-varying heterogeneity that a fixed effects strategy cannot account for. Population size may be an important source of heterogeneity in the response to information flows. For example, Esteban and Ray (2001) point out that the direction of the effect of group size on the likelihood of collective action can depend on features of the environment, such as the cost technology or the congestibility of the public good in question. We explore this possibility in the top panel of online Appendix Figure A.2, where we report results

Table 3—The Effect of Information along the Rail and Telegraph Networks: Robustness

	Any crusade activity a_{it}: meetings, petitions, marches					
Subsample:	All towns					Instruments vary
Instrument variation: (accident radius)	80 km	120 km	50 km	50 km	30–50 km	50 km
Period definition:	5 days	5 days	3 days	7 days	5 days	5 days
Panel A. Second stage	(1)	(2)	(3)	(4)	(5)	(6)
First lag rail	0.034	0.044	0.032	0.052	0.035	0.033
($\mathbf{r}_{i,t}\mathbf{a}_{t-1}$)	[0.012]	[0.013]	[0.014]	[0.016]	[0.013]	[0.012]
	(0.016)	(0.020)	(0.015)	(0.020)	(0.012)	(0.013)
First lag telegraph	0.095	0.095	0.072	0.088	0.169	0.146
($\gamma_i\mathbf{a}_{t-1}$)	[0.029]	[0.032]	[0.015]	[0.017]	[0.031]	[0.032]
	(0.051)	(0.049)	(0.041)	(0.033)	(0.061)	(0.056)
Second lag telegraph	−0.020	−0.022	0.020	0.002	−0.073	−0.047
($\gamma_i\mathbf{a}_{t-2}$)	[0.030]	[0.035]	[0.024]	[0.016]	[0.031]	[0.032]
	(0.069)	(0.074)	(0.036)	(0.024)	(0.081)	(0.074)
Third lag distance	0.003	0.004	−0.001	0.002	0.005	0.005
($\mathbf{d}_i\mathbf{a}_{t-3}$)	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Number of towns	15,934	15,934	15,950	15,906	15,934	5,452
Max. number of periods	16	16	30	11	16	16
Observations	267,247	267,247	487,548	188,384	267,247	91,461
Kleibergen-Paap Wald	31.3	25.3	25.2	28.3	176.7	30.3
J-test statistic	2.28	6.98	1.13	4.54	5.19	1.61
J-test *p*-value	0.81	0.222	0.951	0.475	0.393	0.9
Panel B. First stages (F-statistics)						
First lag rail	29.57	23.77	36.64	29.53	36.46	42.07
($\mathbf{r}_{i,t}\mathbf{a}_{t-1}$)	0.000	0.000	0.000	0.000	0.000	0.000
First lag telegraph	93.09	102.75	113.21	84.12	77.90	74.79
($\gamma_i\mathbf{a}_{t-1}$)	0.000	0.000	0.000	0.000	0.000	0.000
Second lag telegraph	70.95	95.22	54.02	104.02	52.40	66.53
($\gamma_i\mathbf{a}_{t-2}$)	0.000	0.000	0.000	0.000	0.000	0.000
Third lag distance	462.11	292.42	93.77	1,067.74	279.39	276.76
($\mathbf{d}_i\mathbf{a}_{t-3}$)	0.000	0.000	0.000	0.000	0.000	0.000

Notes: The table presents panel IV estimates of equation (1) across alternative specifications. The dependent variable is an indicator of crusading activity: meetings, petitions, or marches. All models include period fixed effects and town fixed effects. Standard errors in square brackets are robust and allow for spatial correlation between neighboring towns along the railroad network. Standard errors in parentheses are clustered at the town level. All models use the lag structure identified as optimal by the Andrews and Lu (2001) test in online Appendix Table A.3 (first order lag for the railroad neighbors' crusade events, first and second order lags for the telegraph neighbors' crusade events, and third order lag for the geographic neighbors' crusade events). All columns define rail neighbors as pairs of towns with first order rail links. Columns 1–5 use the full universe of 1870 US census towns. Column 6 restricts the sample to those towns for which at least one instrument varies over time. Column 1 uses the 80 km radius definition of rail accidents for the instruments. Column 2 uses the 120 km radius definition of rail accidents for the instruments. Column 5 uses the 30–50 km radius definition of rail accidents for the instruments. Columns 3–4 and 6 use the benchmark 50 km radius definition of rail accidents for the instruments. Columns 1, 2, 5, and 6 use the benchmark five-day interval period definition. Column 3 uses an alternative three-day interval period definition. Column 4 uses an alternative seven-day interval period definition. Panel B reports the first stage *F*-statistics and *p*-values corresponding to each endogenous regressor in the corresponding column, from top to bottom.

from models including interactions of rail and telegraph-mediated information flows with log population.[25] In panel A we plot the estimated heterogeneous effect of

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[25] We instrument these interaction terms with interactions between the corresponding instruments and log population.

rail-mediated information as a function of log population (in red), comparing it with the estimated effect from our benchmark model (in blue). In panel B we do the same for the effect of telegraph-mediated information. Across the observed range of values for log population in our sample, neither effect can be distinguished from the corresponding benchmark homogeneous effect, lying within the 95 percent confidence intervals for the benchmark effects throughout the full range of variation in log population. Human capital may similarly be a source of heterogeneity in this context. For example, more educated women may be more skeptical about arriving information, their social networks may be denser, or they may exhibit differential newspaper consumption over the life cycle of the movement. The bottom panel of online Appendix Figure A.2 reports analogous results to those in the top panel, this time allowing for heterogeneity in average schooling. While the estimated heterogeneous effect of rail-mediated information has a negative slope, and the estimated heterogeneous effect of telegraph-mediated information has a positive slope, once again, the effects are statistically indistinguishable from the benchmark homogeneous effects (in blue) throughout the range of variation in schooling.

In an alternative robustness exercise reported in online Appendix Table A.10 we estimate our benchmark specification on the full crusade time horizon (from first to last observed crusade events). We present results for alternative time-period definitions and accident radii for the instruments. Across specifications, the estimates for rail and telegraph-mediated effects are very close to our main estimates. If anything, the estimated rail-mediated effects are slightly larger in magnitude. Thus, restricting attention to the period of rapid diffusion (days 50 to 150) makes no difference to our findings, suggesting little time heterogeneity in the social interaction effects we estimate.

As we pointed out at the top of subsection A, we attempt to control for information flows along alternative (latent) networks using what we refer to as the distance network. The imperfect correlation between geographic distances between pairs of towns and links along other relevant communication networks, however, could make the inclusion of the distance-mediated information flows insufficient as a strategy to control for such omitted network effects. One possibility is that communication along the network of waterways, canals, and rivers was an important mediator of crusade-related news. We geo-referenced the 1860 waterways and canals network from Atack, Bateman, and Margo (2007) onto our set of towns (see online Appendix Figure A.4), and estimate our benchmark specification, including various lags of waterway-mediated information flows computed analogously to our main rail, telegraph, and distance-mediated regressors. We instrument these regressors with the rail accident variation of rail neighbors of waterway-connected towns, in direct analogy to our instrument construction for the benchmark network regressors. Columns 2–4 in online Appendix Table A.11 report these results: we estimate statistically significant but quantitatively very small waterway-mediated effects, while the coefficients on rail, telegraph and distance-mediated information flows remain unchanged, suggesting the omission of waterway-mediated information is not a source of omitted variable bias.

Our model restricts attention to estimating effects across direct links between pairs of towns. Our estimates in Table 2, thus, do not include the impact of crusade-related information across pairs of towns connected to each other indirectly through a third mediating town—one town connected to the mediating town via a rail link, the other via a telegraph link.[26] We can consider any such pairs of towns as constituting a "hybrid" network—a piece of information travels partly along the rail, partly along the telegraph. Such "hybrid" network may also constitute a latent omitted variable in our main specification. Only 215 pairs of towns are linked by such hybrid connections, making the variation from rail accidents of neighboring towns insufficient to construct strong instruments for 'hybrid-mediated' effects. Although we cannot instrument for these regressors, we nevertheless include lags of them in our main specification as control variables to assess the robustness of rail and telegraph-mediated effects to their inclusion. Columns 5–7 in online Appendix Table A.11 report these results, once again showing the robustness of our main estimates.

Instrument Validity.—We now explore the validity of our instruments. In online Appendix Table A.12 we compare specifications using alternative subsets of instruments. From left to right, the columns present results for models with increasing degrees of overidentification, concluding in column 7 with our benchmark specification using the full instrument set for comparison. The first column reports an exactly identified model. The next three columns report models with varying combinations of five of the nine instruments (so as to compare models with exactly one degree of overidentification). Both the second stage coefficient estimates and the strength of the first stages are remarkably similar. In column 5 we then present a model with seven instruments—three degrees of overidentification. In this case, we chose to include only one of the instruments corresponding to the first lag of rail mediated information (own rail link breaks). In this case, the first stage for this regressor is somewhat weaker (F-statistic of 8.9), and correspondingly the second stage coefficient is estimated less precisely. However, its magnitude is very similar to the one we recover from all other specifications. Column 5 illustrates the usefulness of second and third order neighbors' rail link variation. The first stages for telegraph and distance-mediated information flows, on the other hand, are very strong regardless of the number of instruments. Column 6 then presents a specification with eight instruments—four degrees of identification. Point estimates are, once again, very similar to those in column 7.

We also explore the possibility of weak instruments. In our setting, this is a serious concern because in an overidentified setting with multiple endogenous regressors, assessing instrument strength is difficult. For this reason, column 1 of online Appendix Table A.12 presents results for an exactly identified model instead, using a subset of four of the nine instruments. The magnitude and significance of all coefficients are remarkably close to those of the main specification, and the corresponding first stages are equally strong.[27] The coefficient on the first lag of rail information is actually slightly larger in this specification (0.05, SE $=$ 0.02). Of course, this is only one possible permutation of four instruments from a set of nine.

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[26] Notice that our main estimates do include the indirect impact of crusade-related information among such pairs of towns, taking place as a result of the increased likelihood of protest activity in the mediating town.

[27] We also report the Kleibergen-Paap Wald test statistic for the exactly identified model and all other models reported in online Appendix Table A.12. For this exactly identified specification, it takes the value of 130.8, quite similar to the corresponding statistic for the overidentified benchmark model in col. 7, 112.3.

To be exhaustive, in online Appendix Table A.13 we report the Kleibergen-Paap Wald test statistics corresponding to each of the 24 exactly identified models possible when using exactly one of the instruments corresponding to each endogenous regressor.[28] Although not strictly theoretically justified, as ours is a setting with multiple endogenous regressors and non-i.i.d. errors, the Kleibergen-Paap Wald statistic appears to be a reliable indicator of instrument weakness even in these settings (see Andrews, Stock, and Sun 2019). The results from this exercise suggest we have strong instruments available for each of the four endogenous regressors. The last 16 exactly identified models from online Appendix Table A.13 show comfortably large values of the Kleibergen-Paap Wald statistic. The exercise additionally illustrates that on its own, the contemporaneous rail-link variation $\mathbf{r}_{i,t}\boldsymbol{\iota}$, is likely a weak instrument (see the top eight rows of online Appendix Table A.13). This highlights the utility of having second and third order lags of the railroad accidents available as instruments. Indeed, the exactly identified model in column 1 of online Appendix Table A.12 uses the second order lag of rail accidents to instrument for $\mathbf{r}_{i,t}\mathbf{a}_{t-1}$.[29]

To assuage any remaining weak-instruments concerns we follow Andrews (2018), who proposes a two-step approach to building Anderson-Rubin type weak-identification robust confidence sets that are valid even in settings with multiple endogenous regressors and non-homoskedastic errors. We report these results in online Appendix Table A.14. The first four columns report confidence sets under the assumption that all four endogenous regressors are weakly identified. The last four columns, alternatively, restrict the possibility of weak identification to the first lags of rail and telegraph regressors.[30] In each case, we report results for the exactly identified model in column 1 of online Appendix Table A.12, and for the overidentified model in column 7 of online Appendix Table A.12. All confidence sets for the first lag of rail, first lag of telegraph, and third lag of distance comfortably exclude zero, are very similar across specifications (suggesting strong identification as Andrews 2018 points out), and contain our main point estimates.

In online Appendix Figure A.3, we conclude presenting results from a placebo exercise to further support the validity of our instruments. We build false instruments, by taking the base railroad network and simulating railroad link breaks at random every day, at the same rate we observe them break in the data. The figure reports results under the 50 km radius definition for rail link breaks. Using our benchmark lag structure identified as optimal, the top panel in the figure reports the distribution of coefficients for rail and telegraph-mediated effects estimated across 500 simulations of randomly generated rail link breaks, under the five-day period

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[28] In a setting with 9 instruments and 4 endogenous regressors there are a total of 9-choose-4 $=$ 126 possible exactly identified models one could estimate. Our identification argument, however, relies on three instruments for the first endogenous regressor, and on two instruments for each of the remaining three endogenous regressors, making a total of $3 \times 2^3 = 24$ relevant models to consider.

[29] To conserve space, we did not report the coefficient estimates of the models in online Appendix Table A.13. The point estimates and standard errors of all of these exactly identified models, including those with small Kleibergen-Paap Wald statistic values, are indistinguishable from those in column 1 of online Appendix Table A.12.

[30] We use test inversion to construct the confidence sets, so projecting a high-dimensional set onto a one-dimensional set leads to some conservativeness. This is less of an issue when projecting from two than from four to one dimensions. The method also requires the choice of a minimal size distortion $\gamma_{\min}$. Throughout we set $\gamma_{\min} = 0.05$.

definition. The bottom panel reports analogous results for the three-day period definition. Across the simulations, the distributions of estimated effects are tightly concentrated around zero. For all effects, the fraction of simulations leading to a statistically significant IV regression coefficient is less than 1 percent.

B. *The Local Newspaper Channel*

We conclude this section presenting some complementary evidence of the importance of the newspaper as a channel of information diffusion of the Temperance Crusade. We rely on our newspaper text analysis described in Section II and in more detail in online Appendix D. We recorded the number of articles reporting a crusade-related event happening in town i, in newspapers from any other towns.[31] Using this variable we perform two predictive exercises. First, on the panel of crusading towns, we explore whether a collective action event in town i at time t is predictive of news reports about it in other towns at future dates. We explore the effects at between 10 and 50 days ahead by using different leads of the dependent variable, after aggregating the 215 days of the crusade into 21 10-day periods for the panel. We report estimates from this exercise in Table 4. These specifications include town and period fixed effects.[32]

The table illustrates that the average town is mentioned 0.16 times more in the ten days following its crusade event compared to the days before the event. It is mentioned 0.3 times more between 10 and 20 days, 0.17 times more between 20 and 30 days, 0.13 times more between 30 and 40 days, and 0.02 times more between 40 and 50 days after its collective action event has occurred. The effects between 10 and 40 days are statistically significant, and overall reveal an inverted U-shaped pattern that peaks at between 10 and 20 days after the event has taken place.

In a second exercise, we look at how the likelihood of a newspaper report about crusade-related events varies with the network path length between the newspaper's hometown and the town experiencing protest activity.[33] We do this on a panel of all newspaper home-town/crusading-town pairs, controlling for network centrality characteristics of both towns, and for the geodesic distance between them. Because each town is a member of several pairs, we can alternatively include newspaper town and crusading town fixed effects. We report these results in online Appendix Table A.15. Columns 1 and 2 report results for models looking at rail path lengths, and columns 3 and 4 report results for models looking at telegraph path lengths. In both cases, distance along the networks reduces the likelihood of a newspaper report, conditional on the physical distance between the town pair. Results are precisely estimated in the fixed effects specifications. A one standard deviation higher number of links along the railroad (42 links) reduces the likelihood of a newspaper report by 0.79 percentage points ($= 0.00019 \times 42 \times 100$), which is close to half

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[31] The *Chronicling America* online newspaper repository from the Library of Congress reports the town to which each newspaper was registered, and this is the information we use. Local newspapers had additional circulation in other towns, but we do not have detailed data on the geographic circulation of local newspapers.

[32] The econometric specification is crusade-related article mentions about town $i_{[t,t+\tau]} = \alpha_i + \beta a_{i,t} + \xi_t + \varepsilon_{i,t}$.

[33] Along each network, we compute the path length between towns i and j as the shortest number of links between both towns (intermediate towns along the rail line and intermediate stations along the telegraph network).

Table 4—Newspaper Coverage of Temperance Crusade Events

Mentions of town i in crusade-related articles of other town newspapers					
Between days:	$[t, t+10)$ (1)	$[t+10, t+20)$ (2)	$[t+20, t+30)$ (3)	$[t+30, t+40)$ (4)	$[t+40, t+50)$ (5)
$a_{i,t}$	0.162	0.306	0.178	0.134	0.024
	(0.081)	(0.100)	(0.058)	(0.057)	(0.040)
R^2	0.43	0.45	0.47	0.49	0.53
Number of towns	802	802	802	802	802
Number of periods	21	20	19	18	17
Observations	16,842	16,040	15,238	14,436	13,634

Notes: The table presents panel regression estimates for the number of articles mentioning town i in a given ten-day interval, across *all* newspapers in the *Chronicling America* online newspaper repository of the Library of Congress, excluding town i newspapers. The explanatory variable measures the number of Temperance Crusade events taking place in town i during time period t. The sample includes all crusading towns. All specifications include town fixed effects and period fixed effects. The dependent variable in column 1 is the contemporaneous number of article mentions. The dependent variable in column 2 is the first lead of the number of article mentions. The dependent variable in column 3 is the second lead of the number of article mentions. The dependent variable in column 4 is the third lead of the number of article mentions. The dependent variable in column 5 is the fourth lead of the number of article mentions. Standard errors are robust and clustered at the town level.

the baseline probability of a newspaper report in the sample. Quantitatively, the effect is similar along the telegraph network.

We conclude with an exercise illustrating a key mechanism through which our instrumental variables operate: the occurrence of nearby railroad accidents leads to delays in newspaper reporting of crusade activity. Using our matrix encoding the newspaper-article mentions of crusade-related news in other towns, we computed the delay in days between a given crusade event taking place in town j, and the appearance of an article reporting on it in the local newspaper of town i. We estimate whether this delay is correlated with rail link breakages affecting towns i or j happening in the interval of time between the date of the rail accident and the date of the newspaper report.[34]

We report two sets of results in Table 5. In the first two columns, the sample includes all pairs of towns, one with a local newspaper, the other experiencing a crusade event. In this sample, the occurrence of a rail accident is associated with a day of additional delay in reporting. In the last two columns, we restrict attention to pairs of towns with at least one having railroad access. For this sample, the occurrence of rail accidents is associated with around 4.5 days of delay in reporting. These results are robust to the inclusion of newspaper-town and crusade event-town fixed effects, and suggest that an important part of the variation in neighboring crusade activity induced by railroad accidents operates through delays in newspaper reporting. The

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[34] More specifically, we estimate regressions of the form

$$delay_{ij} = \beta_0 + \beta_1 break_{ij} + \delta_i + \delta_j + \varepsilon_{ij},$$

where $break_{ij}$ is a dummy variable indicating whether in the interval of time between the accident and the report, either towns i or j experienced rail link breakages. We include newspaper town (δ_i) and crusade town (δ_j) fixed effects, to control for the baseline overall extent to which either i or j are prone to experiencing rail breakages.

Table 5—Newspaper Delays and Rail Accidents

	Delay in reporting of crusade events							
	1-day periods				5-day periods			
	All pairs		Railroad access		All pairs		Railroad access	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Accident	2.45	0.80	7.44	4.67	0.48	0.16	1.52	0.93
	(0.67)	(0.96)	(2.35)	(2.74)	(0.13)	(0.19)	(0.47)	(0.55)
Observations	20,149	20,071	16,390	16,307	20,149	20,071	16,390	16,307
R^2	0.00	0.29	0.00	0.32	0.00	0.29	0.00	0.32
Newspaper town fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Event town fixed effects	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The table presents OLS regression estimates. Columns 1–4 report results using time-window periods of one day. Columns 5–8 report results using time-window periods of five days. The dependent variable is the delay between the occurrence of a crusade event in town i and its report in a newspaper from town j. The explanatory variable is a dummy taking the value of one if any railroad accident affecting either towns i or j happened in the corresponding time window. Even-numbered columns include newspaper town and crusade event town fixed effects.

reader may recall our main results aggregate the day-level information into five-day intervals. The last four columns of Table 5 replicate the first four columns, using a measure of delay similarly aggregated into five-day intervals, and revealing the same pattern of news reporting delay in response to rail accidents. We believe these results strengthen the case for the validity of our IVs. Given the completely different sources of protests and newspaper information, we find these results consistent with the historical literature highlighting the vibrancy of the newspaper industry and of local newspapers as sources of information for the crusade. They also highlight the major role played by newspapers as channels through which the rail and telegraph networks had the effects we identified above.

IV. Estimating Technological Interaction Effects

When several networks are in place, a natural question is whether interaction effects between them are present in driving the diffusion process. In this section we describe and implement an empirical strategy to identify such interaction effects between railroads and telegraphs, and their importance in the context of the Temperance Crusade. Characteristics such as the speed, range, or informational content of the signals flowing along these networks can have different implications over the resulting patterns of social interaction. Our findings here show that the effects of information depend on the technological features of the different communication networks in place, and on their interaction. In particular, access to the telegraph boosted the effectiveness of railroad connections.

A. *A Cluster Event Study Approach*

Were railroads and telegraphs complementary or substitute technologies for the diffusion of the Temperance Crusade? Answering this question is empirically challenging. Towns with and without rail or telegraph access were likely different from each other, particularly along dimensions making them more responsive to information or more prone to collective action. To address this difficulty, we propose a methodology resembling an event study for each collective action event during the crusade, exploiting the variation across towns in rail and telegraph network connections.

We take all towns falling within a geographic radius of each town experiencing an event during the crusade, and observe their collective action responses within a window of time following the event. We then compare the responsiveness of towns with different network characteristics within this geographic cluster, averaging across all event studies. We can control for all unobserved time-invariant town characteristics because each will fall within several event studies. We can control for all unobserved features common to all towns in a given event study cluster as well, because we average across many events. This allows us to compare the response of towns with and without a direct rail link to the signal-generating town, and how this response varies with additional access to the telegraph network. Thus, we exclude towns without railroad access from the analysis.[35]

We construct our clusters for the event study regressions as follows: for every town $i = 1, \ldots, 802$ with a crusade event—the signal-generating town— we draw a circle of radius d from the town's centroid. We then compute the geodesic distance between town i's centroid and all the town centroids in our census dataset. We keep all towns with centroids at a distance d or less from town i—the signal-recipient towns. For every signal-generating town i experiencing a crusade event at time t, we define $G_d(i,t)$ to be the set of all signal-recipient towns j within distance d to it. We also define $F(t)$ to be the set of towns which, by time t, have not yet experienced a march. This is the subset of towns that can still hold collective action events at time t. We denote by $r_{ij} \in \{0,1\}$ a dummy variable equal to one if signal-generating town i and signal-recipient town j have a *direct* railroad connection. We denote by $\gamma_j \in \{0,1\}$ a dummy variable equal to one if town j has access to the telegraph network.[36] Finally, $a_{j[t,t+\tau]}$ denotes a dummy variable equal to one if signal-recipient town j had any collective action event within the time window $[t, t+\tau]$.

For window size τ, and pooling across all event studies, consider the following specification:

$$a_{j[t,t+\tau]} = \beta_1 r_{ij}\gamma_j + \beta_2 r_{ij}(1-\gamma_j) + \beta_3(1-r_{ij})\gamma_j + \beta_4(1-r_{ij})(1-\gamma_j) + \rho d_{ij} + \varepsilon_{ij,t}, \tag{2}$$

for all $j \in G_d(i,t) \cap F(t)$, where d_{ij} is the geographic distance between towns i and j. Based on this specification, one could compute the following quantities of interest: (i) the average effect of telegraph access among towns with rail connection: $\beta_1 - \beta_2$; (ii) the average effect of telegraph access among towns without rail connection: $\beta_3 - \beta_4$; (iii) the average effect of a rail connection among towns with

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[35] Towns not in the 1870 railroad network were very different along most observable characteristics from towns with access to at least one communication network.

[36] In principle one could study as well a richer set of differential responses depending on whether the signal-generating town i had telegraph access or not. We do not estimate such differential effects: among towns experiencing crusade activity—and thus defining event studies—and having telegraph access, only a small fraction of signal-recipient towns j have telegraph access as well. This is a result of the negative spatial correlation in the telegraph network architecture mentioned above. As a consequence, there is not enough variation in the data to estimate such effects.

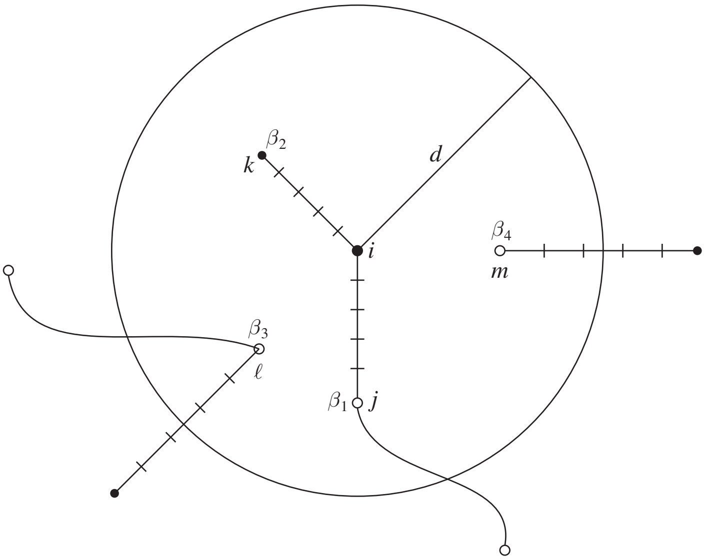

Figure 4. Identification of Technological Interaction Effects under the Cluster Event Study Approach: Illustration

Notes: The figure illustrates the sources of variation we exploit to identify the railroad-telegraph interaction effects. Within a given cluster radius d around a town i experiencing a crusade event at time t, there exist towns j directly linked to i through the railroad and with telegraph access, towns k directly linked to i through the railroad but without telegraph access, towns ℓ with both railroad and telegraph access but not directly linked to i by rail, and towns m with railroad access but without a direct link to i and with no telegraph access.

telegraph access: $\beta_1 - \beta_3$; (iv) the average effect of a rail connection among towns without telegraph access: $\beta_2 - \beta_4$; (v) the differential effect of a rail connection between towns with and without telegraph access: $(\beta_1 - \beta_3) - (\beta_2 - \beta_4)$. This difference of differences is what we refer to as the technological interaction effect between the rail and telegraph networks.[37] Figure 4 illustrates our empirical strategy. In practice, signal-recipient towns either do or do not have a rail link to the signal-generating town, and either do or do not have telegraph access: the first four regressors in equation (2) are perfectly collinear, so we drop $(1 - r_{ij})(1 - \gamma_j)$. The network interaction effect can be recovered as $\beta_1 - \beta_2 - \beta_3$.

In a network effects context, a key confounder is the possibility of an unobserved shock that makes all towns $j, k \in G_d(i,t)$ experience collective action. We can, however, include event-study fixed effects $\delta_{(i,t)}$, comparing signal-recipient towns that vary in their network characteristics. Event-study fixed effects subsume any common shocks to all towns in $G_d(i,t)$. Furthermore, this empirical strategy is also immune to unobservables that affect the likelihood of collective action at the signal-generating

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[37] To be precise, this effect captures, among towns with access to the telegraph, how much larger is the gain in protest event likelihood stemming from a direct rail link to the signal-generating town, compared to the gain among towns without access to the telegraph. The negative spatial correlation in the telegraph network architecture implies signal-generating towns are unlikely to have telegraph access when nearby signal-recipient towns do. So the interaction effect should *not* be interpreted as the differential increase in the likelihood of protest activity resulting from a signal directly arriving through two channels (a telegraph and a rail link). Rather, the differential boost in protest likelihood among towns with telegraph access must be coming from additional indirect communication with third towns.

town i and at the signal-recipient town j because signal-generating towns are not included in the event study defined by their collective action event.

Perhaps more importantly, heterogeneity across towns in their proclivity to collective action can also be correlated with network access. We can partially address this concern controlling for an array of town characteristics potentially relevant for collective action such as religious heterogeneity, access to newspapers or post offices, the female to male ratio, or the number of liquor dealers. Even after controlling for these characteristics, other unobservables remain a concern. However, signal-recipient towns j are members of several different event study clusters $G_d(i,t)$, so we can go further and include town fixed effects ξ_j. In this way, we can control for all time-invariant town unobservables, and all time-varying cluster unobservables. Naturally, we must drop an additional network interaction term in the models where we include town fixed effects. We drop $(1 - r_{ij})\gamma_j$, so we recover the network interaction effect as $\beta_1 - \beta_2$. Finally, a fraction of event studies straddle state boundaries, so we are also able to include state fixed effects in all our specifications.

Main Results.—Table 6 presents our main results, where we do not distinguish between types of crusade events. We fix the cluster radius at $d = 30$ km, but allow for three different time windows following the signal-generating event: $\tau \in \{2\,\text{weeks}, 3\,\text{weeks}, 4\,\text{weeks}\}$. We report standard errors clustered two ways: at the event study level and at the recipient-town level. The row labeled "network interaction" reports our estimate for the network interaction effects, computed as $\beta_1 - \beta_2 - \beta_3$ for the models without town fixed effects (in columns 1–6), and as $\beta_1 - \beta_2$ for the models including town fixed effects (in columns 7–9). The first three columns present results for models without recipient-town fixed effects, for two-, three-, and four-week windows. We do not include any additional covariates besides the distance between signal-generating and signal-recipient towns. The coefficients on $r_{ij}\gamma_j$ and on $(1 - r_{ij})\gamma_j$ are positive and very precisely estimated, while the effect on $r_{ij}(1 - \gamma_j)$ is small and statistically insignificant. The resulting network interaction effect is 0.137 $(\text{SE} = 0.06)$ for the three-week window model.[38]

This estimate, however, may capture the effect of town-level characteristics correlated with network access and important for collective action. The increase in the coefficient magnitude as we increase the window size across columns 1–3 is symptomatic of the presence of such confounders. In columns 4–6 we include the following covariates to address this concern: the native-born and Black shares, the sex ratio, the per capita number of newspapers in circulation, a post office dummy, the religious Herfindahl index, the share of Presbyterians, and log population. The inclusion of these controls reduces the magnitude of the estimated coefficients, but the estimated network interaction effect is almost unaffected in its magnitude and precision. The implied interaction effect still grows in magnitude across event studies with longer time windows. In the last three columns, we move on to models including recipient-town fixed effects. We now find that the coefficient on $r_{ij}\gamma_j$ shrinks considerably, from 0.3 in the models with covariates to 0.1, suggesting the

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[38] We compute standard errors using the full variance-covariance matrix of the vector of estimated coefficients:

$$\text{var}(\beta_1 - \beta_2 - \beta_3) = \sum_{i=1}^{3} \text{var}(\beta_i) - 2\text{cov}(\beta_1, \beta_2) - 2\text{cov}(\beta_1, \beta_3) + 2\text{cov}(\beta_2, \beta_3).$$

Table 6—Rail and Telegraph Technological Interaction Effects: Cluster Event Studies

	2 weeks (1)	3 weeks (2)	4 weeks (3)	2 weeks (4)	3 weeks (5)	4 weeks (6)
Rail and telegraph	0.293	0.358	0.405	0.253	0.308	0.347
$r_{ij}\gamma_j$	(0.059)	(0.062)	(0.063)	(0.056)	(0.058)	(0.061)
Rail and no telegraph	0.006	0.011	0.013	0.007	0.013	0.015
$r_{ij}(1-\gamma_j)$	(0.007)	(0.008)	(0.009)	(0.007)	(0.008)	(0.009)
No rail and telegraph	0.177	0.211	0.233	0.142	0.166	0.183
$(1-r_{ij})\gamma_j$	(0.029)	(0.032)	(0.035)	(0.028)	(0.031)	(0.034)
Network interaction	0.110	0.137	0.159	0.104	0.130	0.149
	(0.059)	(0.060)	(0.064)	(0.058)	(0.058)	(0.062)
Signal-recipient distance	−0.001	0.002	0.004	−0.0002	0.003	0.004
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Controls	N	N	N	Y	Y	Y
Recipient town fixed effects	N	N	N	N	N	N
Mean of dependent variable	0.048	0.062	0.073	0.048	0.062	0.073
R^2	0.111	0.125	0.138	0.138	0.163	0.181
Observations	29,592	29,592	29,592	29,497	29,497	29,497

	2 weeks (7)	3 weeks (8)	4 weeks (9)
Rail and telegraph	0.106	0.109	0.100
$r_{ij}\gamma_j$	(0.047)	(0.032)	(0.033)
Rail and no telegraph	−0.002	0.003	0.007
$r_{ij}(1-\gamma_j)$	(0.005)	(0.005)	(0.004)
Network interaction	0.108	0.106	0.093
	(0.046)	(0.032)	(0.033)
Signal-recipient distance	−0.005	0.0004	0.004
	(0.003)	(0.003)	(0.003)
Controls	N	N	N
Recipient town fixed effects	Y	Y	Y
Mean of dependent variable	0.048	0.062	0.073
R^2	0.066	0.064	0.06
Observations	29,592	29,592	29,592

Notes: The table presents estimation results of the cluster event study approach based on equation (2), using the benchmark 30 km radius clusters. The dependent variable is a dummy variable for whether a town within the cluster radius experienced a crusade event within the time window in each column header following the cluster-defining town experiencing its crusade event. All models include event-cluster fixed effects, state fixed effects, and the distance between signal-generating and recipient towns. Columns 4–6 include the following set of controls: native-born share, Black share, sex ratio, newspapers per capita, post office dummy, religious ascriptions Herfindahl index, Presbyterian share, and log population. Columns 7–9 include recipient-town fixed effects. In columns 1–6 the interaction effects are computed as the difference between the coefficients on $r_{ij}\gamma_j$, $r_{ij}(1-\gamma_j)$, and $(1-r_{ij})\gamma_j$. In columns 7–9 the interaction effects are computed as the difference between the coefficients on $r_{ij}\gamma_j$ and $r_{ij}(1-\gamma_j)$. Standard errors are robust and clustered two-ways, at the event-cluster and at the recipient town levels.

importance of omitted unobservables. This coefficient is, however, precisely estimated, and leads to a similarly precise network interaction effect of 0.1 irrespective of the time window we use. While in columns 1–6 the coefficients on the network interaction effect become larger as we study longer time windows, this is no longer the case in columns 7–9. We see this as strong evidence that the simultaneous inclusion of cluster and town-level fixed effects is sufficient for identifying the rail-telegraph interaction effect. The combination of a direct rail link and telegraph access increases the likelihood of collective action by 10 percentage points relative to having access to just the rail link or to the telegraph. This is 1.6 times the mean of the dependent variable (0.062), a quantitatively large effect. Thus, despite being a newer and more efficient technology, the telegraph did not simply replace the railroad in facilitating protest diffusion. On the contrary, it played a strong complementary role.

Robustness.—We conclude this section with additional robustness exercises strengthening the validity of our results. In online Appendix Table A.16 we present models similar to those in Table 6, for alternative cluster radii. These models include recipient-town fixed effects. We continue to find positive and significant network interaction effects. As we expect in a network setting (where distance imposes frictions on information flows), their magnitude decreases as we increase the cluster radii: from 0.8 for the 50 km specifications to 0.5 for the 120 km specifications. Similar to the baseline 30 km results, different event study time windows make no difference to the estimated magnitudes.

In online Appendix Table A.17, we then test for evidence of heterogeneity in the effects of these network interaction effects. The table reports estimates for different cluster radii (30 and 50 km), but fixing a two-week event study time window. We include interactions between each coefficient and the number of newspapers per capita (columns 1 and 2), the post office dummy (columns 3 and 4), the religious heterogeneity index (columns 5 and 6), and the sex ratio (columns 7 and 8). Across seven of the eight specifications, we find the network interaction to remain stable around 0.1, and no evidence of any significant heterogeneity.

In Table 7 we present results of a placebo test on the event study methodology, to address the possibility of residual time-persistent unobservables. In this exercise the dependent variable is a dummy for a crusade event in the signal-recipient town in the time window *prior* to the crusade event in the signal-generating town (instead of after the crusade event in the signal-generating town). The table presents results for different cluster radii definitions and different time windows, with and without town-level fixed effects. We find no statistically significant network interaction effects, with both negative and positive point estimates across different specifications.

In a second placebo exercise we address the possibility of unobserved similarities between the signal-generating town and the signal-recipient towns in its cluster. We create false clusters by replacing each true signal-generating town i for its closest match k, using a matching algorithm based on covariate similarity between towns.[39] We then estimate the response of towns $j \in G_d(i,t)$, to the crusade event of town k, which generically took place on a different date than i's. We report these results in online Appendix Table A.18, for different cluster radii and event-study time windows. Once again, we find no systematic pattern of signs for the estimated network interaction effects, and all but one of the coefficients across specifications is statistically significant at the 5 percent level. Taken together, these results indicate

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[39] We use the Mahalanobis distance metric to find the closest matches, using the native-born population share, the Black share, the sex ratio, the number of newspapers per capita, the number of alcohol vendors per capita, the religious Herfindahl index, the number of Presbyterian sittings per capita, and log population.

Table 7—Rail and Telegraph Technological Interaction Effects: Placebo Event Studies Using Previous Weeks' Responses

	30 km				50 km	
	2 weeks (1)	4 weeks (2)	2 weeks (3)	4 weeks (4)	2 weeks (5)	4 weeks (6)
Rail and telegraph	0.076	0.150	−0.052	−0.049	−0.001	0.017
$r_{ij}\gamma_j$	(0.023)	(0.038)	(0.031)	(0.032)	(0.022)	(0.024)
Rail and no telegraph	0.0016	0.0063	0.0032	0.0063	0.0007	0.0045
$r_{ij}(1-\gamma_j)$	(0.005)	(0.007)	(0.004)	(0.004)	(0.003)	(0.003)
No rail and telegraph	0.106	0.162				
$(1-r_{ij})\gamma_j$	(0.018)	(0.024)				
Network interaction	−0.032	−0.018	−0.055	−0.055	−0.001	0.013
	(0.026)	(0.041)	(0.031)	(0.032)	(0.022)	(0.024)
Signal-recipient distance	0.0005	0.0035	−0.0012	0.0008	−0.0038	−0.0030
	(0.003)	(0.004)	(0.002)	(0.003)	(0.001)	(0.001)
Recipient town fixed effect	N	N	Y	Y	Y	Y
Mean of dependent variable	0.023	0.036	0.023	0.036	0.020	0.033
R^2	0.075	0.101	0.042	0.062	0.017	0.026
Observations	30,557	30,557	30,557	30,557	81,858	81,858

	80 km		120 km	
	2 weeks (7)	4 weeks (8)	2 weeks (9)	4 weeks (10)
Rail and telegraph	−0.004	0.017	−0.001	0.021
$r_{ij}\gamma_j$	(0.018)	(0.021)	(0.017)	(0.019)
Rail and no telegraph	−0.0014	0.0042	−0.0003	0.0048
$r_{ij}(1-\gamma_j)$	(0.0021)	(0.0026)	(0.0019)	(0.0024)
Network interaction	−0.002	0.012	−0.0005	0.016
	(0.018)	(0.020)	(0.017)	(0.019)
Signal-recipient distance	−0.0029	−0.0021	−0.0021	−0.0014
	(0.0007)	(0.0008)	(0.0005)	(0.0005)
Recipient town fixed effect	Y	Y	Y	Y
Mean of dependent variable	0.01	0.032	0.018	0.030
R^2	0.019	0.017	0.007	0.014
Observations	200,603	200,603	421,712	421,712

Notes: The table presents estimation results of the cluster event study approach based on equation (2), where the dependent variable is a dummy variable for whether a town within the cluster radius experienced a crusade event within the time window in each column header prior to the cluster-defining town experiencing its crusade event. Columns 1–4 use 30 km radius clusters. Columns 5 and 6 use 50 km radius clusters. Columns 7 and 8 use 80 km radius clusters. Columns 9 and 10 use 120 km radius clusters. All models include event-cluster fixed effects, state fixed effects, recipient-town fixed effects, and the distance between generating and recipient towns. In columns 1 and 2 the interaction effects are computed as the difference between the coefficients on $r_{ij}\gamma_j$, $r_{ij}(1-\gamma_j)$, and $(1-r_{ij})\gamma_j$. In columns 3–10 the interaction effects are computed as the difference between the coefficients on $r_{ij}\gamma_j$ and $r_{ij}(1-\gamma_j)$. Standard errors are robust and clustered two-ways, at the event-cluster and at the recipient town levels.

that complementarities between the railroad and telegraph communication networks were an important channel of social interactions in the diffusion of the Temperance Crusade. They also suggest that similar kinds of interaction effects may arise across other kinds of communication networks.

Finally, we leverage the variation in collective action paths across towns in our data to explore whether the type of communication technologies a town has access to are suggestive of differences in the content of signals about *the same* event, when such signals travel along different communication networks.[40] Restricting attention to towns that have not engaged in any crusade activity at a given date (the subsets of towns having previously engaged in some crusade activity are a selected sample), and averaging across event studies, we estimate a discrete choice multinomial logit model with four possible responses from signal-recipient towns: no collective action, holding a meeting, circulating a petition, or staging a march. We allow the conditional choice probabilities to depend on rail and telegraph access. This allows us to compare the relative likelihood of transitioning to different stages of protest activity as a function of the technologies the town has access to.

We present the results in online Appendix Table A.19. For a given window of time (two, three, or four weeks), each column reports the coefficient estimates corresponding to the choice probability for each type of event—meeting, petition, and march. (The omitted category is not holding a crusade event in the corresponding time window). Coefficient estimates in the first row illustrate that compared to towns with neither technology, towns with a railroad connection are less likely to move towards a march, and relatively more likely to move towards petitions or meetings. These differences become starker the wider the time window we consider. If rail-mediated signals flow relatively slowly, they may contain information not just about the occurrence of crusade events but also about their relatively disappointing results, making crusaders more hesitant.

We now move to the second row of coefficients. Compared to towns with neither technology, towns with telegraph access are relatively more likely to move towards a more "advanced" stage of crusading. We see a clear monotonic pattern: telegraph access increases the likelihood of a meeting, increases even more the likelihood of a petition, and increases even further the likelihood of a march. By virtue of their high frequency, telegraph-mediated signals may be, on average, free of information about the (negative) outcomes of neighboring protests. In such a case, crusading women (or perhaps movement leaders with overly optimistic priors) might have responded with more enthusiasm. Finally, among towns with rail access (comparing the first and third rows), we recover the differential response of towns with and without telegraph. In this case, once again, we see that towns with telegraph access are disproportionately more likely to move towards a march. Taken together, these results suggest that technology differences led to signal content differences, which were relevant to the crusaders' decision-making. More broadly, they suggest a mechanism for the rapid diffusion but short duration of a variety of collective action movements. In online Appendix C we provide additional suggestive evidence based on the aggregate patterns of the protest adoption curve, distinguishing between alternative mechanisms underlying the information-mediated social interactions we found here.

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[40] We thank an anonymous referee for suggesting this possibility.

V. Concluding Remarks

We study how communication networks mediate social interactions, leading to the geographic diffusion of protest activity. We do so in the context of the Temperance Crusade, a nineteenth-century female movement of mass collective action. We use a linear model of social interactions and rely on exogenous variation in network connectivity induced by railroad accidents to estimate the causal effect of rail and telegraph-mediated information about crusade activity, on the crusade activity of neighboring towns. We find evidence of large social interaction effects, which account for the spatial diffusion of the protest movement. We also provide evidence of the importance of newspapers for its diffusion. We then propose an event-study methodology allowing us to identify complementarities between the rail and telegraph networks in the responsiveness of protesters to information about neighboring protest activity. Our findings confirm the importance of communication networks as drivers of protest diffusion when social interactions are important, and the key role that organizational stages can have in fostering protest movements. Taken together, our results highlight that collective action in the context of protest activity is shaped by network effects. They also highlight that the information technologies available and their network structure are first order mediators of social interactions. We hope our results encourage further research on the role of competing networks in shaping the quantity and quality of information relevant for political mobilization, public good provision, and other forms of collective action, particularly in contemporary settings where online networks coexist with more traditional communication technologies.

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