CONTAGIOUS POLITICAL CONCERNS: IDENTIFYING UNEMPLOYMENT SHOCK INFORMATION TRANSMISSION USING THE DANISH POPULATION NETWORK *

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While it is widely believed that social pressure influences voters, there is little solid evidence that information transmitted through networks affects voter beliefs, policy preferences, and behavior. We investigate this function of networks with respect to unemployment shocks in post-financial crisis Denmark, where we link panel surveys to rich administrative data covering the entire population. Mapping each respondent's educational, familial, and vocational ties, we find that unemployment shocks afflicting second-degree connections in other municipalities—individuals that a voter interacts with indirectly—increase a voters' self-assessed risk of becoming unemployed, perception of the national unemployment rate, support for unemployment insurance, and probability of voting for left-wing political parties. Voters' own unemployment concerns and political preferences respond primarily to unemployment shocks afflicting second-degree connections in similar industries, whereas voters update about national aggregates from all shocks equally. This implies that political preferences driven by information transmitted through weak ties principally reflect self-interested—rather than sociotropic—motives.

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1 Introduction

It is widely believed that social networks play a central role in explaining life experiences across a wide range of economic and social domains (e.g. Banerjee et al. 2013; Carrell, Sacerdote and West 2013; Chetty, Hendren and Katz 2016; Christakis and Fowler 2007; Sacerdote 2001). It is hard to overstate the potential importance of the networks in which people spend a large fraction of their waking hours. However, political scientists have only recently started to exploit exogenous variation and use detailed network data to rigorously examine their empirical importance (see Fowler et al. 2011).

Social networks could influence economic and political beliefs and behavior through at least three channels. First, social networks may exert powerful *pressure to conform* with network norms, whether through explicit threats or learned norms (e.g. Abrams, Iversen and Soskice 2010; Sinclair 2012). Perhaps most notably, Gerber, Green and Larimer (2008) demonstrate that mobilization mailings promising to publicize a household's turnout among their neighbors substantially increases voter turnout. Second, networks could *coordinate* voter beliefs and actions through explicit interaction or shared expectations (e.g. Chwe 2000; Siegel 2009). This could accentuate pre-existing behavior (e.g. Putnam 2000), or—as in the case of revolutionary collective action (e.g. Kuran 1991; Steinert-Threlkeld 2017)—induce behavior that would not have otherwise occurred. Finally, social networks may *transmit information* that updates voters' beliefs and in turn influences their policy preferences and voting behavior (e.g. Huckfeldt and Sprague 1995; Lazarsfeld, Berelson and Gaudet 1948).

This paper focuses on how information transmission through social networks affects voters' economic and political beliefs, preferences, and behavior. This role of social networks has received comparatively less recent attention than networks' social pressure function among political scientists (e.g. Bond et al. 2012; Gerber, Green and Larimer 2008; McClendon 2014; Nickerson 2008; Sinclair 2012). Sinclair (2012) has suggested that this role of social networks may be limited

in comparison with social pressure. However, given that voters in even the world's best educated and politically engaged democracies are often relatively poorly informed about their economic and political environment (e.g. Delli Carpini and Keeter 1996), cheap access to information via social networks has the potential to substantially affect voters' beliefs (e.g. Downs 1957; Lazarsfeld, Berelson and Gaudet 1948).

In particular, we examine whether the transmission of information concerning economic shocks to second-degree connections—specifically, indirectly-connected individuals becoming unemployed—influences voters' economic perceptions. Such information may be relevant for both an individual's own unemployment prospects and their evaluation of the national economy, and in turn affect policy preferences and voting behavior. On one hand, a large literature argues that economic self-interest drives economic and social policy preferences (e.g. Iversen and Soskice 2001; Moene and Wallerstein 2001; Rehm 2011*b*). This implies that information affecting voter beliefs about their personal unemployment risks will induce those believing that they face elevated unemployment risks to support more generous social insurance and the left-wing parties most likely to provide such policies (Lipset and Rokkan 1967; Rehm 2011*a*). On the other hand, "sociotropic" voters may instead primarily use their evaluation of the national economy—as opposed to their personal economic situation—to inform their vote choice (e.g. Kinder and Kiewiet 1981). This theory instead implies that information about unemployment shocks affecting others will reduce a voter's support for the government, to the extent that they upwardly update their beliefs about that national unemployment rate.

We address the severe empirical challenges facing observational studies seeking to identify causal effects in network contexts, which are explained in detail below, by combining extraordinarily rich Danish administrative data with a panel survey conducted in the aftermath of the 2008 financial crisis. Following significant increases in the national unemployment rate after the crisis, economic policies to address unemployment represented perhaps the most salient political cleavage over this period, including the 2011 general election where the left-wing bloc defeated the

incumbent right-wing coalition by only 0.9 percentage points. Our Danish register data presents an unprecedented opportunity to extensively map weak ties for the entire population of living Danes since 1980, defining an individual's first-degree connections by their: (1) nuclear family and partner; (2) graduating cohort at their final educational institution; and (3) recent co-workers. The register data also identify unemployment shocks afflicting any member of the population. The panel survey links the administrative data to economic and political beliefs, preferences, and behaviors for a subset of the population between 2010 and 2013.

Our identification strategy leverages unemployment shocks to second-degree connections, or individuals to whom an individual is indirectly connected (see Bramoullé, Djebbari and Fortin 2009). Focusing on such *shocks* alleviates the "reflection problem" (Manski 1993), by establishing the direction that the information must pass. In addition to exploiting shocks to second-degree connections, we address the common shocks concern in two ways. First, we restrict our sample of shocks to second-degree connections living in different locations from either our respondent or the first-degree connection connecting the respondent to their second-degree connection. Second, we include a variety of fine-grained fixed effects to ensure that our estimates are identified only from within-year differences in the distribution of shocks within the networks of respondents within the same parish and industry, occupation, and educational categories. These fixed effects flexibly control for key potential confounds such as vocation-specific risks and political exposure, differences in local economic conditions, and localized access to media. Furthermore, examining unemployment shocks to individuals that a respondent does not interact with directly, where the information about shocks passes through first-degree connections living in different locations from the affected individuals, helps us to distinguish information transmission from social conformity pressures and emotional reactions to the plight of people the respondent knows. Although we cannot directly observe the interactions between millions of voters, our analysis of transmission mechanisms strongly suggest that information transmission is the main driver of our results.

We find that the beliefs, policy preferences, and voting behavior of Danes respond to unem-

ployment shocks afflicting second-degree connections. Specifically, unemployment shocks to such connections cause voters to positively update their unemployment expectations for both themselves and the country at large. Such concerns are reflected in significantly increased support for more generous unemployment insurance, a key proposal of left-wing parties following the financial crisis. The shocks also alter voting behavior, with a standard deviation increase in the share of the median individual's second-degree network becoming unemployed increasing their probability of voting for a left-wing political party by approximately 30 percentage points. Variation in incumbency within our sample indicates that voters are not simply punishing the incumbent party, as predicted by sociotropic theories. These findings are robust to a wide variety of tests of the identifying assumptions and placebo exercises. Most notably, we show that there is no effect of unemployment shocks afflicting second-degree connections linked to a respondent via either a type of first-degree connection with whom the respondent does not discuss unemployment or politics, or someone similar to an actual first-degree respondent that the respondent does not know. In addition to demonstrating that information acquired through social networks is a key force underlying distributive and insurance preferences, and preferences between political parties, the magnitude of our estimates could easily explain the left-bloc's wafer-thin electoral victory in 2011.

Analysis of the mechanisms indicates that our findings reflect information transmission within social networks and self-interested responses to new information about unemployment prospects. First, supporting an information flow channel through intermediary connections, responses to first-degree connection shocks are around five times greater than responses to second-degree connections. Second, examining heterogeneity by the similarity of the respondent and the second-degree connection affected by the shock, we show that increased self-assessed unemployment risks and increased support for left-wing parties primarily reflects shocks to second-degree connections in the same industry. This finding, which is difficult to reconcile with social conformity, suggests that the differential responses to the shocks of first- and second-degree connections are principally driven by the lower likelihood that second-degree connections are employed in a respondent's

industry. Moreover, such heterogeneity indicates that self-interest motivates changes in political preferences. Third, further suggesting that our results do not reflect sociotropic voting, we find no comparable effect of unemployment shocks afflicting second-degree connections from similar industries on a respondent's *national* unemployment outlook.

This article makes three primary contributions. First, we provide rigorous evidence that *weak ties* play an important role in the socioeconomic and political lives of voters by transmitting information. Our findings chime with Granovetter's (1973) seminal work suggesting that weak ties provide valuable links to job opportunities, and Kiewiet's (1983) and Lazarsfeld, Berelson and Gaudet's (1948) claims that influential economic and political information often emanates from friends and neighbors. More recent studies have highlighted the importance of peers for enhancing educational and work performance (e.g. Cornelissen, Dustmann and Schönberg 2017; Sacerdote 2001), exposure to alternative perspectives (Barberá 2015), providing political expertise (e.g. Ahn et al. 2013), and mobilizing collective action (Steinert-Threlkeld 2017). In contrast with these studies, and the research highlighting the role of information and especially social pressure among individuals with strong ties (e.g. Sinclair 2012), we demonstrate that information diffusion through relatively weak ties substantially affects political preferences and voting behavior across the working age population in a real-world observational setting.

Second, we show that economic information transmitted through social networks has important consequences for political preferences and voting behavior. In our Danish context, where participation is already high, these findings lend a partisan flavor to recent work identifying the influences of social networks on political mobilization (e.g. Bond et al. 2012; Gerber, Green and Larimer 2008; Nickerson 2008). Moreover, we extend prior research in the United States exploring the diffusion of political information among those discussing politics (Huckfeldt and Sprague 1995; Lazarsfeld, Berelson and Gaudet 1948; Walsh 2004) by examining the diffusion of economic shocks through weak ties, addressing challenging empirical concerns, and leveraging network data with unprecedented detail. We also lend external validity to experimental studies focusing on identifying the

influence of networks' information transmission function in relatively artificial laboratory or online contexts (Ahn et al. 2013; Barberá 2015; Klar and Shmargad forthcoming; Mutz 2002).

Third, our analysis highlights differential predictions between self-interest and sociotropic accounts of voting behavior. Such motivations are often difficult to separate because the experiences of others are informative about both an individual's own employment prospects and the country's (Ansolabehere, Meredith and Snowberg 2014). However, our heterogeneous effects help us to separate shocks that affect an individual's own unemployment prospects from shocks that affect aggregate unemployment prospects, and thus show that—in the context we study—concerns about unemployment primarily influence policy preferences and voting behavior via self-interest. These results support the insurance-based theories proposed by, among others, Iversen and Soskice (2001), Moene and Wallerstein (2001), and Rehm (2011*b*). Moreover, our findings suggest that the wealth of previous findings attributed to sociotropic voting (e.g. Hansford and Gomez 2015) could instead reflect voters updating about their own prospects from the signals they receive within their social networks.

2 Information transmission through social networks

The potential for information to diffuse through networks is widely recognized (Jackson 2010).[1] In many contexts, word-of-mouth is a key source of both general information and specific information relevant for particular types of individuals. In this section we first consider how information about unemployment shocks transmitted through social networks might affect voters, before outlining the significant empirical challenges in identifying such causal relationships.

[1] Our identification strategy is geared toward identifying information transmission, rather than questions of how different network structures or the position of shocked individuals influence information diffusion (e.g. Klar and Shmargad forthcoming).

2.1 Unemployment shock information and voter beliefs, preferences, and behavior

Social interaction can allow informed individuals to transfer their information to uninformed individuals, who may use the new information to update their prior beliefs about the world. Amassed evidence from field experiments shows that voters learn from politically relevant information received from credible media, NGOs, and political sources (Arias et al. 2017; Banerjee et al. 2011; Kendall, Nannicini and Trebbi 2013), with more limited belief updating among voters with stronger prior beliefs (Arias et al. 2017). In our network setting, we expect that receiving information about unemployment shocks through socially-connected individuals causes voters to increase their perceptions of aggregate unemployment, and—if the shock is informative about an individual's own unemployment risk—increase the probability that an individual believes that they will become unemployed themselves. If voters have limited information processing and retention capacities, such changes may only be temporary (Zaller 1992).

Persistent changes in posterior beliefs about national and individual unemployment prospects could in turn alter voters' political preferences and voting behavior. An influential literature argues that voters facing higher individual or occupational unemployment risks may in turn increase their support for government programs. The simple insurance logic is that individuals expecting to experience future unemployment will demand more generous unemployment insurance in anticipation of becoming unemployed (see Iversen and Soskice 2001; Moene and Wallerstein 2001; Rehm 2011*b*). Empirical studies point to robust correlations between risk measures and support for unemployment insurance (e.g. Cusack, Iversen and Rehm 2006; Hacker, Rehm and Schlesinger 2013; Rehm 2011*b*), and the left-wing political parties typically espousing such policies (Lipset and Rokkan 1967; Rehm 2011*a*). This implies that, to the extent learning of unemployment shocks to second-degree connections causes voters to update their expectation of becoming unemployed, such shocks are likely to increase support for more generous unemployment insurance. This prefer-

ence for insurance may not necessarily also translate into support for general redistribution though, since permanent transfers predominantly benefiting the poor are costly for at-risk voters when employed.

While the insurance logic rests on self-interested voters supporting policies that they expect will benefit them materially, sociotropic voters instead vote on the basis of *national*-level economic performance (Kinder and Kiewiet 1981; Lewis-Beck and Stegmaier 2000). Although this could reflect a self-interested desire for electing a competent government, the voter objectives are distinct. Regardless, voters that come to believe that the national unemployment rate is higher than previously-believed are expected to hold the government responsible and decrease their support for the parties in government (e.g. Fearon 1999).

2.2 Challenges in identifying the effects of information transmission

Despite the significant potential of social networks to impart new information, identifying and then interpreting the effects of networks' informational function faces severe empirical challenges.

As computing power ceases to represent a major constraint, causal identification challenges have come to the fore. First, the difficulty of comprehensively mapping social networks both makes it difficult to know where to expect informational effects and to ensure that estimates are not biased by omitted network ties. Even if nodes are missing at random, Chandrasekhar and Lewis (2016) show that this results in non-classical measurement error that can severely upwardly bias estimates. Second, even if a network has been accurately mapped, it is hard to separate information transmission from homophily and correlated shocks (Bramoullé, Djebbari and Fortin 2009). In other words, what appears like information passed through a social network could simply reflect connections developing similar beliefs and behaviors because they sort into networks containing like-minded people (Lazer et al. 2009) or are subjected to contemporaneous shocks such as industry-wide unemployment risks. Third, even in the absence of such correlated shocks, a shock emanating from a particular node is required to address the "reflection problem" that in-

dividual i affecting individual j is observationally equivalent to j affecting i (Manski 1993). In the absence of an exogenous shock affecting either i or j, the reflection problem makes it difficult to disentangle whether an individual's economic and political outcomes are affected by the individuals to which they are directly connected, or vice versa. For all these reasons, one should be cautious when interpreting findings from empirical studies documenting positive correlations in beliefs and preferences among family members, friends, and co-workers (e.g. Barberá 2015; Huckfeldt and Sprague 1995; Jennings, Stoker and Bowers 2009; McClurg 2006; Newman 2014; Sokhey and McClurg 2012).

Even after solving these challenges for causal inference, a separate challenge is separating information from social conformity or other potential mechanisms facilitated by social networks. For example, while Nickerson (2008) provides experimental evidence that turnout propagates within household networks, it is not clear whether information or conformity drive this. Similarly, Newman (2014) cannot distinguish whether it is information about a friend's economic distress or the friend's distress itself that increase class-based conflict. Finally, Pietryka and DeBats (2017) explicitly challenge future research to investigate whether proximity to political elites increases support for the party of the elite due to information or social pressure.

3 Danish social and political context

We study the effects of unemployment shocks transmitted through social networks on economic concerns and political preferences in the aftermath of the 2008 financial crisis in Denmark. The country contains strong informal social networks that actively discuss politics and private economic issues (e.g. European Commission 2004; Pichler and Wallace 2007), while the economic crisis substantially increased unemployment rates from normally low levels. Elections follow a proportional representation system, and Denmark has historically been governed by alternating center-right and center-left coalition governments. The country is divided into 98 municipalities,

the primary unit of subnational government, containing around 50,000 people on average. Within municipalities, Denmark's 2,187 parishes (in 2011) are the smallest administrative unit.[2]

3.1 Informal social ties

Informal networks, rather than formally constituted organizations and activities,[3] are the primary basis of social ties in Danish society. A European Commission (2004) survey shows that 64% of adults report having social contact with friends at least once a week, while 44% report that they meet socially with colleagues outside of work at least once a month. Furthermore, 52% report that they would rely on their social network to receive help with paperwork (related to taxes, social benefits, etc.), 73% say that they would use their social network to discuss private problems, and 40% indicate that they would use their social network to borrow money.[4]

Various studies highlight education (e.g. Nielsen and Svarer 2009) and the workplace (e.g. Glitz and Vejlin 2014) as important sources of social interaction in Denmark. Almost everyone completes a total of 10 years of school, and 93% of the 2012-cohort (Danish Ministry of Education) continued into some form of high school program. High school graduates either enter the labor market or (publicly-funded) tertiary education. Given that only five metropolitan areas in Denmark offer university degrees, the geographic diversity of network connections often expands at this point. In the labor market, individuals are likely to spend more time with coworkers than almost anyone else. The relevance of workplace networks is emphasized by Glitz and Vejlin (2014), who show that labor market information from former coworkers affects displaced workers' re-employment probabilities, and Svarer (2013), who finds that partnership dissolution increases with the fraction of coworkers of the opposite sex at the workplace.

Several recent surveys highlight that educational and vocational ties are comparatively more

[2]The number of parishes varies slightly by year.

[3]Low church attendance means that religious networks are weak.

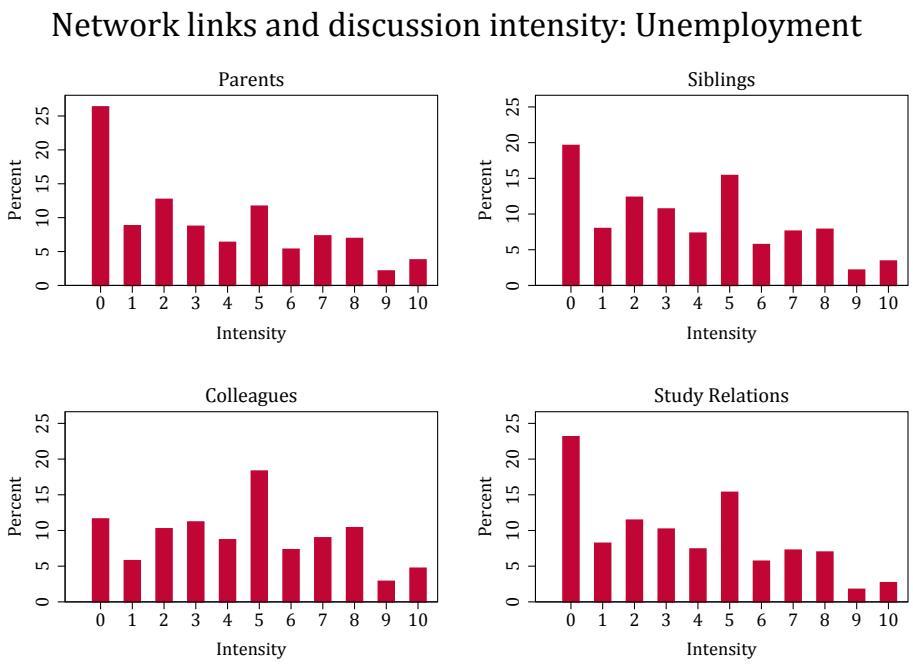
[4]The exact wording of this question was: "In which of the following situations would you be able to rely on friends, work colleagues, neighbors or acquaintances to receive help or support?"

important than familial ties in Denmark. Data from the 2008-2010 wave of the European Values Study ranks Denmark 11th of 46 countries in terms of the importance that Danes assign to friends and acquaintances, and 4th in terms of discussing politics with friends. In contrast, Denmark only ranks 23rd when focusing on the importance that Danes assign to family, and 26th and 20th in terms of discussing politics with their mothers and fathers, respectively. Furthermore, the 2014 wave of the European Social Survey ranks Danes 5th of 21 countries when it comes to socially meeting with friends, relatives, or colleagues. The global comparison permitted by the 2010-2014 wave of the World Values Survey reports even starker differences: Denmark ranks 15th of 91 countries in terms of the importance that Danes assign to friends and acquaintances, but only 65th regarding the importance assigned to family.

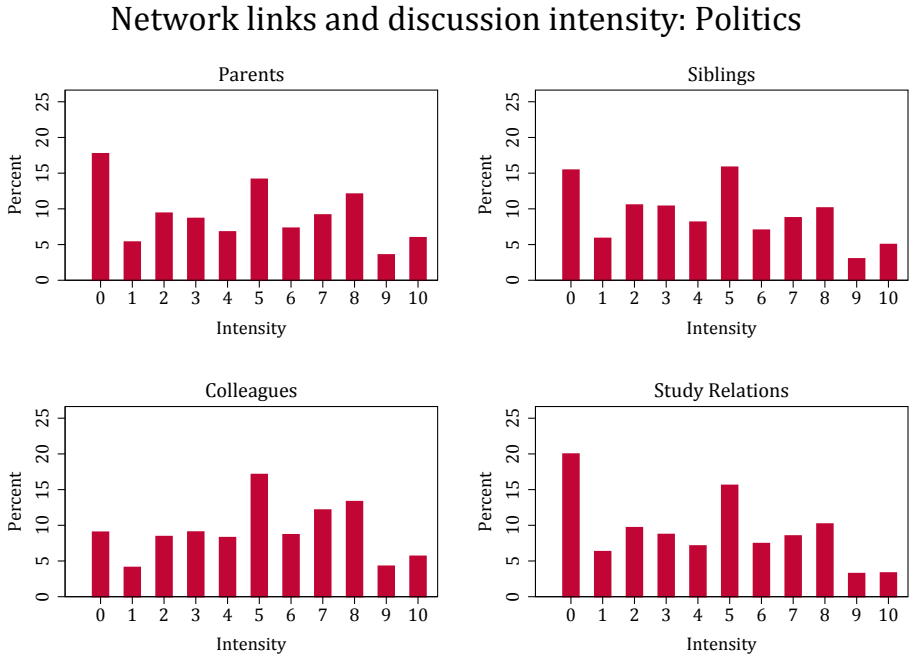
In adult life, social networks tend to be relatively stable over time due to Denmark's limited geographical mobility. In 2014, the number of people changing their official address amounted to 15% of the population, of which only 35% moved between municipalities.[5] Young people typically move across municipal borders when they leave their parents' home around the age of 20-22, and mobility is below average for all age groups above 37. This emphasizes the importance of networks formed by former fellow-students, as well as the importance of social relations to current and former co-workers.

We focus on networks formed by family, current and former colleagues, and cohorts from the most recently completed educational program. Figure 1 demonstrates that discussion of unemployment and politics more generally was common within such networks in 2015. On a scale from 0 to 10, ranging from "never talk to these people about this subject" to "often talk to these people about this subject," panel A shows that the majority of the working age population discusses unemployment within all network categories, and most frequently among work colleagues. Panel B documents similar patterns and higher frequencies regarding the discussion of politics in general.

[5]Statistics Denmark, Statistikbanken, Flytninger, table FLY33 and table FLY66, link.



(a) Discussion of unemployment



(b) Discussion of politics

Figure 1: Frequency of discussion of political issues within social networks in the working age population (source: Danish Panel Study of Income and Asset Expectations 2015)

3.2 Unemployment as a political issue following the 2008 financial crisis

Following a decade of low levels of (gross) unemployment in the 2000s, reaching 2% in early 2008, unemployment almost tripled to around 6% by the 2011 general election.[6] The economy—particularly unemployment—was a central element in the political debate. Nearly 20% of voters cited unemployment as the most important issue for politicians to address, while another 20% regarded the welfare state as most important.[7] Indicating that voters are willing to act on such concerns, previous studies document economic voting in Denmark (e.g. Paldam and Nannestad 1997). After the election, the unemployment rate remained stable around 6%, and the share of Danes regarding unemployment as the biggest political problem rose from 18% in late 2011 to 36% by late 2013.[8] Only in 2014 did the unemployment rate start to fall, stabilizing at around 4% in 2016.

The Danish Unemployment Insurance system—which consists of both means-tested transfers (*kontanthjælp*) and a voluntary contribution scheme (*arbejdsløshedsforsikring*) that enables members to receive around 90% of their pre-employment wage up to around US$35,000—was a key feature of the political debate over this period. The 2011-14 period saw the implementation of a reform limiting the maximum benefit duration under the voluntary scheme, from four to two years, passed in 2010. This caused more unemployed individuals than expected to drop from the more generous voluntary scheme to the lower, means-tested transfer. Further details about Denmark's unemployment insurance system are provided in Appendix section A.1.

[6] Unlike net unemployment, gross unemployment counts labor force participants in active labor market programs as unemployed.

[7] Danish Election Study, cited here.

[8] The 2011 poll is from the Danish Election Study, cited here. The December 2013 poll is from Jyllands-Posten here.

3.3 Political proposals to address the economic crisis

Historically, the leading parties that have always formed part of the government in Denmark are: Venstre/Liberals and Conservatives (center-right), the Social Democrats (center-left), and the Social Liberals (center, sometimes right and sometimes left). The center-right was in government 2001-2011. In 2011, the center-left narrowly defeated the incumbent center-right government, but lost in 2015. Though Danish politics is often characterized by compromise, due to the often-changing coalitions behind policy reforms, there remain major ideological differences between the main blocks, in particular on economic policies and unemployment insurance. These are well known to most voters.

The traditional left-right ideological differences in party platforms were clear in the wake of the financial crisis in 2008, up to the Danish national election in 2011. The Venstre-led government in 2001-2011 implemented a "tax freeze" early on, and with a new prime minister taking over in 2009, proposed and implemented a number of market-oriented policies, including a 2009-2010 tax reform with upper bracket tax cuts on wage incomes, a 2011 "growth program" providing subsidies to small businesses and promising cuts to corporation tax, and—most controversially—the 2010 reform of the unemployment insurance system mentioned above.

In contrast, the Social Democrats and Socialist People's Party campaigned on a platform called "Fair Solution" in the 2011 election. This program also contained many policies focused on labor market imbalances, but from a markedly different ideological viewpoint, emphasizing demand-side and educational policies. They strongly criticized the reform of the unemployment insurance system, and called for public investments, labor agreements, and improved academic and vocational education to create new jobs.

4 Empirical design

This section first introduces our main variables and defines social networks, before detailing our empirical strategy for identifying the effects of unemployment shocks transmitted through social networks.

4.1 Panel survey and administrative data

We leverage two high-quality sources of data. For our outcome variables, we use questions capturing subjective unemployment perceptions, policy preferences, and vote choice from the 2010, 2011, 2012, and 2013 rounds of the Danish Panel Study of Income and Asset Expectations (Kreiner, Lassen and Leth-Petersen 2013). This telephone survey first sampled around 6,000 Danes registering some labor income between 1998 and 2004, and has resampled from this pool to maintain the sample size in the face of attrition.[9] The first wave commenced in January and February 2010 and has been repeated annually over those months each year. Table 1 shows that our sample broadly resembles the Danish working age population, although—unsurprisingly for a telephone-based survey—is somewhat older, more educated, and richer.

To define unemployment shocks and construct social networks, we rely on extraordinarily detailed individual-level administrative register data for the entire population. These records, which include family ties, education, and income tax returns, are reported annually by employers, tax authorities, and government agencies, and are available between 1980 and 2012. We thus possess unique identifiers and data for any individual living in Denmark over this period. In total, 7,974,509 different individuals appear at some point over this period. Panel survey responses are linked to these records by Statistics Denmark, Denmark's official statistical agency. Access to the

[9]The initial response rate was 50% (including those who were selected but couldn't be contacted), and attrition into 2011 was 31%. The 2011-2013 surveys randomly sampled additional respondents from the same pool until the original panel size was restored. The 2014-2016 waves cannot be used until the corresponding administrative data becomes available.

Table 1: Summary statistics from each sample

	Full population, aged 20-65		Survey respondents		Respondents' first-degree connections		Respondents' second-degree connections	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
Woman	0.50	0	0.49	0.16	0.54	1	0.51	1
Age	42.90	43.00	45.73	46.44	43.54	43.98	42.26	42.75
Children	0.78	0	1.01	1	0.97	1	0.86	0
Single	0.35	0	0.21	0	0.26	0	0.32	0
Gross income (DKK)	325,251	294,646	403,680	362,382	432,486	379,042	364,075	329,481
Total assets (DKK)	848,888	375,907	1,256,176	808,325	1,117,726	777,658	949,692	562,529
Total debt (DKK)	635,145	271,566	849,482	563,955	826,296	597,285	726,920	427,722
Homeowner	0.49	0	0.68	1	0.63	1	0.55	1
Education basic	0.33	0	0.20	0	0.09	0	0.23	0
Education short	0.39	0	0.42	0	0.41	0	0.45	0
Education medium	0.16	0	0.25	0	0.30	0	0.21	0
Education long	0.08	0	0.13	0	0.20	0	0.10	0
Unemployed	0.06	0	0.04	0	0.03	0	0.04	0
Unemployment shock	0.04	0	0.03	0	0.02	0	0.03	0
Observations	13,400,000	13,400,000	19,709	19,709	1,882,767	1,882,767	9,022,069	9,022,069

Note: To comply with Statistics Denmark's anonymity restrictions, medians and lower and upper bounds of ranges are computed across five observations.

register data is described in Appendix section A.2.

4.1.1 Outcomes: economic and political beliefs and preferences

With respect to beliefs about unemployment, we first measure an individual's self-assessed risk of becoming unemployed using an individual's subjective assessment of their personal unemployment risk. Specifically, survey respondents were asked to estimate the probability, from 0 to 1, that they would become unemployed in the forthcoming year. We also measure two variables that capture broader beliefs about aggregate unemployment prospects: in 2011-2013, the survey elicited respondents' best guess at the current national unemployment rate; in 2011 and 2013, the survey also requested that individuals forecast the national unemployment rate over the next year.

We consider three measures of policy preferences: we define indicator variables for the 34% of respondents expressing support for increasing unemployment insurance above the existing level, the 38% of respondents believing that the government should do more to support the poor, and the 39% of respondents that believe that the government should use a non-market-based stimulus—public investments or a temporary increase in unemployment insurance, as opposed to (income or VAT) tax cuts or firm subsidies—to address the economic crisis (only available in the 2010 survey).[10]

Two measures capture support for political parties: intention to vote for a left-wing party—the Social Democrats, Social Liberals, Socialist Party, or Red-Green Alliance; and an indicator for whether a voter reported having voted for such a left-wing party at the 2011 election.[11] Respectively, 42% and 50% of respondents supported the left by these measures.

[10] In each case, "don't know" or "none of the above" were coded as 0.

[11] Reported turnout rates in our survey were 98%, while the official 2011 turnout rate was 88%. Consequently, our decision not to drop those that did not turn out is inconsequential.

4.1.2 Individual unemployment shocks

Our primary source of variation is individual unemployment shocks. To capture a shock that represents potentially interesting news, we focus on instances of *becoming* unemployed. This requires that an individual register their unemployed status to receive unemployment benefits or social security. In contrast, longer-term unemployment status is less likely to be discussed and is more vulnerable to biases arising from common shocks. Accordingly, we define unemployment shocks using an indicator that denotes whether an individual was registered as unemployed in the November preceding the survey—the snapshot at which the Danish register data is collected—but was not registered as unemployed in November the year prior.[12] On average, every year, 3% of working age Danes experience such a shock over our study period. We therefore do not distinguish between unemployment duration or the number of times an individual became unemployed in a given year. An advantage of this measure is that it relies on official government data, rather than self-reported recollections of information receivers. Furthermore, the timing of our shocks makes it unlikely that survey respondents heard about them just before completing the survey.

4.1.3 Mapping social networks

The register data presents an unprecedented opportunity to fairly comprehensively map weak ties for every individual in the population. Although some social ties are stronger than others, a comprehensive network is important for two key reasons. First, as noted above, a failure to identify first-degree connections could induce bias by failing to accurately measure the information or social pressure an individual is subject to. Second, although in some instances only close ties may influence voter behavior, information about unemployment shocks to second-degree connections is precisely the type of information likely to pass between loose social networks (Glitz and Vejlin 2014; Granovetter 1973). Figure 1 suggests that unemployment is not such a sensitive issue that

[12]Those in active labor market programs are counted as unemployed. Our definition follows international standards.

individuals would be unwilling to share employment experiences. Consequently, we adopt a relatively exhaustive definition of social networks, in order to minimize biases potentially produced by missing connections. This likely entails estimating a lower bound on the effect of information passed among close ties.

Specifically, we define first-degree network connections according to the following criteria:

1. **Family**: parents (including adoptive parents), siblings (including half-siblings), and partners.[13]

2. **Education**: graduating cohort for highest level of educational degree obtained, or cohort at the point of dropping out of school without a degree.[14]

3. **Vocation**: coworkers from within the previous two years. We include all coworkers for individuals in firms with fewer than 25 employees, and only coworkers within the same one-digit educational category for firms with 25 or more employees or for individuals that accumulated more than 50 co-workers across multiple firms over the two years.

The education restriction captures the likelihood that ties attenuate upon moving on to another educational institution. Nevertheless, our results are robust to defining larger networks that include both high school and university-degree graduating classes. The firm size restriction reflects the reality that in sufficiently large firms individuals are most likely to interact with similar colleagues, and education is a good proxy for their rank within the firm. Our approach to defining networks is broadly analogous to Pietryka and DeBats (2017), but at a population level in a less religious modern society.

Our definition of networks inevitably omits some genuine members of an individual's social network. To address the possibility that this could bias our estimates, we control for indicators of

[13]Siblings and parents are linked if a father or mother is alive and registered at any point between 1980 and 2012.

[14]For university-level degrees, we use subject-degree cohort.

education- and firm-level network truncation as a robustness check. Although our network connections may not be perfectly comprehensive,[15] the size and detail of our population-level networks provide an unprecedented opportunity to analyze the effect of information passing through social ties on political belief and preference formation in the real world.

Combining the familial, educational, and vocational networks defined above allows us to construct an $7,974,509 \times 7,974,509$ adjacency matrix. This adjacency matrix relates every individual in the Danish population alive between 1980 and 2012 to every other, using a one to denote the fact that two individuals are linked by one of the connections described above.[16] We explain how this matrix is computed in Appendix section A.3. We focus on the networks of the 8,747 unique labor force participants that appear in our panel survey in 2010, 2011, 2012, or 2013. The mean and median survey respondent in a given year respectively register 224 and 81 first-degree network connections, of which 2% and 8%, 24% and 53%, and 74% and 49% are familial, educational, and vocational ties respectively. Among our respondents, 91% experience in an unemployment shock in their first-degree network in a given year; this rises to over 99% for their second-degree network.

4.2 Identification strategy

Our goal is to estimate the effect of unemployment shocks transmitted through first-degree connections on an individual's economic and political beliefs, preferences, and behavior. To combat the empirical challenges enumerated above, we leverage our detailed network data to exploit unemployment shocks afflicting working age (20-65) second-degree connections—people that are directly connected to individuals' first-degree connections—that have no direct connection to those

[15]The most obvious omission is non-work colleague, non-school, friends and non-nuclear family members. To the extent that such individuals live locally and have local friends themselves, our restriction to shocks to individuals from other municipalities or the municipalities of their first-degree connections (see below) should reduce any biases.

[16]Our definition of educational ties means that the adjacency matrix is not symmetric. For example, someone who only completed high school would be linked to someone in their graduating cohort that did not attend university, but not vice versa.

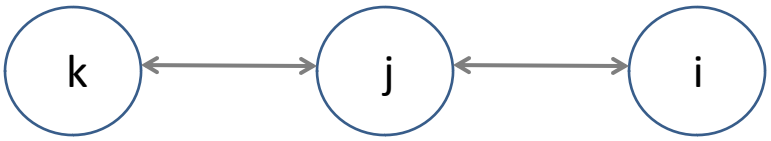


Figure 2: Illustration of network connections

Note: In this example, individual i is observed in our survey, and we estimate the effect of an unemployment shocks to individual k on individual i.

individuals. By shocking the second-degree connections to whom an individual is only indirectly connected, we seek to ensure that our estimates neither reflect direct interactions between our survey respondent and the individual experiencing an unemployment shock nor reflect shocks afflicting individuals united by common experiences and vulnerabilities. We argue that, after removing nearby second-degree connections and controlling for fine-grained fixed effects that account for a wide array of differences between respondents, such shocks are exogenous to other determinants of respondent beliefs, preferences, and behavior. We now explain in detail how this strategy addresses the problems severely inhibiting the study of social networks.

Our empirical strategy requires the construction of each individual's second-degree network. This network can be computed using simple matrix operations illustrated in Appendix section A.3. The defining feature of an individual i's second-degree network is that i is not directly connected to a second-degree connection k. We thus focus on "intransitive triads" where i and j are connected and j and k are connected, but i and k are *not* connected. Leveraging this idea, we exploit unemployment shocks to second-degree connections k that are linked to individual i through a first-degree connection j. Consequently, an unemployment shock to k should only affect i because j transmits this information to i. Figure 2 illustrates this approach, where i is our panel survey respondent, j is her first-degree network connection, and k is her second-degree connection.

This approach serves three functions. First, focusing on shocks afflicting specific individuals establishes the direction in which information travels, and thus addresses the reflection problem. Second, by focusing on shocks to second-degree connections—rather than persisting cir-

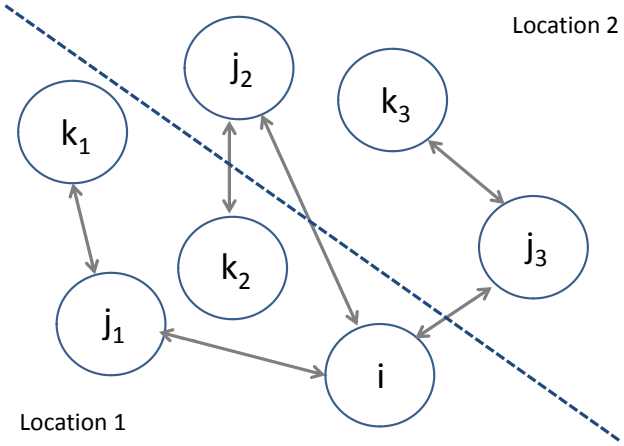


Figure 3: Illustration of excluded cases

cumstances such as parental wealth (e.g. Bramoullé, Djebbari and Fortin 2009)—we also reduce the risk that common shocks reflecting homophily drive k's effect on i. Absent common shocks, it is hard to imagine how i—who does not herself know k—could learn of k's unemployment shock other than by becoming informed of k becoming unemployed. Third, focusing on shocks to second-degree connections helps us to distinguish the effect of information transmission from other changes that could emanate from interacting directly with a first-degree connection who has become unemployed. In particular, the anger or frustration felt by k upon becoming unemployed is unlikely to be conferred through interactions between i and j when k is not present, and is unlikely to persist for months after the shock occurred. Similarly, the social pressure to respond to the shock is also likely to substantially dissipate when passed between connections, especially given the spatial restrictions we next introduce.

Nevertheless, a key concern with respect to the exogenous assignment of unemployment shocks is that such shocks are spatially correlated. The exogeneity of shocks is violated if i receives essentially the same, or highly correlated, information about an unemployment shock to k without receiving such information from a j linking i to k. This is particularly concerning in contexts like Denmark where geographic mobility is relatively low. To address this concern, we exclude

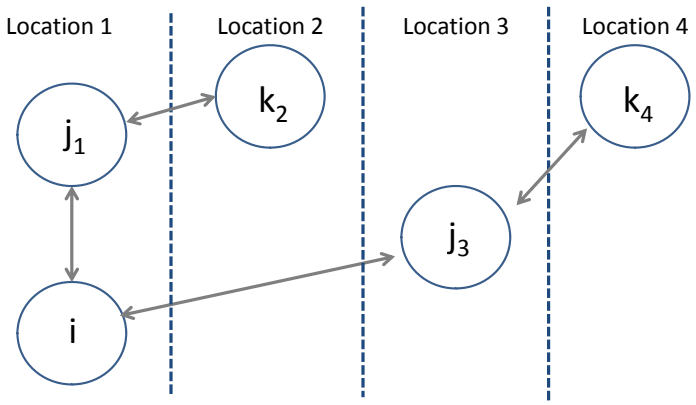


Figure 4: Illustration of included cases

second-degree connections k located in the same municipality as i.[17] Second-degree connections k_1 and k_2 in Figure 3 are examples of the cases that this restriction excludes.

A related spatial concern reflects avenues, other than through j, that unemployment shocks to k could influence i. In particular, the same, or highly correlated, unemployment information that results from an unemployment shock to k could be received by i from j if j experiences shocks correlated with k. For example, if both j and k simultaneously become unemployed, then the effect attributed to k's unemployment shock could instead reflect the shock to j. We address this concern by excluding any second-degree connection k located in a parish where any first-degree connection j resides.[18] This excluded case is exemplified by the second-degree connection k_3 in Figure 3. As a consequence, our identification strategy only exploits unemployment shocks to individuals k in a different location from both individuals i and j. This is represented in Figure 4.

These two restrictions substantially reduce the number of eligible second-degree connections. Before making any restrictions, the mean survey respondent had 17,632 second-degree connections in a given year, while the median had 7,831. Removing second-degree connections from the same

[17] As a robustness check, we further exclude individuals k from within the same region (Denmark has five regions).

[18] Appendix Table A4 shows that our estimates are unchanged but become less precise when k's located in the same municipality as j are excluded.

municipality as the respondent or the same parish as the first-degree connection reduces this set by around half: the mean and median respondent possess 7,130 and 4,364 second-degree connections per year respectively. Table 1 shows that the socioeconomic characteristics of working age second-degree connections in our sample are broadly similar to the working age population over the 2010-2013 period.

Finally, we address non-spatial common shock concerns by using fine-grained fixed effects. Specifically, we restrict attention to variation in unemployment shocks that arise due to differences in network composition between individuals within the same industry, occupation, educational, and geographic groupings. To do so, we include four sets of i-level fixed effects. First, industry-year fixed effects absorb common economic and political attitudes among voters within a given two-digit industry classification in a given year. For example, this ensures that we only focus on differences in unemployment concerns among respondents facing similar industry-level risks, and experiencing similar types of vocational interactions. Second, occupation-year fixed effects fully control for differences across one-digit occupational classifications in a given year. Third, education-year fixed effects capture differences across time in the attitudes of voters within a given one-digit educational classification. Appendix A.4 describes these digit classifications. Fourth, parish-year fixed effects absorb common parish-specific shocks that could induce individuals with different networks to adopt similar unemployment concerns and political preferences. Together, these fixed effects control for many potential common shocks, and increase confidence that our estimates reflect differences in the distribution of second-degree shocks experienced by otherwise similar individuals.[19] Nevertheless, as further robustness checks below, we use a variety of more demanding control strategies, sample restrictions, and placebo tests to assuage lingering concerns.

In sum, we estimate the effect of an unemployment shock to k on i's beliefs, attitudes, and

[19]We exploit cross-sectional differences because there is limited variation in networks and especially the number of shocks experienced within a network over time.

behavior using the following OLS regression,

$$Y_{iwoept} = \beta\, Unemployment\ shock_{kp't} + \gamma_{wt} + \delta_{ot} + \eta_{et} + \mu_{pt} + \varepsilon_{ikwoepp't}, \quad (1)$$

where $Unemployment\ shock_{kp't}$ represents an unemployment shock—being unemployed two months before the survey, having not been unemployed a year earlier—to a second-degree connection k of individual i located in parish p' in year t. Respectively, γ_{wt}, δot, η_{et}, and μ_{pt} are i-level industry-year, occupation-year, education-year, and parish-year fixed effects. Throughout, standard errors are double-clustered by both i's municipality and k's municipality (see Cameron and Miller 2015), to capture the reality that we only actually observe 19,709 responses from 8,747 different panel survey participants between 2010 and 2013, and that second-degree connections in the same municipality may experience similar shocks. To weight each survey response equally, observations are weighted by the inverse of the number of second-degree connections for a surveyed individual in a given year.

5 Unemployment shocks and economic and political beliefs, preferences, and behavior

This section presents our main finding that unemployment shocks to second-degree connections significantly affect economic and political beliefs, preferences, and behavior, before demonstrating its robustness to potential challenges to our identification strategy.

5.1 Effects of unemployment shocks afflicting second-degree connections

Table 2 reports our main results, using equation (1) to estimate the impact of unemployment shocks to second-degree connections on working age Danish voters. The first three columns examine personal and aggregated unemployment belief outcomes, the next three examine economic and

social policy preference outcomes, and the final two examine voting behavior. It is important to recall that unemployment shocks occurred at least two months before respondents were surveyed. Our 141 million observations reflect all second-degree connections surviving our restrictions for each respondent-year of our panel survey.

Our first key finding is that voters significantly update their beliefs about their own unemployment risk following a shock afflict a second-degree connection. Column (1) shows that each such shock increases an individual's self-assessed probability of becoming unemployed within the next year by 0.0035 probability points (i.e. 0.3 percentage points). This represents a 0.01 standard deviation increase in this self-assessed risk, or a 2.5% increase relative to the mean. However, because individuals have many second-degree connections, this estimate implies large and meaningful effects. A standard deviation increase in the share of second-degree connections becoming unemployed (1.5 percentage points) for the median respondent with 4,364 second-degree connections entails a 0.23 point increase in an individual's subjective probability of becoming unemployed. These results imply substantial and lasting effects consistent with voters receiving information that updates their prior beliefs. This suggests that although voters may generally have relatively well-formed unemployment risk perceptions based on their industry or occupation (e.g. Cusack, Iversen and Rehm 2006; Rehm 2011*a*), such beliefs are not fixed since individuals still place significant weight on recent "word of mouth" relaying of experiences.

In addition to updating their subjective unemployment expectations, columns (2) and (3) demonstrate that voters' aggregate unemployment beliefs respond to unemployment shocks transmitted through first-degree connections. Our point estimates indicate that unemployment shocks increase both an individual's current guess at the national unemployment rate and their expectation for the coming year by almost 0.001 points, or 0.1 percentage points. In both cases, a standard deviation increase in the share of second-degree connections becoming unemployed for the median respondent implies around a 0.06 point increase in an individual's assessment of aggregate unemployment rates. Such responses to relatively up-to-date information reinforce the results above and in Alt,

Table 2: Estimates of second-degree connection unemployment shocks on respondent economic and political perceptions and preferences

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market-based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployment shock	0.0035***	0.0009***	0.0012***	0.0031**	0.0013	0.0013	0.0037**	0.0050**
	(0.0010)	(0.0003)	(0.0004)	(0.0015)	(0.0015)	(0.0026)	(0.0015)	(0.0020)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.1380	0.0849	0.0759	0.3146	0.3893	0.4204	0.4799	0.5415
Outcome standard deviation	0.2638	0.0543	0.0470	0.4644	0.4876	0.4936	0.4996	0.4983
Unemployment shock mean	0.0185	0.0182	0.0179	0.0185	0.0185	0.0194	0.0185	0.0183
Survey years unavailable		2010	2010, 2012			2011, 2012, 2013		2010, 2011

Notes: All specifications are estimated using OLS, and include respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. All observations are inversely weighted by the respondent's number of second-degree connections in that year. Standard errors are double clustered by respondent municipality and second-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Lassen and Marshall (2016), and demonstrate that, even among a relatively informed electorate, voter beliefs about national unemployment rates are malleable.

These unemployment shocks, which affect voters' economic outlook, also drive their policy preferences. In particular, column (4) shows that a shock to a second-degree connection significantly increases the probability that an individual supports more generous unemployment insurance. In particular, a standard deviation increase in the share of the median respondent's second-degree network becoming unemployed entails an 0.20 point increase in the probability of supporting more generous unemployment insurance. Although they are not statistically significant, columns (5) and (6) suggest that unemployment shocks may also increase support for redistribution toward the poor and non-market-based government stimulus. It is possible that changes in support for such measures is more limited because they are less directly targeted at those expecting to become unemployed. Together these findings suggest that unemployment shocks transmitted through connections cause voters to adopt more left-wing policy positions. This could reflect both self-interested and sociotropic voters.

However, consistent with the expectation that risk and distributive preferences translate into support for left-wing political parties, unemployment concerns and policy preferences are mirrored in the increased propensity of a respondent to vote for one of Denmark's left-wing parties. Columns (7) and (8) demonstrate that an unemployment shock to a second-degree connection significantly increases the intention to vote for a left-wing party and actually voting for a left-wing party in the 2011 election by 0.0037 and 0.005 probability points respectively. A standard deviation increase in the share of second-degree connections becoming unemployed for the median respondent thus entails 0.24 and 0.33 point increases in the probability of left-wing voting. These substantial effects indicate that information transmitted through social networks can alter electoral outcomes, particularly in competitive elections like those recently experienced in Denmark.

The elevated vote for left-wing opposition in 2011 could also reflect sociotropic voting, since the center-right was in power. However, Appendix Table A2 shows that an unemployment shock

does not reduce intention to vote for the government (which contained right-wing parties in 2010 and 2011 and left-wing parties in 2012 and 2013 at the time of the survey). We provide further evidence against the sociotropic interpretation of voter responses below.

Although familial, educational, and vocational connections all underpin social interactions in Denmark, it is natural to examine differences in transmission across types of connections. To examine this heterogeneity by type of connections, Appendix Tables A5 and A6 interact unemployment shock with the type of connections linking respondent to first-degree connection and linking first-degree connection to second-degree connection. The results generally suggest that shocks transmitted through different types of connections operate similarly.

5.2 Robustness checks

Perhaps the greatest concern is that our estimates reflect common shocks afflicting both the respondent and their second-degree connections. Beyond our sample restrictions and fine-grained fixed effect structure, we employ several additional robustness checks. First, panel A of Table 3 shows that our results are robust to simultaneously controlling for the variables in Table 1 as well as second-degree network size. Second, to further allay concerns relating to geographically-dispersed common shocks, panel B excludes second-degree connections from the same region—one of Denmark's five regions—as the respondent. Although this entails dropping around half the sample, and thus substantially reducing estimate precision, the magnitudes of the point estimates are robust.

Third, we conduct a placebo test examining whether unemployment shocks affect respondents that do not talk about unemployment and politics with the individuals to which they are directly connected. If a respondent does not speak with a familial, educational, or vocational first-degree connection about unemployment and politics, then an unemployment shock to a second-degree connection should not affect that respondent's beliefs and preferences—even when their first- and second-degree connections do speak about these issues among themselves. To test this, we use the survey responses documented in Figures 1a and 1b to restrict our sample to *i*-*k* dyads where *i*

Table 3: Robustness checks

	Own unemployment expectation (1)	Guess national unemployment rate (2)	National unemployment rate expectation (3)	Want more unemployment insurance (4)	Government should support the poor (5)	Support non-market-based stimulus (6)	Intend to vote for left party (7)	Voted for left party in 2011 (8)
Panel A: controlling for covariates								
Unemployment shock	0.0018*	0.0008***	0.0011***	0.0022	0.0006	0.0008	0.0034**	0.0046**
	(0.0010)	(0.0002)	(0.0004)	(0.0015)	(0.0014)	(0.0026)	(0.0014)	(0.0020)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Panel B: second-degree connection lives in a different region from respondent								
Unemployment shock	0.0047***	0.0009*	0.0012*	0.0022	0.0022	-0.0020	0.0033**	0.0047*
	(0.0017)	(0.0005)	(0.0007)	(0.0018)	(0.0018)	(0.0024)	(0.0016)	(0.0028)
Observations	101,102,172	75,781,837	50,014,975	101,102,172	101,102,172	25,320,335	101,102,172	30,061,802
Panel C: placebo where no information is transferred between respondent and first-degree connection								
Placebo unemployment shock	0.0027	-0.0001	-0.0008	0.0007	0.0047**	0.0015	-0.0035	0.0065
	(0.0022)	(0.0006)	(0.0010)	(0.0019)	(0.0023)	(0.0028)	(0.0029)	(0.0044)
Observations	3,467,055	2,786,265	1,874,529	3,467,055	3,467,055	680,790	3,467,055	831,183
Panel D: placebo examining shocks to similar j's that i does not know								
Placebo unemployment shock	0.0018	0.0004	0.0004	0.0015	-0.0054*	0.0005	-0.0077***	-0.0173**
	(0.0023)	(0.0005)	(0.0008)	(0.0025)	(0.0029)	(0.0051)	(0.0026)	(0.0078)
Observations	147,612,755	111,546,689	73,061,520	147,612,755	147,612,755	36,066,066	147,612,755	30,094,652
Panel E: second-degree connection industry-year, occupation-year, education-year, and parish-year fixed effects								
Unemployment shock	0.0030***	0.0005**	0.0008**	0.0019**	0.0019	0.0008	0.0048***	0.0068***
	(0.0009)	(0.0003)	(0.0004)	(0.0009)	(0.0013)	(0.0021)	(0.0012)	(0.0015)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Panel F: cohort-year fixed effects								
Unemployment shock	0.0030***	0.0009***	0.0012***	0.0035***	0.0018	0.0018	0.0039***	0.0058***
	(0.0010)	(0.0003)	(0.0004)	(0.0014)	(0.0014)	(0.0024)	(0.0015)	(0.0018)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Panel G: control for network truncation								
Unemployment shock	0.0044***	0.0010***	0.0012***	0.0031**	0.0015	0.0014	0.0038**	0.0054**
	(0.0010)	(0.0003)	(0.0004)	(0.0014)	(0.0014)	(0.0025)	(0.0016)	(0.0023)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Panel H: removing second-degree connections connected to the respondent by a familial first-degree connection								
Unemployment shock	0.0038***	0.0009***	0.0012***	0.0028*	0.0012	0.0020	0.0036**	0.0039
	(0.0011)	(0.0003)	(0.0004)	(0.0016)	(0.0016)	(0.0027)	(0.0016)	(0.0026)
Observations	136,543,459	102,160,667	67,286,388	136,543,459	136,543,459	34,382,792	136,543,459	27,188,066
Panel I: maximum second degree network of 10,000 connections								
Unemployment shock	0.0027**	0.0010***	0.0013***	0.0028*	0.0016	-0.0008	0.0042**	0.0047*
	(0.0011)	(0.0003)	(0.0005)	(0.0016)	(0.0018)	(0.0031)	(0.0018)	(0.0024)
Observations	52,638,472	38,790,852	25,757,794	52,638,472	52,638,472	13,847,620	52,638,472	14,966,178

Notes: All specifications are estimated using OLS, and include respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. The controls noted in panel A include all variables in Table 1 and second-degree network size. The placebo tests in panels C and D are described in the main text. Panel E includes indicators for educational or vocational network truncation at the i and j levels as controls. All observations are inversely weighted by the respondent's number of second-degree connections in that year. Standard errors are double clustered by respondent municipality and second-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

reports not speaking about unemployment or politics with the j (or all j's) that induce the second-degree connection with k. Specifically, this requires registering zero unemployment discussion with a j of the corresponding type for our first unemployment-related outcomes, and zero unemployment and zero political discussion for our five political preferences outcomes.[20] The results in panel C add significant credence to our main results: we find no association between unemployment shocks and unemployment concerns, policy preferences, and political outcomes. This suggests that it is unlikely that common shocks, which should affect similar people that do not directly interact, are driving our findings.

Fourth, a second placebo test instead assigns to respondents first-degree connections who are not actually in their network but are similar to those that are. Specifically, each j was replaced by a randomly selected j' from our sample that lives in the same municipality *and* works in the same one-digit industry as j in a given year (without replacement), but i does *not* know. We then examine the effects of shocks affecting the k''s associated with each j'. If our findings indeed reflect shock transmission through social networks, rather than common shocks afflicting respondents with similar types of networks, shocks to fake second-degree connections should not affect our respondents. Consistent with the placebo's logic, panel D reports no evidence that such fake shocks influence respondent beliefs and preferences. The negative coefficients in columns (5), (6), and (7) are in the opposite direction to our main findings.

Fifth, common shocks could also arise if parish-level shocks in the location of second-degree connections are correlated with those affecting the respondent. Similarly, respondents could learn, e.g. through access to local media or word of mouth, about general unemployment conditions in another area through avenues other than a specific second-degree connection's experience. To address such concerns, we include k-level industry-year, occupation-year, education-year, and parish-year fixed effects to control for the second-degree connections' environment at the time at which

[20]To increase the power of this placebo test, we assign 2015 conversation levels to all previous years in which the individual participates in the survey. We thus assume that low levels of conversation with certain types of connections is persistent across time.

unemployment shocks may occur. We thus exploit only variation in unemployment shocks within the industries, occupations, educational categories, and parishes of second-degree connections in a given year. Panel E shows that our results are robust to including such fixed effects.

Sixth, shocks affecting second-degree connections also belonging to the respondent's same cohort might be associated with those affecting the respondent themselves (e.g. due to legislation or labor demand decisions that differentially affect different age groups). To address this concern, we include (birth year) cohort-year fixed effects, and thus exploit only variation in unemployment shocks to second-degree connections belonging to the same cohort in a given year. Panel F shows that including such fixed effects does not alter our findings.

Seventh, although our networks are unique in how comprehensively they can capture weak ties, there nevertheless remains the concern that omitted connections could bias our estimates. We address this concern by controlling for indicators for respondents whose networks were truncated at the i and j levels by our education and vocational network restrictions. The results in panel G suggest that our estimates are unlikely to reflect biases resulting from incomplete networks.

Eighth, two concerns could arise from a reliance on familial connections. First, familial connections may be better measured than educational and occupational connections, and thus drive the effects that we estimate. Second, familial connections could drive the results due to a higher frequency of contact, although section 3.1 indicates that Danes may be more likely to discuss unemployment and political issues with educational and vocational than familial connections. To address these potential concerns, we drop second-degree connections generated by familial first-degree connections. Panel H shows that our results are not substantively affected after dropping such connections.

Finally, our results are robust to focusing only on respondents with relatively small second-degree networks—less than 10,000 second-degree connections satisfying the restrictions defining our identification strategy above. Although the drop in sample size reduces the power of our estimates, panel I reinforces our main findings by reporting similar point estimates. Similar results

using a second-degree network cutoff of 5,000 are reported in Appendix Table A3. These tests indicate that our results are not driven by the individuals with the largest networks.

6 Information transmission mechanisms driving voter responses

We now seek to illuminate the process through which unemployment shocks to second-degree connections influence voters. We first show that respondents exhibit greater responses to shocks to first- than second-degree connections, except with respect to national unemployment aggregates Such behavior supports an information transmission mechanism through connections, and appears to reflect voters reacting most to unemployment shocks most relevant to them. Consistent with this, rather than information simply decaying across ties, we find that voters update their beliefs and alter their political preferences most following unemployment shocks afflicting second-degree connections similar to themselves. In contrast, shocks to similar second-degree connections do not differentially affect national outlooks. These results indicate both some degree of sophisticated updating and suggest that political responses are primarily driven by self-interested rather than sociotropic motivations. We finally discuss the limits of what our analysis can reveal about how politically-relevant information travels through social networks.

6.1 Transmission through first-degree connections

Information transmission between directly-connected individuals likely requires that the intermediary internalizes unemployment shocks similarly to the ultimate recipient. It is difficult to see how a respondent could be sensitive to unemployment shocks experienced by people outside their direct network without such a chain of events.

To examine whether such transmission is plausible, we would ideally estimate the effects of the same unemployment shocks examined above on the intermediary j linking respondent i and their second-degree connection k. Unfortunately, very few of these intermediaries were also participants

in our panel survey. Accordingly, in the spirit of two-sample instrumental variable techniques (see Angrist and Krueger 1992; Inoue and Solon 2010), we instead use the first-degree link between i and j to substitute for the first-degree link between j and k that we would ideally estimate. More precisely, if i and j links and j and k links are independently sampled from the same population, then we will obtain the same estimates in expectation (Inoue and Solon 2010). Table 1 shows that our respondents' first-degree connections are broadly similar to both our respondents and their second-degree connections as required.[21] We then approximate the first step in transmission of information via direct connections by estimating the following equation using OLS:

$$Y_{iwoept} = \beta\, Unemployment\ shock_{jp't} + \gamma_{wt} + \delta_{ot} + \eta_{et} + \mu_{pt} + \varepsilon_{ijwoepp't}, \tag{2}$$

where $Unemployment\ shock_{jp't}$ is now an unemployment shock to connection j, a first-degree connection of i located in parish $p' \neq p$. The fixed effect structure is analogous to equation (1), while we similarly remove first-degree connections located in the same municipality as a respondent.

The results reported in Table 4 support the information transmission mechanism through first-degree connections. Specifically, first-degree unemployment shocks invariably influence respondents in the same direction as in Table 2, and most associations are similarly statistically significant. The key difference is with respect to magnitude: the effects of an unemployment shock to a first-degree connection on unemployment concerns, social policy preferences, and vote choices are approximately five times greater. In contrast, the effects of an unemployment shock to first- and second-degree connections on a respondent's national unemployment outlook are similar in magnitude. This difference suggests that the differences in magnitude between first- and second-degree shocks cannot be entirely attributed to information decay arising from the greater probability that i learns about a shock to j than i learns about a shock to k through j.[22]

[21] A full instrumental variables approach could further weight the data to match the sample moments defining the j-k links.

[22] A model of diffusion with decay would similarly predict magnified responses, but would also imply that national outlook responses should also be larger.

Table 4: Estimates of first-degree connection unemployment shocks on respondent economic and political perceptions and preferences

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market-based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployment shock	0.0170***	0.0011	0.0009	0.0130**	0.0139*	0.0152	0.0111*	0.0231**
	(0.0052)	(0.0010)	(0.0011)	(0.0057)	(0.0071)	(0.0119)	(0.0057)	(0.0092)
Observations	3,532,009	2,635,040	1,763,837	3,532,009	3,532,009	896,969	3,532,009	1,059,267
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.1247	0.0839	0.0761	0.2954	0.3737	0.4088	0.4651	0.5336
Outcome standard deviation	0.2479	0.0530	0.0484	0.4562	0.4838	0.4916	0.4988	0.4989
Unemployment shock mean	0.0159	0.0157	0.0154	0.0159	0.0159	0.0166	0.0159	0.0158
Survey years unavailable		2010	2010, 2012			2011, 2012, 2013		2010, 2011

Notes: All specifications are estimated using OLS, and include respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. All observations are inversely weighted by the respondent's number of first-degree connections in that year. Standard errors are double clustered by respondent municipality and first-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

One possible explanation is that the differential response between subjective expectations and preferences and aggregate perceptions reflects the likelihood that first-degree connections are more similar to respondents than second-degree connections. Consequently, respondents may be more sensitive to unemployment shocks afflicting first-degree connections because shocks to *similar* people cause respondents to substantially update beliefs about their own risks, while *any* unemployment shock is relevant when inferring national aggregates. We further test this interpretation by next examining whether respondents are indeed most responsive to shocks to those similar to them.

6.2 Preferences and voting behavior are motivated by informed self-interest

The greater effects of unemployment shocks afflicting direct friends supports the information transmission mechanism, but may also indicate that voters differentiate information about unemployment shocks on the basis of their implications for their own prospects. We test the argument that unemployment shocks to similar people provide a stronger signal of an individual's prospects by estimating the following specifications:

$$\begin{aligned} Y_{iwoept} &= \alpha \textit{Unemployment shock}_{kp't} + \beta \textit{Similarity}_{ikt} + \\ &\quad \gamma(\textit{Unemployment shock}_{kp't} \times \textit{Similarity}_{ikt}) + \gamma_{wt} + \delta_{ot} + \eta_{et} + \mu_{pt} + \varepsilon_{ikwoepp't}, \quad (3) \end{aligned}$$

where $\textit{Similarity}_{ikt}$ captures i and k being either in the same two-digit industry or one-digit occupation.[23]

The results in Table 5 indicate that voters indeed respond more to shocks afflicting second-degree connections similar to themselves, principally those working within the same industry.

[23]The analogous approach for first-degree connections yields broadly similar results. However, it is under-powered by the fact that—by definition—a large majority of first-degree connections are from the same group. For similar reasons, we do not present estimates for similar education grouping because 88% of second-degree connections are classified in the same way as our respondents.

Table 5: Heterogeneity of second-degree connection unemployment shocks on respondent economic and political perceptions and preferences, by similarity of respondent and second-degree connection

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market-based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: same two-digit industry as second-degree connection								
Unemployment shock	0.0020*	0.0010***	0.0012**	0.0015	0.0010	0.0002	0.0028	0.0028
	(0.0012)	(0.0003)	(0.0005)	(0.0018)	(0.0016)	(0.0026)	(0.0018)	(0.0025)
Same industry	-0.0135***	0.0009**	0.0004	-0.0071**	-0.0003	-0.0029	0.0025	0.0056
	(0.0024)	(0.0004)	(0.0004)	(0.0032)	(0.0038)	(0.0064)	(0.0035)	(0.0058)
Unemployment shock	0.0060**	0.0000	0.0002	0.0069**	0.0016	0.0057	0.0046	0.0104*
× Same industry	(0.0024)	(0.0005)	(0.0009)	(0.0033)	(0.0032)	(0.0061)	(0.0033)	(0.0063)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Same industry mean	0.3619	0.3671	0.3701	0.3619	0.3619	0.3463	0.3619	0.3566
Panel B: same one-digit occupation as second-degree connection								
Unemployment shock	0.0025**	0.0008***	0.0012**	0.0019	0.0006	0.0008	0.0049**	0.0058***
	(0.0010)	(0.0003)	(0.0005)	(0.0018)	(0.0018)	(0.0029)	(0.0019)	(0.0022)
Same occupation	-0.0074***	0.0011***	0.0005	-0.0003	-0.0030	-0.008*	0.0005	0.008*
	(0.0019)	(0.0004)	(0.0004)	(0.0033)	(0.0035)	(0.0048)	(0.0029)	(0.0041)
Unemployment shock	0.0040*	0.0004	0.0001	0.0049	0.0030	0.0018	-0.0050	-0.0029
× Same occupation	(0.0023)	(0.0004)	(0.0007)	(0.0034)	(0.0034)	(0.0055)	(0.0038)	(0.0039)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Same occupation mean	0.3922	0.3928	0.3953	0.3922	0.3922	0.3903	0.3922	0.3774

Notes: All specifications are estimated using OLS, and include respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. All observations are inversely weighted by the respondent's number of second-degree connections in that year. Standard errors are double clustered by respondent municipality and second-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Comparing the estimates in the first and third rows of panel A shows that the effects of unemployment shocks to second-degree connections within the same two-digit industry category on subjective unemployment expectations, support for more generous unemployment insurance, and having voted for left-wing parties are 3-4 times greater than unemployment shocks to dissimilar second-degree connections. In contrast, shocks to dissimilar second-degree connections do not significantly influence political preferences. National unemployment perceptions again paint a stark contrast, as respondents update equally from all unemployment shocks to all types of second-degree connections. This lack of distinction reinforces the finding above that respondents update equally about aggregate employment from shocks to first- and second-degree connections becoming unemployed. Panel B reports similar—but less precise—results for similar occupations, although differential responses are no longer apparent for voting. Together, these results indicate that voters distinguish the relevance of different types of information received through directly-connected individuals, particularly with respect to industry of employment.

Given that policy and political responses are concentrated among unemployment shocks to similar individuals, these findings suggest that voter responses are primarily motivated by self-interest. Put differently, only information transmitted through social networks that affects a respondent's own prospects induces a political reaction. In contrast with sociotropic accounts, Table 5 demonstrates that all types of unemployment shocks impact national unemployment perceptions, but only shocks to similar second-degree connections affect political preferences. Furthermore, Appendix Table A7 shows that respondents update their *personal* outlook less from unemployment shocks afflicting second-degree connections that frequently become unemployed. In addition to highlighting voters' self-interest, this additional result is inconsistent with the information decay story: while decay predicts that the interaction with frequency of unemployment shock should be constant across outcomes,[24] our results again show that subjective unemployment concerns and

[24]Cases of frequent unemployment could be discussed more or less among directly-connected individuals, but an informational decay explanation implies that the decay should constantly affect all outcomes.

support for more generous unemployment insurance are sensitive to the unemployment history of the person affected, while aggregate unemployment expectations are not. In sum, these findings support a self-interest-based interpretation of voter preferences, whereby information transmitted through individuals with a direct connection about second-degree connections increase a voter's own concern about unemployment, which is reflected in a stronger preference for left-wing policies and political parties.

6.3 Discussion

Our main results show that Danish voters' economic and political beliefs and preferences are heavily influenced by unemployment shocks afflicting second-degree connections. Furthermore, our evidence examining the mechanisms further suggests that this information is transmitted through the individuals connecting respondents to second-degree connections, that voters update their beliefs in a logical fashion (upweighting similar types when considering their own unemployment prospects, but not doing this when forming aggregate unemployment projections), and base their policy preferences and voting behavior primarily on their subjective concerns.

It is difficult to see how social conformity could solely account for these findings. A strictly social conformity explanation would require that the unemployment shock to a second-degree connection changes their behavior in a way that alters the social expectations governing the behavior of a first-degree connection in the presence of our respondent, when the second-degree connection suffering the shock is not present. This alternative account relies on no information being transferred between individuals at either step in the chain, only changes in behavior.

However, it is unlikely that k can change social norms in the networks that j and k share in a way that influences norms in the networks that i and j share. This is especially unlikely under our design because k and j *and* j and i live in different locations, and thus either k and j or j and i are unlikely to frequently interact in person—the most plausible source of pressure to conform. Furthermore, the social conformity explanation struggles to explain why respondents react more

to shocks afflicting second-degree connections in the same industry, given that the knowledge of their similarity does not arise from direct interaction. Social conformity could drive this result if j reacts more to, or engages in more, pressure when they work in the same industry as i and k. However, Appendix Table A8 demonstrates that respondents still differentially update their subjective concerns and political preferences from shocks afflicting similar k's even after controlling for the interaction between an unemployment shock and j and k being in the same industry. Moreover, j and k being in the same industry does not differentially affect our respondents' beliefs, preferences and behavior. It is thus hard to explain how social conformity could differentially influence our respondents when j is in a different industry from i and k without j informing i that k was in the same industry. For the same reasons, it is hard to explain our findings through the lens of coordination theories that do not themselves at least in part rely on some information transmission. There are thus good reasons to believe that our findings principally reflect information transmission within social networks.

A notable feature of our findings is the magnitude of voter updating. Since voter prior beliefs—even about their own industry—are often imprecise and based on information that may have gone out of date, we do not find this to be particularly surprising. A large literature shows that information acquired through social networks (e.g. Sokhey and McClurg 2012), the media (see DellaVigna and Gentzkow 2010), or non-governmental organizations (see Pande 2011) can significantly alter voter beliefs. Moreover, standard learning models even show that voters will update away from a correct prior belief if there is uncertainty about this belief and the signal received is sufficiently credible. Nevertheless, transmission of unemployment shocks through direct connections could accentuate responses to new information by also inducing a psychological reaction reflecting anxiety (e.g. Lerner and Keltner 2001; Marcus 2010). If such an emotional response is likelier to emanate from shocks affecting someone that the respondent actually knows, and to dissipate relatively quickly, our estimates of the medium-term effects of shocks afflicting people that individuals only know indirectly are unlikely to be explained by this alternative mechanism.

An important question beyond the scope of this study is how, and what type of, information is transmitted within social networks. There are major benefits to our design with respect to plausibly isolating exogenous variation in unemployment shocks that could only reach an individual via at least some information transmission through their social networks. Our administrative data also make this possible at an unprecedented scale and level of detail. A downside, however, is that the exact nature of what is transferred is "black-boxed." Specifically, we cannot discern what second-degree connections communicate to a respondent's first-degree connections, how information is parsed by these intermediary connections, and what politically-relevant discussions arise between our respondents and their first-degree connections as a consequence of the second-degree connections becoming unemployed. It is therefore not clear whether changes in second-degree connections' economic beliefs and political preferences, or just the information about unemployment shocks themselves, induce the changes we observe among voters two degrees of separation away.[25]

Nevertheless, our analysis yields some suggestive insights. Table 4 shows that first-degree connections alter their beliefs and preferences similarly to our respondents, which suggests that unemployment information is transferred through both interactions. Without this, it is unclear why the respondent's unemployment expectations would change. Moreover, the greater effect of shocks to similar second-degree connections further suggests that the unemployment information transmitted is likely to at least partially be individual- or industry-specific, rather than simply referring to aggregate levels. However, it is less clear whether political views are transferred, or whether individuals reach similar conclusions from the same unemployment information. Ultimately, disentangling this two-step process is almost impossible to fully navigate in observational studies leveraging a single source of exogenous variation.

[25]We thus do not adopt an instrumental variables strategy where the unemployment shock instruments for a peer's unemployment expectations (Bramoullé, Djebbari and Fortin 2009), given that the exclusion restriction may be violated.

7 Conclusion

We argue that the transmission of information within social networks plays a key role in shaping economic and policy beliefs and preferences, and ultimately voting behavior. Combining Denmark's extraordinarily detailed individual-level data with an identification strategy exploiting unemployment shocks to second-degree connections, we circumvent the perennial identification challenges faced by previous studies investigating the impact of information diffusion within social networks. Our findings show that voters are highly responsive to unemployment shocks afflicting second-degree connections, updating both beliefs about their personal unemployment risk and national unemployment levels. However, while perceptions of national aggregates respond to any person becoming unemployed, self assessments are only responsive to shocks afflicting those in the same industry. Consistent with a self-interested outlook, voters only alter their policy preferences and vote choices in response to concerns close to home. This induces them to ultimately support more generous unemployment insurance and vote for left-wing political parties.

These findings add to the growing literature examining the role of social networks in political life. In particular, we provide—to the best of our knowledge—the first strong evidence that networks change beliefs and behavior by diffusing politically-relevant information. To the extent that the information transmitted is credible and properly processed, this diffusion function should enable voters to make better-informed decisions. It is important to emphasize that this does not challenge the social conformity channel frequently emphasized in the academic literature.

Our study faces two notable limitations. First, although this study represents a rare opportunity to employ data that is both detailed *and* may generalize about a major global phenomenon (Pietryka and DeBats 2017), our findings are nevertheless based on post-financial crisis Denmark. This context could be somewhat unique to the extent that the uptick in unemployment around our surveys was exceptional and Danish politics was remarkably competitive and oriented around economic issues. On the other hand, Denmark's political and labor market institutions and expe-

riences with the financial crisis are similar to other Western European nations. Further studies are ultimately required to assess the extent to which information diffusion through networks operates in other contexts, affects a wider range of political behaviors, and compares with social pressure in terms of influence.

Second, as we note in the discussion above, a key limitation demanding future research is the question of what types of information is transmitted between directly-connected individuals. Qualitative studies in the United States observing political discussion in small communities (e.g. Walsh 2004) suggest one valuable blueprint. Such studies could be complemented by experiments focusing on small groups in controlled settings where communication and beliefs can be carefully monitored (e.g. Klar and Shmargad forthcoming). Although both approaches may entail trading off detailed contextual information for external validity, progress in examining how and what information is passed through social networks relies on understanding these processes in detail.

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A Appendix

Contents

A.1 Unemployment insurance in Denmark

Danish citizens are entitled to a means-tested government benefit of around US$1,650 per month, with a supplement for those with children and a further supplement for single parents. Legislation coming into force in 2012 made immigrants eligible for the standard unemployment benefit. Until January 2012, unemployed immigrants were also subject to a special transfer which was lower than the standard transfer.[1] A lower transfer for immigrants was reinstated in September 2015. Educated people below the age of 30 receive a lower benefit equal to the monthly government student transfer (around US$1,150), which is further reduced for uneducated people below the age of 30.[2]

Workers can also enter a voluntary unemployment insurance system. This is principally financed by members (a flat fee independent of income covering two thirds of the expenses), but also supported by the government (one third of the costs). Members of this insurance system receive benefits of around 90% of an individual's pre-unemployment wage up to a threshold of around US$35,000, beyond which compensation is capped. This threshold is lower for people below the age of 25: the rate for graduates is 71.5% of the standard rate (82% if they have children), and the rate for people below age 25 is 50% of the standard rate. The maximum duration of such unemployment insurance was four years until July 2012, when it was contentiously reduced to two years. To receive unemployment insurance (and to regain the right to receive the transfer), one must have worked sufficient hours to equal one year's full time employment (1,924 hours) within the last three years, stay in Denmark permanently, and be actively looking for a job. When unemployment insurance expires, the unemployed remain eligible for the means-tested government transfer which has no maximum duration.

[1] This applied to everyone who had not been a resident of the European Union in a minimum of 7 years within the last 8 years.

[2] This age limit for a special student transfer was increased from 25 to 30 years old by an unemployment benefits reform agreed on in June 2013 and implemented in January 2014.

A.2 Accessing the Danish register data

The register data set used in this paper is based on several Danish administrative registers which are merged using social security numbers. Physically, these administrative micro data are located on specific computers at Statistics Denmark and may not be transferred to computers outside Statistics Denmark due to data security considerations. Researchers and their research assistants are allowed to use these data if their research project is approved by Statistics Denmark and if they are affiliated with a research institution accepted by Statistics Denmark. Access to the data at Statistics Denmark is provided through the internet. At the moment, researchers or their assistants are only allowed access to these data from research institutions in Denmark. If a researcher at a university or other research institution outside Denmark wishes to use these data, this may be accomplished by visiting a Danish research institution or by cooperating with researchers or research assistants working in Denmark. If researchers want to analyze our data for replication purposes, we will provide guidance with regard to getting a project approval from Statistics Denmark. The replication code will be provided online upon publication.

A.3 Computation of second-degree connections

The starting point to compute second-degree connections in the Danish population is the symmetric adjacency matrix that captures its first-degree networks connections. Each entry is either 0 or 1, indicating a tie between i and j. In practice this was computed by first associating i with an educational institution, work institution (from within 2 years and satisfying our other restrictions), parent or partner, and then generating second-degree connections through the process described below. In the case of parents and partners, first-degree connections were also retained.

To illustrate our computation, consider a 5×5 adjacency matrix $\mathbf{g}$ relating 5 individuals to each other. In our example, persons 1 and 2, 1 and 4, 2 and 3, and 3 and 5 are all first-degree connections. Matrix multiplying $\mathbf{g}$ with itself produces a matrix containing the number of second-

degree connection between each pair of individuals, except along the diagonal, which gives the network degree or the number of first-degree connections to other individuals. Finally, we define our second-degree matrix **S** as a matrix of indicators for second but not first-degree connections between each pair of individuals, with the diagonal set to zero. In this example, there are 3 second-degree connections, between individuals 1 and 3, 2 and 5, and 2 and 4. In the Danish population, the number is far higher because the average individual has approximately 150^2 second-degree connections.[3]

$$\mathbf{g} \equiv \begin{bmatrix} 0 & 1 & 0 & 1 & 0 \\ 1 & 0 & 1 & 0 & 0 \\ 0 & 1 & 0 & 0 & 1 \\ 1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 0 \end{bmatrix} \quad \mathbf{g}'\mathbf{g} = \begin{bmatrix} 2 & 0 & 1 & 0 & 0 \\ 0 & 2 & 0 & 1 & 1 \\ 1 & 0 & 2 & 0 & 0 \\ 0 & 1 & 0 & 1 & 0 \\ 0 & 1 & 0 & 0 & 1 \end{bmatrix} \quad \mathbf{S} = \begin{bmatrix} 0 & 0 & 1 & 0 & 0 \\ 0 & 0 & 0 & 1 & 1 \\ 1 & 0 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 & 0 \end{bmatrix}$$

A.4 Industry, occupation, and education digits

Table A1 shows the full one-digit classification by industry, occupation, and education used in this paper. The two-digit industry classification we use in the paper are available online;[4] we omit the full list for brevity.

A.5 Variable definitions

Own unemployment expectation. The probability, as a fraction, assigned by the respondent to the possibility that they will experience a period of unemployment in the forthcoming year. Respondents were asked the following question: "What is your assessment of the probability that you will experience a period without a job during the year of [current year]? I would like you to

[3] The number is in practice slightly lower because some connections are shared.
[4] See http://www.dst.dk/en/Statistik/Publikationer/VisPub?cid=22256.

Table A1: Industry, occupation, and education one-digit classifications

Industry one-digit classification
1: agriculture, fishery
2: industry
3: construction
4: trade and transport
5: information and communication
6: finance and insurance
7: real estate and rental service
8: service business
9: public administration, teaching, and health care
10: culture and other services

Occupation one-digit classification
1: military
2: management
3: work that requires knowledge at the highest level within that field
4: work that requires knowledge at the intermediate level within that field
5: office work, customer service
6: service and sales
7: agriculture, fishery
8: craftsman
9: machine operator, installation, transportation
10: other manual work

Education one-digit classification
1: primary school
2: regular high school
3: business high school
4: vocational school
5: short higher education
6: intermediate higher education
7: bachelor's degree
8: long higher education (university)
9: research
10: none

provide a number between 0 and 100, where 0 means that you think that the event certainly does not occur and 100 means that you think that the event certainly occurs."

Guess national unemployment rate. Respondent's answer (given as a fraction, not a percentage) to the question "Unemployment in Denmark is typically measured by the unemployment rate, that is, the share of people who want to work but don't have a job. Over the last 25 years, the unemployment rate has been between 1.5 and 12%. What is your estimate of the current unemployment rate in Denmark? We would like your best estimate, even if you are not entirely sure."

National unemployment rate expectation. Respondent's answer (given as a fraction, not a percentage) to the question "What is your best estimate of what unemployment will be in 2013? We would like your best estimate, even if you are not entirely sure."

Want more unemployment insurance. An indicator coded 1 for respondents that registered 1 in response to the following question: "The economic crisis has caused many people to lose their jobs. Do you think that the Government should support those who become unemployed: 1. More than they do now, 2. Less than they do now 3. The same as they do now."

Government should support the poor. An indicator coded 1 for respondents that registered 2 or less in response to the following scale: "Some think the Government should do all it can to raise the standard of living for poor Danes: that is 1 on the scale. Others think it is not the responsibility of government, each should take care of themselves: that is 5."

Support non-market-based solutions. An indicator coded 1 for respondents that answered 2 or 5 in response to the following question: "If politicians were to implement yet another policy to mitigate the effects of the economic crisis, which type of policy would you then prefer: 1. Tax cuts, 2. Public investments, 3. Support to firms, 4. Temporary VAT cuts, 5. Temporary higher unemployment benefits, 6. None of these policies."

Intend to vote for left party. An indicator coded 1 for respondents that report intending to vote for a left party (Social Democrats, Social Liberals, Socialist People's, or Red-Green parties). Respondents were asked "How would you vote tomorrow?"

Intend to vote for an incumbent party. An indicator coded 1 for respondents that report intending to vote for an incumbent party (Venstre (Liberal Party of Denmark) or The Conservative People's Party for the 2010 and 2011 surveys, and Social Democrats, Social Liberals, or Socialist People's parties for the 2012 and 2013 surveys). Respondents were asked "How would you vote tomorrow?"

Voted for left party in 2011. An indicator coded 1 for respondents that reported voting for a left party (Social Democrats, Social Liberals, Socialist People's, of Red-Green parties) in the previous election.

Unemployment shock. An indicator coded 1 for second-degree connections of a respondent that were registered as unemployed in the November preceding the survey, but were not registered as unemployment in the prior November.

Woman. An indicator coded 1 for female respondents.

Age. The respondent's age in years.

Children. The number of children that a respondent has.

Single. An indicator coded 1 for respondents that are single.

Gross income (DKK). Total annual income, including wage income, government transfers and capital income.

Total assets (DKK). Value of total assets, including bank deposits, bonds, stocks, and property.

Total debt (DKK). Value of total debt, including bank loans, credit card debt and mortgage debt.

Homeowner. An indicator coded 1 for respondents that own property.

Education basic. An indicator coded 1 for respondents that have completed high school or less.

Education short. An indicator coded 1 for respondents that have completed vocational school.

Education medium. An indicator coded 1 for respondents that have a bachelor's degree.

Education long. An indicator coded 1 for respondents that have a master's degree or a PhD.

Unemployed. An indicator coded 1 for respondents that are unemployed.

Second-degree network size. Number of second-degree connections for a respondent (that survive our restrictions).

Same industry. An indicator coded 1 for respondents in the same two-digit industry as their second-degree connection.

Same occupation. An indicator coded 1 for respondents in the same one-digit occupation as their second-degree connection.

First-degree familial/educational/vocational connection. An indicator coded 1 for respondents who are connected to second-degree connections by a familial/educational/vocational tie.

Second-degree familial/educational/vocational connection. An indicator coded 1 for peers (of our respondent) who are connected to second degree connections (of our respondent) by a familial/educational/vocational tie.

A.6 Additional results

In this section we present various additional results cited in the main paper.

A.6.1 Incumbent party vote intention

Table A2 examines the effect of unemployment shock on intention to vote for an incumbent party. Contrary to sociotropic accounts, the results show that unemployment shocks do not significantly affect support for the incumbent party. Unlike the 2011 vote choice, this outcome is particularly helpful in separating self-interest and sociotropic explanations because the vote intention variable extends across center-right and center-left governments.

A.6.2 Restricting the size of second-degree networks

Table A3 reports the results when all respondents with more than 5,000 second-degree connections are removed. After this restriction, we have 5,325 unique i's and 10,155 unique i-year observa-

Table A2: Estimates of second-degree connection unemployment shocks on intention to vote for a party from the governing coalition

	Intend to vote for an incumbent party (1)
Unemployment shock	-0.0014
	(0.0015)
Observations	140,509,875
Outcome range	{0,1}
Outcome mean	0.4799
Outcome standard deviation	0.4996
Unemployment shock mean	0.0185

Notes: Specification is estimated using OLS, and includes respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. All observations are inversely weighted by the respondent's number of second-degree connections in that year. Standard errors are double clustered by respondent municipality and second-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

tions. While this substantially reduces the sample size, the results are broadly similar in magnitude and generally remain statistically significant. Beyond the inverse weighting scheme employed, these results suggest that our main findings are not driven by individuals with large second-degree networks.

A.6.3 Removing second-degree connections located in the same municipality as the first-degree connection linking them to our respondent

Our main estimates exclude k's located in the same parish as the j (or j's) that link them to i. To further address the potential concern that j experiences shocks correlated with k, Table A4 excludes k's that are linked to i through a j located within the same municipality as k. Although this further reduces our sample, the magnitudes of our estimates are similar, and thus suggest that such correlated shocks are not driving our findings.

Table A3: Estimates of second-degree connection unemployment shocks on respondent economic and political perceptions and preferences, maximum second-degree network of 5,000 connections

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market-based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployment shock	0.0027**	0.0011***	0.0015**	0.0021	0.0018	0.0006	0.0058***	0.0066**
	(0.0014)	(0.0004)	(0.0006)	(0.0019)	(0.0022)	(0.0038)	(0.0022)	(0.0030)
Observations	21,115,818	15,550,408	10,403,150	21,115,818	21,115,818	5,565,410	21,115,818	6,169,746
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.1841	0.0853	0.0754	0.3184	0.3781	0.4013	0.4040	0.4838
Outcome standard deviation	0.3084	0.0552	0.0488	0.4658	0.4849	0.4902	0.4907	0.4997
Unemployment shock mean	0.0231	0.0223	0.0222	0.0231	0.0231	0.0253	0.0231	0.0229
Survey years unavailable		2010	2010, 2012			2011, 2012, 2013		2010, 2011

Notes: All specifications are estimated using OLS, and include respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. All observations are inversely weighted by the respondent's number of second-degree connections in that year. Standard errors are double clustered by respondent municipality and second-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A4: Estimates of second-degree connection unemployment shocks on respondent economic and political perceptions and preferences, removing second-degree connections located in the same municipality as the first-degree connection linking them to our respondent

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market-based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployment shock	0.0030***	0.0009***	0.0011***	0.0025	0.0015	0.0012	0.0036**	0.0053**
	(0.0010)	(0.0003)	(0.0004)	(0.0016)	(0.0014)	(0.0027)	(0.0017)	(0.0026)
Observations	118,526,080	88,686,037	58,393,777	118,526,080	118,526,080	29,840,043	118,526,080	23,584,797
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.1390	0.0848	0.0758	0.3145	0.3888	0.4213	0.4793	0.5431
Outcome standard deviation	0.2648	0.0545	0.0471	0.4643	0.4875	0.4938	0.4996	0.4981
Unemployment shock mean	0.0191	0.0188	0.0185	0.0191	0.0191	0.0200	0.0191	0.0185
Survey years unavailable		2010	2010, 2012			2011, 2012, 2013		2010, 2011

Notes: All specifications are estimated using OLS, and include respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. All observations are inversely weighted by the respondent's number of second-degree connections in that year. Standard errors are double clustered by respondent municipality and second-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

A.6.4 Heterogeneity by type of tie

Tables A5 and A6 respectively show interactions between unemployment shocks and the type of links between respondent and first-degree connection and between first-degree and second-degree connection. Note that some ties can reflect multiple types. The results, and the F tests at the foot of the table, suggest that different types of tie produce relatively similar effects, although familial ties are perhaps most important—at least in Table A5.

A.6.5 Heterogeneity by frequency of second-degree connection unemployment

Table A7 shows the interaction between unemployment shocks and the number of times that the second-degree connection has become unemployed in the past ten years. The results suggest that, at least for an individual's own unemployment concerns and their support for unemployment insurance, that shocks to individuals likely to become unemployed have weaker effects on respondents. There is no evidence of differential effects on aggregate unemployment projections. This again suggests information decay does not account for these findings, and sociotropic voting does not account for political preferences. Rather the results suggest that voters are self-interested.

A.6.6 Social conformity interpretation check

Table A8 shows the interaction between unemployment shocks and similarity of respondent and second-degree connection, controlling for similarity of first-degree connection and second-degree connection. This tests whether our main similarity finding between i and k remains robust in cases where j and k are also in the same industry. This is an informative check because it is unlikely that a purely social conformity explanation could drive our heterogeneous effects by same industry, given that i and k do not know each other (and presumably that i does not know that k is in the same industry without being informed of this by j), unless j also being in the same industry as i and k enhances the effects of conformity. The results show that the interaction estimates for i-k

Table A5: Heterogeneity of second-degree connection unemployment shocks on respondent economic and political perceptions and preferences, by type of respondent-first-degree connection relationship

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market-based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployment shock	-0.0010	-0.0008	-0.0001	-0.0212**	-0.0033	-0.0111	-0.0117	-0.0187
	(0.0044)	(0.0008)	(0.0009)	(0.0084)	(0.0080)	(0.0155)	(0.0102)	(0.0125)
First-degree familial connection	-0.0027	-0.0018	-0.0018	-0.0374***	-0.0096	0.0103	-0.0243***	-0.0284*
	(0.0051)	(0.0011)	(0.0014)	(0.0087)	(0.0083)	(0.0172)	(0.0094)	(0.0154)
First-degree educational connection	-0.0185***	0.0004	-0.0001	-0.0347***	-0.0117*	0.0012	-0.0144*	-0.0048
	(0.0035)	(0.0009)	(0.0009)	(0.0063)	(0.0065)	(0.0128)	(0.0087)	(0.0139)
First-degree vocational connection	-0.0456***	-0.0016	-0.0004	-0.0353***	-0.0186**	0.0179	-0.0149	-0.0132
	(0.0039)	(0.0010)	(0.0011)	(0.0085)	(0.0076)	(0.0182)	(0.0099)	(0.0169)
Unemployment shock ×	0.0144**	0.0031*	0.0029	0.0223***	0.0032	0.0033	0.0199**	0.0379**
First-degree familial connection	(0.0059)	(0.0019)	(0.0025)	(0.0072)	(0.0110)	(0.0171)	(0.0099)	(0.0149)
Unemployment shock ×	0.0064	0.0019**	0.0009	0.0231***	0.0038	0.0183	0.0122	0.0223*
First-degree educational connection	(0.0039)	(0.0008)	(0.0009)	(0.0078)	(0.0082)	(0.0149)	(0.0104)	(0.0119)
Unemployment shock ×	0.0015	0.0012	0.0007	0.0247***	0.0063	0.0108	0.0149	0.0177
First-degree vocational connection	(0.0048)	(0.0009)	(0.0009)	(0.0085)	(0.0081)	(0.015)	(0.0098)	(0.0116)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	27,960,832
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.1380	0.0849	0.0759	0.3146	0.3893	0.4204	0.4799	0.5444
Outcome standard deviation	0.2638	0.0543	0.0470	0.4644	0.4876	0.4936	0.4996	0.4980
Unemployment shock mean	0.0185	0.0182	0.0179	0.0185	0.0185	0.0194	0.0185	0.0180
First-degree familial connection mean	0.0544	0.0541	0.0550	0.0544	0.0544	0.0553	0.0544	0.0541
First-degree educational connection mean	0.3721	0.3671	0.3693	0.3721	0.3721	0.3868	0.3721	0.3751
First-degree vocational connection mean	0.6366	0.6426	0.6411	0.6366	0.6366	0.6188	0.6366	0.6350
F-test: shock × familial = Shock × education (p-value)	0.0847	0.4335	0.4151	0.8662	0.9283	0.1679	0.2025	0.0825
F-test: shock × familial = Shock × vocational (p-value)	0.0134	0.1721	0.3795	0.6250	0.6025	0.4128	0.3935	0.0425
F-test: shock × educational = Shock × vocational (p-value)	0.0767	0.0430	0.7082	0.6775	0.5135	0.2847	0.4130	0.3594
Survey years unavailable		2010	2010, 2012			2011, 2012, 2013		2010, 2011

Notes: All specifications are estimated using OLS, and include respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. All observations are inversely weighted by the respondent's number of second-degree connections in that year. Standard errors are double clustered by respondent municipality and second-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A6: Heterogeneity of second-degree connection unemployment shocks on respondent economic and political perceptions and preferences, by type of first-degree-second-degree connection relationship

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market-based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployment shock	-0.0066	-0.0001	0.0016	-0.0245***	-0.0110*	-0.0116	-0.0200**	-0.0043
	(0.0041)	(0.0007)	(0.0014)	(0.0056)	(0.0063)	(0.0121)	(0.0080)	(0.0109)
Second-degree familial connection	-0.0235***	-0.0005	0.0006	-0.0312***	-0.0226**	0.0080	-0.0303***	-0.0208
	(0.0041)	(0.0009)	(0.0009)	(0.0070)	(0.0091)	(0.0132)	(0.0095)	(0.0156)
Second-degree educational connection	-0.0314***	-0.0015*	-0.0003	-0.0334***	-0.0208***	-0.0030	-0.0290***	-0.0276**
	(0.0036)	(0.0008)	(0.0007)	(0.0070)	(0.0079)	(0.0112)	(0.0084)	(0.0129)
Second-degree vocational connection	-0.0261***	-0.0008	-0.0003	-0.033***	-0.0208***	0.0025	-0.0269***	-0.0167
	(0.0034)	(0.0008)	(0.0007)	(0.0060)	(0.0073)	(0.0114)	(0.0086)	(0.0124)
Unemployment shock ×	0.0055	0.0005	-0.0018	0.0344***	0.0098	0.0005	0.0297***	0.0267
Second-degree familial connection	(0.0050)	(0.0010)	(0.0017)	(0.0064)	(0.0077)	(0.0167)	(0.0076)	(0.0182)
Unemployment shock ×	0.0108***	0.0011*	0.0000	0.0262***	0.0106	0.0115	0.0266***	0.0150
Second-degree educational connection	(0.0037)	(0.0007)	(0.0008)	(0.0058)	(0.0070)	(0.0125)	(0.0067)	(0.0106)
Unemployment shock ×	0.0102**	0.0011	-0.0006	0.0272***	0.0135**	0.0157	0.0207**	0.0046
Second-degree vocational connection	(0.0042)	(0.0008)	(0.0016)	(0.0053)	(0.0060)	(0.0119)	(0.0083)	(0.0107)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	27,960,832
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.1380	0.0849	0.0759	0.3146	0.3893	0.4204	0.4799	0.5444
Outcome standard deviation	0.2638	0.0543	0.0470	0.4644	0.4876	0.4936	0.4996	0.4980
Unemployment shock mean	0.0185	0.0182	0.0179	0.0185	0.0185	0.0194	0.0185	0.0180
Second-degree familial connection mean	0.0282	0.0284	0.0287	0.0282	0.0282	0.0278	0.0282	0.0276
Second-degree educational connection mean	0.3499	0.3525	0.3524	0.3499	0.3499	0.3420	0.3499	0.3405
Second-degree vocational connection mean	0.6736	0.6719	0.6729	0.6736	0.6736	0.6788	0.6736	0.6842
F-test: shock × familial = Shock × education (p-value)	0.2293	0.5074	0.2634	0.0514	0.8953	0.4899	0.5128	0.4045
F-test: shock × familial = Shock × vocational (p-value)	0.2765	0.2151	0.1020	0.0721	0.4668	0.3800	0.0937	0.2003
F-test: shock × educational = Shock × vocational (p-value)	0.8681	0.9643	0.7048	0.7606	0.4166	0.4778	0.1489	0.1251
Survey years unavailable		2010	2010, 2012			2011, 2012, 2013		2010, 2011

Notes: All specifications are estimated using OLS, and include respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. All observations are inversely weighted by the respondent's number of second-degree connections in that year. Standard errors are double clustered by respondent municipality and second-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A7: Heterogeneity of second-degree connection unemployment shocks on respondent economic and political perceptions and preferences, by frequency of unemployment shocks to second-degree connections

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market-based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployment shock	0.0027	0.0007	0.0008	0.0008	-0.0048**	0.0070	0.0027	0.0020
	(0.0022)	(0.0005)	(0.0007)	(0.0021)	(0.0019)	(0.0052)	(0.0029)	(0.0045)
Shocks experienced by second-degree connection in last 10 years	0.0029***	0.0007***	0.0006***	0.0045***	0.0029***	-0.0003	0.0016	0.0035**
	(0.0006)	(0.0001)	(0.0001)	(0.0011)	(0.0010)	(0.0016)	(0.0011)	(0.0014)
Unemployment shock × Shocks experienced by second-degree connection in last 10 years	-0.0020	-0.0004	-0.0003	-0.0023*	0.0017	-0.0037	-0.0007	-0.0010
	(0.0014)	(0.0003)	(0.0006)	(0.0012)	(0.0015)	(0.0029)	(0.0018)	(0.0029)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.1380	0.0849	0.0759	0.3146	0.3893	0.4204	0.4799	0.5415
Outcome standard deviation	0.2638	0.0543	0.0470	0.4644	0.4876	0.4936	0.4996	0.4983
Unemployment shock mean	0.0185	0.0182	0.0179	0.0185	0.0185	0.0194	0.0185	0.0183
Shocks experienced by second-degree connection in last 10 years mean	0.1630	0.1648	0.1610	0.1630	0.1630	0.1577	0.1630	0.1588
Shocks experienced by second-degree connection in last 10 years standard deviation	0.4591	0.4606	0.4551	0.4591	0.4591	0.4543	0.4591	0.4527
Survey years unavailable		2010	2010, 2012			2011, 2012, 2013		2010, 2011

Notes: All specifications are estimated using OLS, and include respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. All observations are inversely weighted by the respondent's number of second-degree connections in that year. Standard errors are double clustered by respondent municipality and second-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

similarity are similar to those in Table 5, and thus support the information transmission interpretation. Although the interactions in columns (4) and (8) are no longer statistically significant, they remain relatively large and positive. Moreover, the interaction between j and k never significantly increases the effect of an unemployment shock, further suggesting that social conformity driven by j differentially changing their behavior when a k in their industry becomes unemployed is unlikely to explain our findings.

Table A8: Heterogeneity of second-degree connection unemployment shocks on respondent economic and political perceptions and preferences, by similarity of respondent and second-degree connection, controlling for similarity of first-degree connection and second-degree connection

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market-based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployment shock	0.0029*	0.0010**	0.0013*	-0.0009	0.0015	-0.0047**	0.0041**	0.0016
	(0.0015)	(0.0004)	(0.0008)	(0.0019)	(0.0018)	(0.0023)	(0.0018)	(0.0035)
Same industry (*i*-*k*)	-0.0167***	0.0008*	0.0005	-0.0057	0.0002	-0.0041	0.0024	0.0082
	(0.0028)	(0.0005)	(0.0004)	(0.0038)	(0.0044)	(0.0073)	(0.0041)	(0.0067)
Unemployment shock	0.0058*	0.0000	0.0005	0.0040	0.0026	-0.0015	0.0065*	0.0082
× Same industry (*i*-*k*)	(0.0030)	(0.0006)	(0.0008)	(0.0037)	(0.004)	(0.0063)	(0.0039)	(0.0071)
Same industry (*j*-*k*)	0.0072***	0.0002	-0.0002	-0.0031	-0.0011	0.0026	0.0003	0.0020
	(0.0018)	(0.0003)	(0.0004)	(0.0027)	(0.0031)	(0.0053)	(0.0035)	(0.0045)
Unemployment shock	-0.0003	-0.0001	-0.0006	0.0062**	-0.0019	0.0153***	-0.0039	0.0040
× Same industry (*j*-*k*)	(0.0027)	(0.0005)	(0.0009)	(0.0029)	(0.0038)	(0.0053)	(0.0027)	(0.0056)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	27,960,832
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.1380	0.0849	0.0759	0.3146	0.3893	0.4204	0.4799	0.5444
Outcome standard deviation	0.2638	0.0543	0.0470	0.4644	0.4876	0.4936	0.4996	0.4980
Unemployment shock mean	0.0185	0.0182	0.0179	0.0185	0.0185	0.0194	0.0185	0.0180
Same industry i-k mean	0.3619	0.3671	0.3701	0.3619	0.3619	0.3463	0.3619	0.3569
Same industry i-k mean	0.6076	0.6094	0.6120	0.6076	0.6076	0.6026	0.6076	0.6104
Survey years unavailable		2010	2010, 2012			2011, 2012, 2013		2010, 2011

Notes: All specifications are estimated using OLS, and include respondent-level industry-year, occupation-year, education-year, and parish-year fixed effects. All observations are inversely weighted by the respondent's number of second-degree connections in that year. Standard errors are double clustered by respondent municipality and second-degree connection municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.