DIFFUSING POLITICAL CONCERNS: HOW UNEMPLOYMENT INFORMATION PASSED BETWEEN SOCIAL TIES INFLUENCES DANISH VOTERS *

JAMES E. ALT †
AMALIE JENSEN ‡
HORACIO A. LARREGUY §
DAVID D. LASSEN ¶
JOHN MARSHALL ‖

APRIL 2020

While social pressure is widely believed to influence voters, evidence that information passed between social ties affects beliefs, policy preferences, and voting behavior is limited. We investigate whether information about unemployment shocks diffuses through networks of strong and mostly weak social ties and influences voters in Denmark. We link surveys with population-level administrative data that logs unemployment shocks afflicting respondents' familial, vocational, and educational networks. Our results show that the share of second-degree social ties—individuals that voters learn about indirectly—that became unemployed within the last year increases a voter's perception of national unemployment, self-assessed risk of becoming unemployed, support for unemployment insurance, and voting for left-wing political parties. Voters' beliefs about national aggregates respond to all shocks equally, whereas subjective perceptions and preferences respond primarily to unemployment shocks afflicting second-degree ties in similar vocations. This suggests that information diffusion through social ties principally affects political preferences via egotropic—rather than sociotropic—motives.

*We thank Allison Carnegie, Charlotte Cavaille, Bob Erikson, Alex Hertel-Fernandez, Thomas Groll, Torben Iversen, Dipali Mukhopadhyay, Carlo Prato, Tonya Putnam, Philipp Rehm, Oskar Nordström Skans, Brandon Stewart, Edoardo Teso, Yiqing Xu, and participants at APSA, Berkeley, Buffalo, Columbia, Copenhagen, Emory, EPSA, MIT, MPSA, and Stockholm for excellent feedback and advice. Financial support from a Sapere Aude grant from the Danish Council for Independent Research and an ERC Starting Grant (#313673) are gratefully acknowledged.

†Department of Government, Harvard University, jalt@iq.harvard.edu.
‡Department of Economics and Woodrow Wilson School, Princeton University, ajensen@princeton.edu.
§Department of Government, Harvard University. hlarreguy@fas.harvard.edu.
¶Department of Economics, University of Copenhagen, david.dreyer.lassen@econ.ku.dk.
‖Department of Political Science, Columbia University. jm4401@columbia.edu.

1 Introduction

It is widely believed that social networks—the web of strong ties that individuals interact with regularly and weak ties that individuals interact with occasionally—play a central role in explaining economic and social outcomes (e.g. Banerjee et al. 2013; Carrell, Sacerdote and West 2013; Chetty, Hendren and Katz 2016; Sacerdote 2001). Indeed, it is hard to overstate the potential importance of the composition and structure of the networks in which people spend many 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 two channels. First, social ties may exert powerful pressures to conform with or coordinate around norms of political engagement through explicit threats or learned norms (e.g. Bond et al. 2012; Gerber, Green and Larimer 2008; Larson 2017; Marshall 2019; Nickerson 2008; Siegel 2009). This pressure appears to be exerted predominantly by an individual's few strong ties (Sinclair 2012). Second, social networks may diffuse information that influences citizens' attitudes and voting behavior (e.g. Carlson 2019; Huckfeldt and Sprague 1995; Katz and Lazarsfeld 1955). The most novel information, relating to events beyond a voter's own experiences, may be conveyed by an individual's many weaker ties that possess more distinctive social networks (Granovetter 1973).

While the political significance of networks' social pressure function among strong ties is now well-established, the information diffusion role of social networks has received limited rigorous empirical attention. This in part stems from the difficulty of reliably mapping large networks of weaker ties (Eagle, Pentland and Lazer 2009) and the information possessed by each node. Nevertheless, since voters—even in the world's most politically engaged democracies—are often poorly informed about their economic and political environment, cheap and frequent access to information through social ties has the potential to substantially affect voters' beliefs, policy preferences, and voting behavior. Beyond the academic value of distinguishing between the diffusion and social pressure mechanisms, establishing the importance of information diffusion within social networks

may guide how political parties and NGOs should target their information campaigns and illuminate whether voters cast their ballots based on aggregated information.

This article examines how the diffusion of unemployment information between social ties affects voters' perceptions of the national economy and their own unemployment prospects, policy preferences, and voting behavior. In the aftermath of the 2008 financial crisis, we estimate the diffusion effects of other individuals becoming unemployed within the last year in Denmark. In a context where policies to address unemployment were politically salient, the diffusion of information relating to such unemployment shocks—which incorporate any downstream consequences of this experience, including the possibility that some of those individuals may regain employment—could alter political attitudes through two main channels. On one hand, "egotropic" interests may drive a voter's economic and social policy preferences (e.g. Iversen and Soskice 2001; Margalit 2013; Moene and Wallerstein 2001; Rehm 2011*b*). This implies that information inducing individuals to believe that they personally face greater unemployment risks will lead them to support more generous social insurance and the left-wing parties advocating such policies (Rehm 2011*a*). On the other hand, "sociotropic" voters may instead use their assessment of the national economy—rather than their personal economic situation—to inform their vote choice (Kinder and Kiewiet 1981). This theory instead implies that information about unemployment shocks affecting others will reduce a voter's support for the government.

By linking Danish administrative data with surveys conducted between 2010 and 2013, we address two major obstacles to identifying the political effects of unemployment shocks that diffuse through social networks. First, our rich administrative data enables us to objectively and accurately map networks of social ties through which information could pass for all living Danes since 1980. Our network of strong and mostly weak ties includes: (i) nuclear family and partner; (ii) recent coworkers; and (iii) the graduating cohort of an individual's most recent degree program. Although information may not always pass between *every* individual tie, our nationally representative survey validates that conversations—including about unemployment—are common between these ties. While survey methods effectively identify close ties, our approach to mapping both strong and weak

ties at scale reduces the severe risk of introducing biases by omitting relevant ties (Chandrasekhar and Lewis 2016).

Second, the administrative data enable us to estimate the effects of *second-degree* social ties—individuals about whom someone could learn only through shared first-degree ties, but does not interact with directly—becoming unemployed within the last 12 months. We focus on information about shocks that must pass through two ties in order to: (i) mitigate the challenges of estimating causal effects within social networks using non-experimental data; and (ii) help distinguish information diffusion from social conformity pressures or emotional reactions that are most likely to arise when one actually knows people that became unemployed. Our identification strategy, which builds on Bramoullé, Djebbari and Fortin (2009), rests on two key features. First, our focus on unemployment *shocks*, rather than *status*, alleviates the "reflection problem" (Manski 1993) by establishing the shock's source, and thus the direction that any information must pass. Second, beyond focusing on shocks affecting second-degree social ties, we further mitigate the risk that common shocks—that could reflect vocation-specific risks, exposure to different political perspectives, differences in local economic conditions, and localized access to media content—might instead drive voter responses by: (i) restricting the set of second-degree ties to those living in different locations from either the respondent or the first-degree tie connecting the respondent to the second-degree tie; and (ii) including fine-grained fixed effects that ensure that our identifying variation comes only from differences in the distribution of shocks within the networks of respondents in the same parish and same industry, occupation, and educational categories within any given year.

We find that the beliefs, policy preferences, and voting behavior of Danes are highly responsive to unemployment shocks afflicting second-degree social ties. Indicating that information relatively frequently flows through our networks of predominantly weak ties, an increase in the share of second-degree ties that became unemployed within the last year increases voters' expectations of unemployment—for both the country at large and themselves. These concerns are reflected in increased support for more generous unemployment insurance, which was proposed by Denmark's left-wing parties after the financial crisis. Such second-degree unemployment shocks also influence

voting behavior, increasing a voter's probability of voting for a left-wing political party. Variation in incumbency within our sample indicates that voters are not simply punishing the incumbent party, as predicted by sociotropic theories. Beyond showing that information acquired through social ties is a key force underlying policy preferences and voting behavior, the magnitude of our estimates could account for the left-bloc's wafer-thin electoral victory in 2011. Indeed, a 3 percentage point increase in the share of second-degree ties that became unemployed in the last year increases a voter's probability of voting for a left-wing party by around 3 percentage points.

Although we cannot directly observe interactions between millions of voters, our analysis of transmission mechanisms suggests that our findings reflect information diffusion through social networks and egotropic economic interests guiding policy and political responses to beliefs about unemployment. First, our survey data indicate that voters regularly discuss unemployment shocks with others, and that such conversations often entail discussing unemployment risks, unemployment insurance policies, and—to a lesser extent—politics. Second, consistent with social ties sharing unemployment information relatively frequently, responses to first-degree ties becoming unemployed are around five times greater than responses to second-degree ties becoming unemployed. Third, increased subjective unemployment risks and increased support for greater unemployment insurance and left-wing parties primarily reflect shocks to second-degree ties in the *same* industry or occupation as a respondent. Such heterogeneity suggests that egotropic interests drive political preferences. This interpretation is further supported by voters not differentiating between the industries or occupations of second-degree social ties when assessing the national unemployment rate, and barely altering their policy preferences or voting behavior when second-degree ties in industries or occupations other than their own become unemployed.

This article makes two main contributions. First, we leverage network data with unprecedented detail to demonstrate that social ties play an important role in the political lives of Danish voters by diffusing information pertaining to individuals outside a voter's immediate network. Given the predominance of weak ties in our networks, our findings chime with seminal studies suggesting that weak ties facilitate job opportunities by supplying more novel information (Granovetter 1973)

and that economic and political information often emanates from friends and neighbors (Huckfeldt and Sprague 1995; Katz and Lazarsfeld 1955; Kiewiet 1983). More recent studies have highlighted the importance of peers for enhancing work and educational 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 turnout (Bond et al. 2012; Gerber, Green and Larimer 2008; Nickerson 2008) and collective action (Siegel 2009; Steinert-Threlkeld 2017). In contrast with these studies, and the research already highlighting the role of information and especially social pressure among individuals with close ties (e.g. Sinclair 2012), we show that information diffusion through relatively weak ties significantly affects political preferences and voting behavior in an unfavorable real-world economic environment. Our findings thus lend external validity to experimental studies that identify information diffusion within networks in more artificial laboratory or online contexts (Ahn et al. 2013; Barberá 2015; Carlson 2019; Klar and Shmargad 2017; Mutz 2002).

Second, our analysis indicates that concerns about unemployment risks primarily influence policy preferences and voting behavior via egotropic, rather than sociotropic, considerations. We overcome the difficulty of distinguishing such accounts (see Ansolabehere, Meredith and Snowberg 2014) by separating personal and national unemployment expectations and differentiating sensitivity to the similarity of the industry of the individuals that became unemployed. Our results support the insurance-based theories proposed by Iversen and Soskice (2001), Moene and Wallerstein (2001), and Rehm (2011*b*). While the effects that we observe are, unsurprisingly, smaller than for individuals becoming unemployed themselves, information about others received through social networks appear to be more persistent and likely to influence voting behavior (see Margalit 2019). 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 unemployment prospects from the signals they receive within their social networks. In suggesting that egotropic motives outweigh sociotropic motives, our results complement Fisman, Jakiela and Kariv's (2015) finding that exposure to the Great Recession made voters more selfish and less egalitarian.

2 Information diffusion through social networks

The potential for information to diffuse through networks to reach uninformed individuals is widely recognized (see Jackson 2010).[1] Indeed, citizens become informed about job opportunities and increase their productivity through their social ties (e.g. Caldwell and Harmon 2019; Cornelissen, Dustmann and Schönberg 2017). Granovetter (1973) further distinguishes strong ties that individuals interact with more frequently from weak ties that individuals interact with occasionally. The more novel information that weak ties with low levels of network overlap can provide is often most valuable (Aral and Van Alstyne 2011; Granovetter 1973), whereas strong ties may be comparatively important for supporting monitoring and enforcement within groups (e.g. Larson 2017).

The informative role of social networks may be especially important in political contexts. Given that voters with limited interest in politics face weak incentives to acquire costly political information for themselves (Downs 1957; Huckfeldt and Sprague 1995), many voters in advanced democracies encounter politically-relevant information through a somewhat diverse group of friends and family (Huckfeldt, Johnson and Sprague 2004; Kiewiet 1983). Better-informed opinion leaders are particularly influential within these networks (Huckfeldt and Sprague 1995; Katz and Lazarsfeld 1955). While information is inevitably modified somewhat as it diffuses between individuals (Carlson 2019), social ties remain a critical source of politically-relevant information—in large part because many citizens, even in media-abundant contexts, are exposed to little else.

In the context of unemployment shocks afflicting others, we expect that such information will often be passed between strong and weak ties in some form. Regardless of the accuracy of their prior beliefs about economic conditions, learning of more cases of others becoming unemployed is likely to increase a voter's perception of aggregate unemployment rates if such individuals are not all quickly reemployed or if reemployment information is conveyed less frequently. Such signals may also increase an individual's perception of their own unemployment risk, if their risk is perceived to be associated with the risk of those that became unemployed. In line with studies

[1] Our focus is not on how different network structures or the position of shocked individuals influence information diffusion (see e.g. Klar and Shmargad 2017).

demonstrating that West European voters update in sophisticated ways from politically-relevant information provided by credible media and political sources (e.g. Alt, Lassen and Marshall 2016; Kendall, Nannicini and Trebbi 2015), we expect that:

H1. *Exposure to information relating to more individuals becoming unemployed that is conveyed through social ties will increase an individual's perception of aggregate unemployment.*

H2. *Exposure to information relating to more individuals becoming unemployed that is conveyed through social ties will increase an individual's perceived risk of becoming unemployed themselves, especially when the unemployment shocks affect those in similar vocations.*

Persistent changes in posterior beliefs about national and individual unemployment prospects could in turn alter voters' political preferences and voting behavior. This could reflect egotropic or sociotropic logics. Proponents of the egotropic approach posit that voters facing higher individual or occupational unemployment risks will increase their support for government programs, including demanding more generous unemployment insurance in the face of greater risks of becoming unemployed (Iversen and Soskice 2001; Moene and Wallerstein 2001; Rehm 2011*b*) and voting for the left-wing political parties typically espousing such policies (Rehm 2011*a*). To the extent that learning of unemployment shocks afflicting others causes voters to form different expectations about becoming unemployed themselves, the egotropic logic predicts that:

H3. *If voters are guided by egotropic motivations, being exposed to information about unemployment shocks that is conveyed through social ties will increase an individual's support for more generous social insurance programs and left-wing political parties.*

This preference for insurance could also translate into greater support for general redistribution on the margin, although such policies remain costly for higher-income voters when employed.

While the egotropic logic rests on voters supporting policies that they expect will personally benefit them materially, sociotropic voters instead vote on the basis of *national*-level economic performance (Kinder and Kiewiet 1981). This could reflect a self-interested desire to elect a competent government or more altruistic motivations. Regardless, we expect voters that come to believe that

the national unemployment rate is higher than they previously believed will then hold the government responsible and accordingly decrease their support for the parties in government:

H4. *If voters are guided by sociotropic motivations, being exposed to information about unemployment shocks that is conveyed through social ties will decrease an individual's support for incumbent parties.*

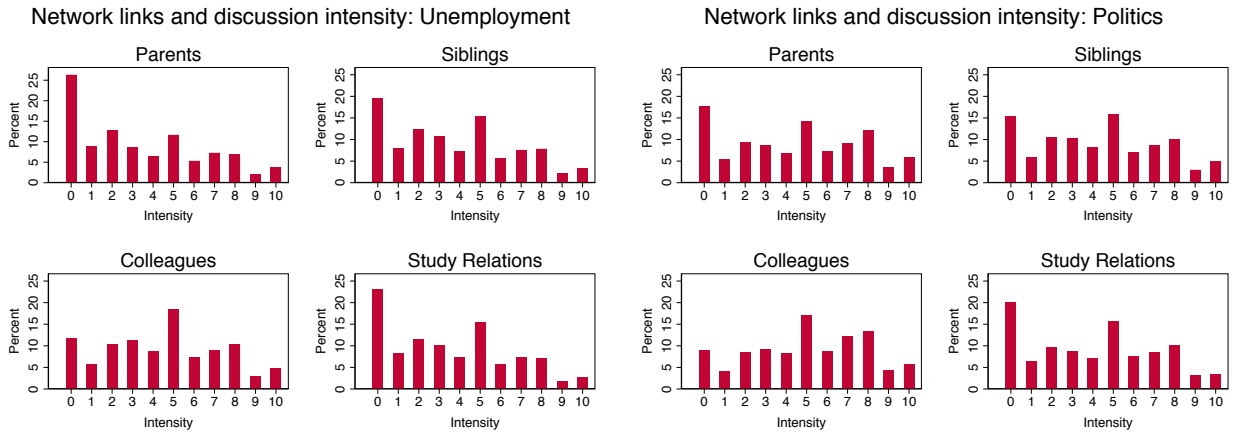
3 Danish social and political context

We study the effects of unemployment shocks transmitted through social ties on economic concerns and political preferences in the aftermath of the 2008 financial crisis in Denmark. Elections follow a proportional representation system, and Denmark has historically been governed by alternating center-right (Venstre/Liberal and Conservative parties) and center-left (Social Democrat and Socialist People's parties) coalition governments. The center-right governed between 2001 and 2011, before regaining control in 2015. Denmark's 98 municipalities, the primary unit of subnational government, contain 2,187 parishes (in 2011)—the country's smallest administrative unit.

3.1 Informal social ties

Informal networks, rather than formally constituted organizations,[2] are the primary basis of social ties in Danish society. While family ties are undoubtedly important, various studies also highlight the workplace (e.g. Glitz and Vejlin 2014) and educational institutions (e.g. Nielsen and Svarer 2009) as important sources of social interaction in Denmark. In the workplace, individuals are likely to spend more time with coworkers than almost anyone else. A European Commission (2004) survey further shows that 44% of adults report meeting socially with colleagues outside of work at least once a month. Almost all students complete 10 years of school, and 93% of the 2012-cohort continued into some form of high school program. High school graduates either enter the labor market or tertiary education. Given that only five metropolitan areas in Denmark offer university

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



(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)

degrees, the geographic diversity of social ties often expands at this point. For many individuals, their closest friends emerge from their final educational institution, while 64% of adults report having social contact with friends at least once a week (European Commission 2004).

Social interactions through these informal networks often diffuse economic and political information. For example, 52% of survey respondents 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 (European Commission 2004). These types of interactions are also consequential, as labor market information from former coworkers affects displaced workers' reemployment probabilities (Glitz and Vejlin 2014) and job-to-job mobility (Caldwell and Harmon 2019). More generally, discussion of unemployment and politics is common among family members, current and former colleagues, and former classmates. 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," Figure 1a shows that the majority of the working age population discusses unemployment within each class of tie in 2015, and most frequently among work colleagues. Figure 1b documents similar patterns and higher frequencies regarding the discussion of politics in general. In comparative perspective, the 2008-2010 wave of the European Values Study ranked Denmark 4th of 46 countries

in terms of discussing politics with friends, and 26th and 20th in terms of discussing politics with their mothers and fathers, respectively.

In adult life, social ties tend to be stable over time due to limited geographical mobility. In 2014, the number of people changing their official address amounted to 15% of the population, of which only 35% moved across municipalities.[3] 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. Ties with some former classmates, and especially ties with current and former co-workers, thus remain stable and active for many Danes over their adult lives.

3.2 Unemployment as a political issue following the 2008 financial crisis

After a decade of low (gross) unemployment rates in the 2000s, reaching 2% in early 2008, unemployment almost tripled to around 6% by the 2011 general election.[4] The economy, and especially unemployment, was central to the political debate. Nearly 20% of voters cited unemployment as the most important issue for politicians to address, while a further 20% regarded the welfare state as most important.[5] After the election, unemployment remained 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.[6] Only in 2014 did the unemployment rate start to fall, stabilizing at around 4% in 2016.

Left-right ideological differences in party platforms were clear in response to the unemployment surge during the financial crisis. The Venstre-led center-right government implemented a "tax freeze" before proposing and passing several market-oriented policies, including a regressive tax cut in 2009-2010, a 2011 "growth program" providing subsidies to small businesses and promising cuts to corporation tax, and—most controversially—a 2010 reform of Denmark's unemployment insurance system that limited the maximum benefit duration of the generous voluntary insurance scheme from four to two years.[7] In contrast, the Social Democrats and Socialist People's Party

[3] Statistics Denmark, Statistikbanken, Flytninger, table FLY33 and table FLY66, link.
[4] Gross (unlike net) unemployment counts those in active labor market programs as unemployed.
[5] Danish Election Study, cited here.
[6] The 2011 Danish Election Study poll is available here. The 2013 Jyllands-Posten poll is here.
[7] The insurance system and this reform are described further in Appendix section A.1.

winning 2011 campaign revolved around their "Fair Solution." This program also contained many policies focused on labor market imbalances, but instead emphasized demand-side and educational policies. They strongly criticized the reform of the unemployment insurance system, and called for public investments, labor agreements, and improved education to create new jobs.

4 Empirical design

This section first introduces our main variables and operationalizes social ties. We then detail our empirical strategy for identifying the effects of an increase in the share of second-degree social ties that experienced an unemployment shock, which we hypothesize could diffuse through first-degree social ties to influence respondents to our panel survey. We focus on shocks to second-degree individuals that survey respondents do not interact with directly to mitigate the risk that common shocks drive our estimates and to help distinguish information diffusion from social conformity.

4.1 Panel survey and administrative data

We leverage two high-quality sources of data. First, our outcome variables are questions from the 2010-2013 rounds of the Danish Panel Study of Income and Asset Expectations capturing subjective unemployment perceptions, policy preferences, and vote choice. Each survey was fielded in January and February. This telephone survey first sampled around 6,000 Danes registering some labor income between 1998 and 2004, and has randomly resampled from this pool to maintain the sample size over time.[8] Although this sample is slightly older, better educated, and richer, comparing columns (1) and (2) with columns (3) and (4) in Table 1 shows that our survey respondents broadly resemble the Danish working age population.

Second, to define unemployment shocks and social ties between individuals, we rely on detailed individual-level administrative data for the entire population. These government-collected registers, which contain family relations, education, and income tax returns, are available annually between

[8]The initial response rate was 50% (including unreachables), and attrition into 2011 was 31%.

Table 1: Summary statistics for the population and our sample

	Full population, aged 20-65		Survey respondents		Survey respondents' first-degree social ties		Survey respondents' second-degree social ties	
	Mean (1)	Median (2)	Mean (3)	Median (4)	Mean (5)	Median (6)	Mean (7)	Median (8)
Woman	0.50	0	0.49	0.16	0.54	1	0.51	1
Age	42.90	43.00	45.61	46.43	43.55	43.98	42.27	42.75
Children	0.78	0	1	1	0.97	1	0.86	0
Single	0.35	0	0.22	0	0.26	0	0.32	0
Gross income (DKK)	325,251	294,646	406,620	362,241	432,552	379,043	364,299	329,621
Total assets (DKK)	848,888	375,907	1,231,939	807,815	1,118,013	777,663	950,351	563,494
Total debt (DKK)	635,145	271,566	827,908	563,647	826,492	597,290	727,413	428,592
Homeowner	0.49	0	0.67	1	0.63	1	0.55	1
Education basic	0.33	0	0.19	0	0.09	0	0.23	0
Education short	0.39	0	0.41	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	0.14	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	13,385,137	13,385,137	17,816	17,816	1,882,081	1,882,081	9,009,683	9,009,683

Notes: To comply with Statistics Denmark's anonymity restrictions, medians and lower and upper bounds of ranges are computed across five observations. The summary statistics in columns (3)-(8) are for the largest sample used in our analyses below.

1980 and 2012. We thus possess unique identifiers and data for all 7.98 million individuals living in Denmark over this period. Panel survey responses were linked to these records by Statistics Denmark. Access to this administrative 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 measure national and personal assessments. First, we measure beliefs about aggregate unemployment in two ways: in 2011-2013, the survey elicited respondents' best guess at the *current* national unemployment rate; and in 2011 and 2013, the survey elicited respondents' national unemployment rate *forecast* for the next year. Second, an individual's risk of becoming unemployed is based on their self-assessed probability, from 0 to 1, of becoming unemployed in the forthcoming year.

We define three indicator variables to capture policy preferences: for the 34% of respondents expressing support for increasing unemployment insurance above the existing level; for the 38% of respondents believing that the government should do more to support the poor; and for the

39% of respondents believing 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).[9]

Two further outcomes measure 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 voters that reported having voted for such a left-wing party at the 2011 election.[10] Respectively, 42% and 50% of respondents supported the left by these two measures.

4.1.2 Individual unemployment shocks

To capture individual unemployment shocks that represent novel information, we follow Margalit (2013) in focusing on instances of other individuals recently *becoming* unemployed.[11] 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 within the last year using an indicator that denotes whether an individual was registered as unemployed in the November preceding the survey—the snapshot at which the Danish administrative data is collected—but was not registered as unemployed in November the year prior.[12] On average, 3% of working age Danes experience such a shock each year over our study period. The timing of these shocks makes it unlikely that survey respondents heard about them just before completing the surveys conducted in January and February.

Individuals that become unemployed often later regain employment, and these two processes can occur concurrently for different second-degree ties. Unsurprisingly, there is thus a strong positive correlation between the share of second-degree ties that became unemployed and reemployed ($\rho = 0.38$). The share of second-degree ties that experience unemployment shocks should thus be considered a compound of learning about individuals becoming unemployed and all events that

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

[10] Reported turnout rates in our survey were 98%, although nationwide turnout in 2011 was 88%.

[11] When exiting employment, individuals are transferred to unemployment status and receive unemployment benefits or cash assistance (see Appendix section A.1).

[12] Following international standards, those in active labor market program count as unemployed.

occur subsequently, including some of those individuals regaining employment.

4.1.3 Mapping social networks of strong and weak ties

Our administrative data presents a rare opportunity to extensively map social ties throughout a population. Although some ties are closer than others, a comprehensive network is critical for ensuring that bias is not introduced by the omission of ties through which information passes. Indeed, our estimates would be upwardly biased if unemployment shocks directly or indirectly affecting social ties were correlated with shocks affecting other unmeasured ties. Chandrasekhar and Lewis (2016) further prove that missing ties can produce non-classical measurement error that can severely upwardly bias estimates, even when nodes are missing at random. Such concerns are pertinent in Denmark, where—as Figure 1 illustrates—information related to employment often passes between relatively weak ties (see also Caldwell and Harmon 2019; Glitz and Vejlin 2014). To minimize biases, we adopt an inclusive definition of social ties that encompasses ties of varying strengths. This enables us to estimate effects that average across any information that flows between more and less distant ties.

Specifically, we approximate an individual's *first-degree* network of strong and predominantly weak ties using the following criteria:

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

2. **Vocation**: coworkers from within the previous two years. For firms with 25 or more employees or for individuals that accumulated more than 50 co-workers across multiple firms, we only include coworkers within the same one-digit educational category.

3. **Education**: fellow students from the cohort at the institution where their highest level of educational degree was obtained (e.g. subject-degree class at a specific university for university-level degrees), or the cohort at the point of dropping out of school without a degree.

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

The firm size restriction reflects the likelihood that individuals in large firms interact primarily with recent colleagues doing similar types of jobs within the firm. The education restriction captures the likelihood that ties attenuate upon moving on to another educational institution. Although our definition of weak ties inevitably includes some omissions,[14] our results are robust to defining larger networks that include more past colleagues and high school and university-degree graduating classes as well as adjusting for indicators of firm- and education-level network truncation.

While the interaction between some of these individuals may be negligible, our operationalization of social ties does capture meaningful real-world communications between Danes. First, data from the mobile money app MobilePay shows that these familial, vocational, and educational ties are all significant predictors of electronic payments between Danes (Sheridan 2019). Second, we further conducted a nationally representative survey of 1,506 Danish adults in November 2018 to validate our operationalization of social ties.[15] Figure 2 shows that respondents report having conversed with significant numbers of social ties by our definition. Within the last year, the mean respondent had a general conversation with more than 50% of individuals within our family and vocational categories, and a conversation about economics or politics with at least 30%. Respondents converse with fewer members of their terminal educational cohort, although the relationships that they do sustain may comprise their closest ties. The robustness checks in Appendix Table A7 report similar results when shocks that could only be transmitted through educational ties are excluded.

We combine the familial, vocational, and educational information above to construct an adjacency matrix characterizing first-degree social ties between every individual in the Danish population alive between 1980 and 2012. Appendix section A.4 explains how this matrix was computed. We focus on the social ties of the 8,747 unique labor force participants that appear in our 2010-2013 surveys. The mean and median survey respondent in a given year respectively register 224 and 81 first-degree ties, of which 2% and 5%, 74% and 43%, and 24% and 52% are familial, vocational, and educational ties, respectively.

[14]The most obvious omissions are (non-work and non-school) friends and non-nuclear family members. If such individuals live locally and have local friends themselves, our sample restrictions described below should mitigate the bias from these omissions.

[15]Appendix section A.3 describes the survey protocols.

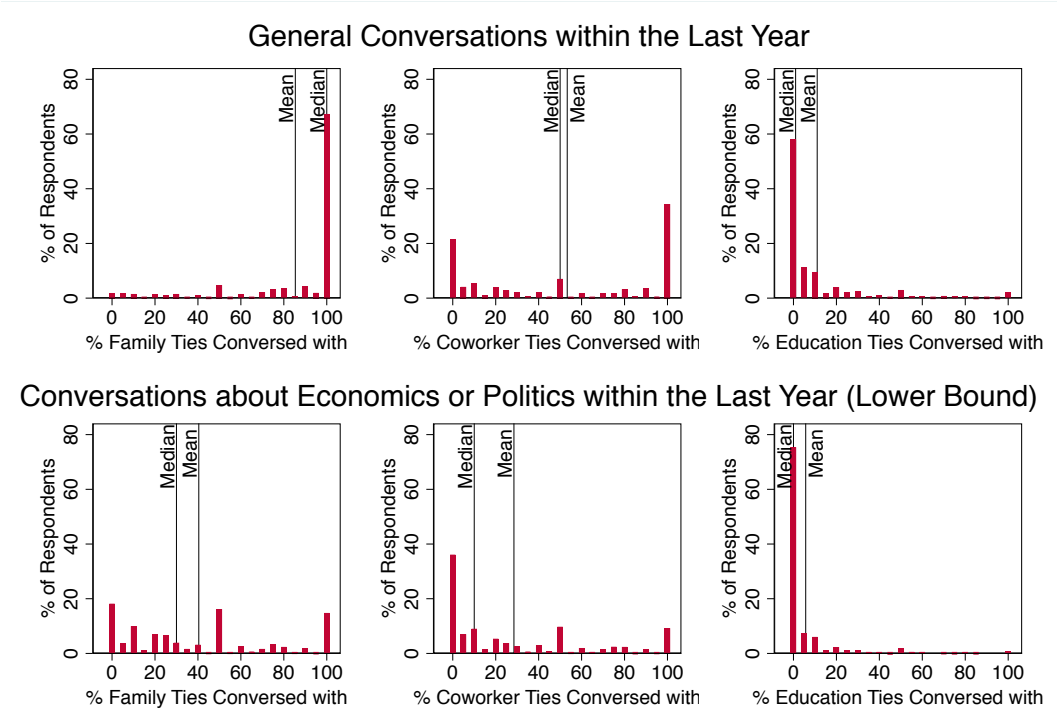


Figure 2: Histograms showing the share of first-degree weak ties that individuals converse with

Notes: All questions were phrased to reflect our operational definitions of first-degree weak ties (see Appendix section A.3). Conversations about economics *or* politics represent a lower bound because, since we cannot identify the union of the exact individuals that respondents refer to, we take the maximum of the percentage of ties conversed with about each topic.

4.2 Identification strategy

Our goal is to estimate the effect of information relating to unemployment shocks that diffuses through social ties on a voter's economic and political beliefs, preferences, and behavior. To maintain a reasonable probability that information relating to unemployment shocks reaches our survey respondents, while reducing the possibility that this is confounded by social pressure or emotional reactions that could arise when respondents are linked directly to those becoming unemployed, we leverage our population-level network data to exploit unemployment shocks afflicting working age (20-65) *second-degree* ties. A second-degree tie is an individual that is a first-degree tie of at least one of a respondent's first-degree ties, but is not a first-degree tie of the respondent. After excluding nearby second-degree ties and including fine-grained fixed effects that account for factors that could generate differences in respondents' networks, this approach leverages distant shocks to

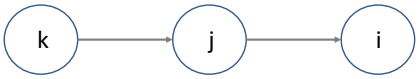


Figure 3: Illustration of undirected connections between weak ties

Note: In this example, individual i is observed in our survey, and we estimate the effect of an unemployment shock afflicting individual k—information about which must pass through individual j—on individual i.

"friends of friends" that are plausibly exogenous to other determinants of our outcomes. We now explain this identification strategy in detail.

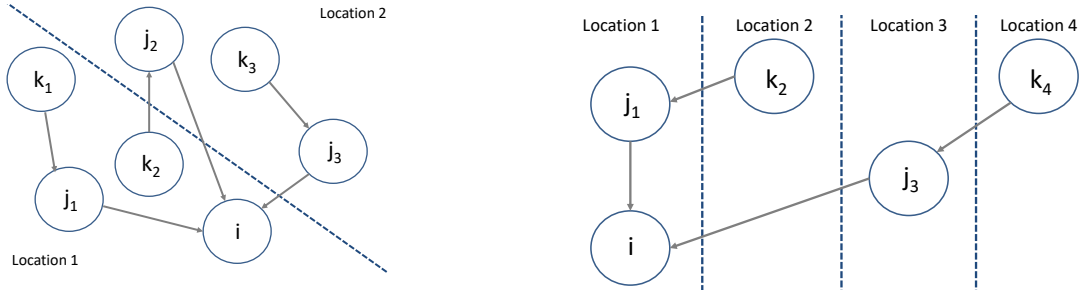
More formally, our design focuses on "intransitive triads" where individuals i and j are connected and individuals j and k are connected, but i and k are *not* connected.[16] To trace informational shocks, we exploit variation in the share of i's second-degree ties—that is to say k's that are only connected to i through a j—that became unemployed in a given year.[17] Because each k is only connected to i through a j that is a first-degree tie for both i and k, unemployment shocks afflicting i's second-degree ties should only affect i by diffusing through j. Figure 3 illustrates this design, where i is our panel survey respondent, j is a first-degree tie, and k is a second-degree tie.

Leveraging unemployment shocks afflicting second-degree ties addresses two challenges that often impede the estimation of information diffusion effects within social networks. First, our focus on k-specific unemployment shocks addresses the reflection problem—that correlated economic or political outcomes between individuals i and k could reflect i affecting k through j or k affecting i through j (see Manski 1993)—by establishing the source of the shock, and thus the direction in which any information must diffuse. Second, by focusing on unemployment *shocks* and shocks with two degrees of separation, we reduce the risk that any correlation between shocks afflicting second-degree ties and respondent outcomes reflects common characteristics or experiences shared by i and k (Bramoullé, Djebbari and Fortin 2009).

Nevertheless, a key concern is that respondents and their second-degree ties experience common shocks. Consequently, i would receive essentially the same, or highly correlated, information about an unemployment shock to k without receiving such information from a j linking k to i. We

[16] Appendix section A.4 illustrates how second-degree social ties are constructed.

[17] k's that are linked to i through multiple j's are counted only once to compute this share. Appendix Table A2 shows that the results are robust to counting each j,k pair separately.



(a) Excluded cases

(b) Included cases

Figure 4: Illustration of excluded and included second-degree social ties

address the spatial component of this concern by first excluding all second-degree ties located in the same municipality as i.[18] Second-degree ties k_1 and k_2 in Figure 4a are examples of such excluded cases. To address an analogous problem arising when j experiences shocks correlated with k, we further exclude any second-degree tie k that is located in a parish where any first-degree tie j that indirectly connects i and k resides.[19] This excluded case is exemplified by the second-degree tie k_3 in Figure 4a. Consequently, our identification strategy only exploits unemployment shocks to individuals k in a different location from both individuals i and j, as illustrated in Figure 4b.

These two restrictions reduce the number of second-degree ties used to compute the share of a respondent's second-degree ties that became unemployed by around half. Before applying these exclusions, survey respondents had a mean of 17,632 and a median of 7,831 second-degree ties in a given year; after the exclusions, the mean and median respectively drop to 7,130 and 4,364 second-degree ties. Although excluding proximate ties that could generate biases may reduce the external validity of the shocks, Table 1 shows that the socioeconomic characteristics of the remaining working age second-degree ties are broadly similar to the working age population.

We further address more general common shocks by including fine-grained i-level fixed effects. Specifically, we use four sets of respondent-by-year fixed effects to restrict attention to variation in unemployment shocks that arise due to differences in network composition between individuals

[18] Appendix Table A7 shows similar results if we further exclude k's from i's region.

[19] Appendix Table A7 reports similar point estimates when k's located in the same municipality as j are excluded.

within the same industry, occupation, educational, and geographic groupings.[20] First, industry × year fixed effects absorb common economic and political attitudes as well as vocational interactions among voters within a particular two-digit industry classification in a given year. Second, occupation × year fixed effects absorb differences across one-digit occupational classifications in a given year. Third, education × year fixed effects absorb differences across time in the attitudes of voters within a given one-digit educational classification. Appendix section A.5 describes these digit classifications. Fourth, parish × year fixed effects absorb parish-specific shocks—such as common community preferences or localized media coverage—that could induce individuals with different networks to adopt similar unemployment concerns and political preferences. Together, these fixed effects capture many potential common shocks, and increase confidence that our estimates reflect differences in the distribution of second-degree shocks experienced by otherwise similar individuals. A placebo test and robustness checks employing more demanding adjustment strategies and sample restrictions further address common shock concerns.

In sum, we estimate the effect of an increase in the share of respondent i's second-degree ties that recently became unemployed on outcomes for i using the following OLS regression,

$$Y_{it} = \beta \textit{Second-degree unemployment shock share}_{it} + \gamma_{wt} + \delta_{ot} + \eta_{et} + \mu_{pt} + \varepsilon_{it}, \tag{1}$$

where $\textit{Second-degree unemployment shock share}_{it}$ is the share of i's (non-excluded) second-degree ties that were unemployed two months before the survey in year t (having not been unemployed in year $t-1$), and γ_{wt}, δ_{ot}, η_{et}, and μ_{pt} are respondent-level industry, occupation, education, and parish fixed effects that vary by year t. Standard errors are clustered by i's municipality.

[20] Rather than leverage within-respondent variation, we exploit cross-sectional variation because—by the law of large numbers—there is limited variation in the *share* of a respondent's second-degree ties becoming unemployed each year (conditional on year-interacted fixed effects). Although meaningful temporal variation requires year-on-year significant changes in the risk profile of a second-degree network, Appendix Table A3 shows that the results are generally robust to including a lagged dependent variable to adjust for stable determinants of outcomes.

Table 2: Estimates of second-degree social tie unemployment shocks on a respondent's economic and political beliefs, preferences, and behavior

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment 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)
Second-degree unemployment shock share	0.226***	0.260***	0.743***	0.652*	0.279	0.212	0.780**	1.242**
	(0.056)	(0.059)	(0.211)	(0.354)	(0.327)	(0.427)	(0.301)	(0.577)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	5,209
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.085	0.076	0.174	0.320	0.387	0.402	0.439	0.500
Outcome standard deviation	0.055	0.050	0.302	0.467	0.487	0.490	0.496	0.500
Unemployment shocks mean	0.022	0.022	0.023	0.023	0.023	0.025	0.023	0.022
Unemployment shocks std. dev.	0.014	0.014	0.015	0.015	0.015	0.018	0.015	0.014
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. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5 Results

This section presents our main finding that unemployment shocks to second-degree social ties significantly affect economic and political beliefs, preferences, and behavior, before leveraging placebo and sensitivity analyses to demonstrate the robustness of our findings.

5.1 Effects of unemployment shocks afflicting second-degree social ties

Table 2 reports our main results estimating the effect of an increase in the share of working-age Danish voters' second-degree social ties that became unemployed in the last year on our outcomes of interest. A unit increase in the share experiencing an unemployment shocks implies a shift from 0% of second-degree ties becoming unemployed to 100%, while the standard deviation is 1.5%.

We first find, in line with hypothesis H1, that unemployment shocks to second-degree social ties increase perceptions of aggregate unemployment rates. Our point estimates in columns (1) and (2) indicate that a percentage point increase in the share of second-degree ties that became unemployed in the last year increases both an individual's current guess at the national unemployment rate and their expectation for the coming year by around 0.25 percentage points. A standard deviation increase in the share of a respondent's second-degree ties becoming unemployed thus implies

around a 0.06 standard deviation increase in an individual's assessment of aggregate unemployment rates. Even among a relatively informed electorate, and consistent with Alt, Lassen and Marshall (2016), voter beliefs about national unemployment rates are thus quite malleable. Moreover, the positive estimates suggest that information about second-degree ties becoming unemployed dominates, on average, any possible subsequent information about the smaller share of such ties that became reemployed.

Voters' beliefs about their own unemployment risk are also influenced by second-degree unemployment shocks. Supporting H2, column (3) shows that each percentage point increase in the share of a respondent's second-degree social ties that became unemployed in a given year increases an individual's self-assessed probability of becoming unemployed within the next year by 0.74 percentage points on average. This level of responsiveness, which exceeds differences in perceptions about aggregate unemployment, implies that a 3 percentage point—or almost a two standard deviation—increase in unemployment shocks afflicting individuals that a respondent is indirectly connected to increases the subjective risk of unemployment by 2.2 percentage points. Such an effect is around 14 times smaller than the 31 percentage point increase in the perceived risk of being unemployed associated with respondents themselves suffering an unemployment shock in the last year (see panel C of Table 3 below). Nevertheless, our results still suggest that unemployment experiences of second-degree social ties that are relayed by "word of mouth" are also important determinants of voters' subjective economic outlook. Such beliefs are consistent with voters forming posterior beliefs without possessing the information required to account for the fixed effects in our statistical model (see Appendix section A.7.3).

Beyond influencing a respondent's economic outlook, unemployment shocks afflicting "friends of friends" also affect policy preferences. Consistent with both H3 and H4, column (4) shows that a 3 percentage point increase in the share of second-degree ties becoming unemployed increases the probability that an individual supports more generous unemployment insurance by 2.0 percentage points. This amounts to around one eighth of the greater support for unemployment insurance among respondents that themselves became unemployed. Although the estimates in columns (5)

and (6) are not statistically significant, they suggest that unemployment shocks afflicting second-degree ties 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 are more limited because they are less directly targeted toward citizens expecting to become unemployed. Together, these findings suggest that unemployment shocks transmitted through networks of mostly weak ties cause voters to adopt more left-wing policy positions.

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 a 3 percentage point increase in the share of a respondent's second-degree ties that became unemployed in the last year increases the intention to vote for a left-wing party and actually voting for a left-wing party in the 2011 election by 2.3 and 3.7 percentage points respectively. By way of comparison, these effects are around half the size of the difference in left vote between respondents that did and did not themselves become unemployed. The relatively substantial effects of information that diffuses through social ties thus suggest that networks of mostly weak ties could alter electoral outcomes and governing coalitions, particularly in the close elections experienced recently in Denmark.

Thus far, our findings are consistent with both egotropic (H3) and sociotropic (H4) voting motivations. Even the elevated vote for the left-wing opposition party in 2011 could have reflected sociotropic voting if voters came to view the center-right incumbent coalition as less competent. However, Appendix Table A5 shows that unemployment shocks did not reduce intention to vote for the *government*, which comprised left-wing parties in 2012 and 2013 survey rounds. We provide further evidence against the sociotropic interpretation of voter responses below by showing that political preferences respond primarily to concerns about their own unemployment risks.

Although social interactions between familial, vocational, and educational weak ties are all fairly common in Denmark, it is natural to consider heterogeneity by type of social tie. Appendix Tables A9 and A10 interact unemployment shocks with the type of ties linking a respondent to a

Table 3: Placebo and main robustness checks

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment 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: placebo test examining shocks to similar first-degree social ties that the respondent does not know								
Placebo second-degree	0.036	0.024	0.117	0.097	-0.345**	0.022	-0.489***	-0.810***
unemployment shock share	(0.039)	(0.050)	(0.158)	(0.157)	(0.145)	(0.232)	(0.152)	(0.189)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	3,243
Panel B: controlling for respondent cohort × year fixed effects								
Second-degree unemployment	0.220***	0.265***	0.629***	0.748**	0.386	0.292	0.828***	1.479***
shock share	(0.054)	(0.059)	(0.216)	(0.335)	(0.330)	(0.408)	(0.292)	(0.552)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	5,209
Panel C: adjusting for the respondent themselves suffering an unemployment shock								
Second-degree unemployment	0.223***	0.257***	0.667***	0.617*	0.262	0.137	0.774**	1.203**
shock share	(0.056)	(0.058)	(0.216)	(0.351)	(0.337)	(0.431)	(0.302)	(0.559)
Unemployment shock	0.014***	0.007	0.310***	0.153***	0.100***	0.120**	0.035	0.081*
	(0.004)	(0.005)	(0.018)	(0.028)	(0.029)	(0.047)	(0.024)	(0.044)
Observations	12,991	8,664	17,799	17,799	17,799	4,808	17,799	5,206
Panel D: adjusting for 10 predetermined respondent covariates								
Second-degree unemployment	0.199***	0.246***	0.380*	0.477	0.138	0.123	0.718**	1.155**
shock share	(0.053)	(0.057)	(0.209)	(0.359)	(0.333)	(0.438)	(0.287)	(0.564)
Observations	12,991	8,664	17,799	17,799	17,799	4,808	17,799	5,206

Notes: All specifications are estimated using OLS, and include respondent-level industry × year, occupation × year, education × year, and parish × year fixed effects. The placebo test in panel A is described in the main text. The covariates included in panel D are: gender, age, whether single, number of children, annual income, total asset wealth, total debt, homeowner status, whether unemployed, and the number of second-degree social ties; the regression coefficients for these variables are reported in Table A6. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

first-degree weak tie and linking a respondent's first-degree tie to a second-degree tie. The results overall suggest that shocks transmitted through each type of tie operate relatively similarly.

5.2 Robustness checks

Perhaps the greatest concern is that our estimates reflect common shocks afflicting both the respondent and their second-degree social ties. Beyond our sample restrictions and fine-grained fixed effect structure, we address further this concern using various robustness checks.

First, we conduct a placebo test designed to detect common shocks afflicting respondents with similar types of network by assigning respondents "fake" first-degree social ties that are similar to a respondent's actual social ties. Specifically, each j was replaced by a randomly selected $j' \neq j$ from our sample (without replacement) that lives in the same municipality *and* works in the same one-digit industry as j in a given year, but is *not* actually a first-degree tie of the respondent. We then examine the effects of shocks affecting the k''s associated with each j'. Consistent with

common shocks not driving our results, panel A of Table 3 reports no evidence that shocks influence respondent beliefs and preferences. The negative coefficients in columns (5), (7), and (8) for the variables capturing left-wing attitudes run in the opposite direction to our main findings.

Second, shocks afflicting second-degree social ties belonging to the respondent's same cohort could be associated with those affecting the respondent themselves (e.g. due to legislation or labor demand decisions that differentially affect certain age groups). We address this potential source of common shocks by including (birth year) cohort $\times$ year fixed effects, and thus exploit only variation in unemployment shocks to second-degree ties belonging to the same cohort in a given year. Panel B shows that the inclusion of such fixed effects does not alter our findings.

Third, we further address the concern that our findings are spurious by adjusting for predetermined covariates. Panel C includes an indicator for a respondent becoming unemployed in the last year. Panel D adds nine further respondent-level socioeconomic and demographic covariates: gender, age, whether single, number of children, annual income, total asset wealth, total debt, homeowner status, whether unemployed, and the number of second-degree social ties. In neither case does adjusting for these covariates substantively alter our findings.

Appendix Table A7 reports the results of five additional checks addressing potentially confounding factors or sensitivity to network construction. We show that our findings are robust to: (i) further excluding second-degree ties from the same region as the respondent; (ii) excluding second-degree ties that live in the same municipality as the first-degree tie linking them to the respondent; (iii) adjusting for indicators for respondents whose social tie networks were truncated at the i and j levels by our vocational and educational restrictions; (iv) excluding respondents with more than 10,000 or 5,000 second-degree ties; and (v) ties that rely on ties generated by familial or educational ties. Furthermore, Appendix Table A8 shows that the effect of increasing the share of second-degree ties that became unemployed is relatively linear.

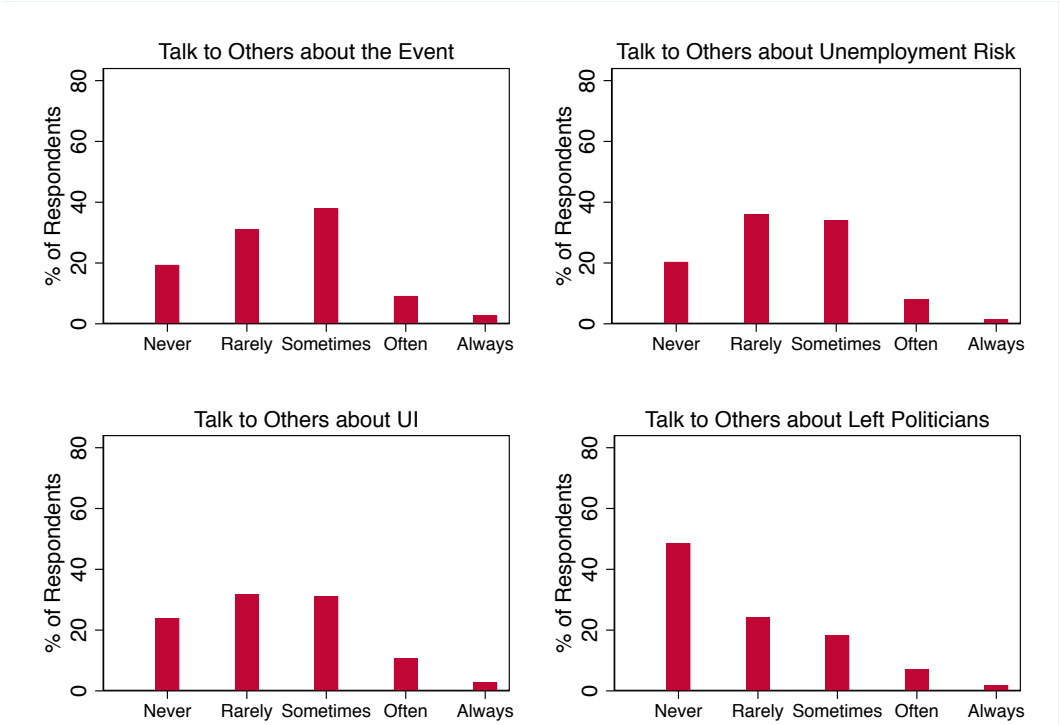


Figure 5: Discussions that individuals instigate when a first-degree social tie becomes unemployed

Notes: All responses are from our 2018 nationally representative survey of adult Danes. All questions refer to what happens after "someone you know becomes unemployed."

6 Information transmission mechanisms driving voter responses

We next illuminate the process through which unemployment shocks to second-degree social ties influence voters. The following analyses indicate that information diffuses through first-degree ties, who respond similarly, and suggest that political responses are primarily driven by egotropic rather than sociotropic motivations.

6.1 Information diffusion through first-degree social ties

Information diffusion between second-degree ties likely requires that the intermediary internalizes unemployment shocks at least somewhat similarly to the ultimate recipient. It is difficult to see how a respondent could be sensitive to unemployment shocks experienced by individuals that they do not interact with without such a chain of events.

To assess this transmission mechanism, we first fielded a nationally representative survey in 2018 to examine what information is passed to others when "someone you know" becomes unemployed. Figure 5 shows that most respondents at least occasionally relay this event to others within their social tie network. Furthermore, many respondents instigate discussions with others about unemployment risk, unemployment insurance, and—to a lesser extent—the need for more left-wing politicians in response to someone they know becoming unemployed. Only 9% of respondents report never instigating any kind of discussion after someone they know becomes unemployed. Danes thus often seem to diffuse politically-relevant information to their first-degree social ties when another first-degree tie becomes unemployed.

A second implication of the information diffusion mechanism is that intermediary ties should alter their beliefs and preferences in response to unemployment shocks in a similar manner to our respondents. Ideally, we would test this by estimating the effect of unemployment shocks afflicting second-degree ties on the intermediary tie j that links respondent i to the k's. Unfortunately, very few of these intermediaries also participated in our panel survey. In the spirit of two-sample instrumental variable techniques, we instead use the first-degree tie between i and j to substitute for the first-degree tie between j and k that we would ideally estimate. If i-j and j-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 social ties are broadly similar to both our respondents and their second-degree ties, as required. We then approximate the first step in the transmission of information from k to j by estimating the following OLS regression:

$$Y_{it} = \beta \textit{First-degree unemployment shock share}_{it} + \gamma_{wt} + \delta_{ot} + \eta_{et} + \mu_{pt} + \varepsilon_{it}, \tag{2}$$

where $\textit{First-degree unemployment shock share}_{it}$ is now the share of a respondent's first-degree social ties that became unemployed within the last year. The fixed effects are analogous to equation (1), while we exclude first-degree ties located in the same municipality as the respondent.

The results reported in Table 4 add further credence to the information diffusion mechanism. An increase in the share of first-degree social ties that became unemployed influences respondents'

Table 4: Estimates of first-degree social tie unemployment shocks on respondent economic and political beliefs, preferences, and behavior

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment 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)
First-degree unemployment shock share	0.014	0.012	0.205***	0.156**	0.166**	0.165	0.133*	0.388**
	(0.012)	(0.015)	(0.068)	(0.065)	(0.084)	(0.125)	(0.069)	(0.190)
Observations	12,771	8,496	17,454	17,454	17,454	4,683	17,454	5,137
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.085	0.076	0.171	0.319	0.387	0.405	0.440	0.502
Outcome standard deviation	0.055	0.050	0.299	0.466	0.487	0.491	0.496	0.500
Unemployment shocks mean	0.025	0.026	0.027	0.027	0.027	0.031	0.027	0.025
Unemployment shocks std. dev.	0.053	0.052	0.057	0.057	0.057	0.065	0.057	0.046
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. These estimates are not directly comparable to those in Table 2 because the denominator underpinning the unemployment shock share differs; the sample is slightly lower than Table 2 because respondents whose first-degree ties all live in the same municipality are excluded. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

own unemployment concerns and policy and political preferences in the same direction as we found for shocks to second-degree ties in Table 2. Specifically, a 3 percentage point increase in the share of first-degree ties that became unemployed in the last year significantly increases a respondent's own perceived risk of becoming unemployed by 0.6 percentage points, their support for unemployment insurance by 0.5 percentage points, and their support for left-wing political parties by 0.5-1 percentage points. We now also observe a statistically significant increase in support for government policies supporting the poor.

However, the effects of unemployment shocks afflicting first-degree ties also differ from the effects of shocks afflicting second-degree ties in two important ways. First, the effects of first-degree ties becoming unemployed on a respondent's own concerns and political preferences are notably larger *per shock*. To see this, note that the point estimates in Tables 2 and 4 are not comparable because the denominators that define the share of ties that became unemployed differ substantially: in these empirical analyses, the mean respondent has 186 first-degree social ties, but 4,487 second-degree social ties. To compare the effect of a single tie becoming unemployed, we divide the coefficients in Tables 2 and 4 by these means respectively. This implies that, per shock, the effects of a first-degree social tie becoming unemployed on unemployment concerns, social policy

preferences, and vote choices are 4-8 times greater than the effects of a second-degree tie becoming unemployed. Second, and in stark contrast, the effect of unemployment shocks to first- and second-degree ties on a respondent's national unemployment outlook are relatively similar in magnitude per shock. This contrast suggests that the difference between the effects of shocks to first- and second-degree ties 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.

One possible explanation is that the differential response between subjective expectations and preferences and aggregate perceptions reflects the likelihood that first-degree social ties are more similar to respondents than second-degree ties. Respondents may then be more sensitive to unemployment shocks afflicting first-degree ties because shocks to similar people are more informative about their own risks, whereas *any* unemployment shock is relevant for inferring national aggregates. We further test this interpretation by next examining whether respondents are indeed most responsive to shocks afflicting individuals that are similar to themselves, as hypothesis H2 predicts.

6.2 Preferences and voting behavior are motivated by egotropic interests

The greater effects of unemployment shocks afflicting first-degree social ties corroborate the information diffusion mechanism, but also suggest that voters may differentiate information about unemployment shocks on the basis of their implications for their personal economic interests. We test the argument that unemployment shocks afflicting similar people provide a stronger signal of an individual's own prospects by estimating the following specifications:

$$\begin{aligned} Y_{it} \;=\; & \beta_1 \textit{Second-degree unemployment shock share}_{it} + \beta_2 \textit{Second-degree share similar}_{it} \\ & + \beta_3 (\textit{Second-degree unemployment shock share}_{it} \times \textit{Second-degree share similar}_{it}) \\ & + \gamma_{wt} + \delta_{ot} + \eta_{et} + \mu_{pt} + \varepsilon_{it}, \end{aligned} \tag{3}$$

Table 5: Heterogeneity in the effect of the share of second-degree social ties that became unemployed in the the last year on respondent economic and political beliefs, preferences, and behavior, by economic similarity of respondent and second-degree ties

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment 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)
Second-degree unemployment shock share	0.212***	0.237***	0.274	-0.145	0.224	-0.616	0.632	0.372
	(0.072)	(0.087)	(0.352)	(0.469)	(0.549)	(0.640)	(0.422)	(0.790)
Second-degree share similar	0.007	0.002	-0.098***	-0.093**	-0.028	-0.122*	0.004	0.028
	(0.004)	(0.005)	(0.020)	(0.035)	(0.047)	(0.067)	(0.040)	(0.060)
Second-degree unemployment shock share × share similar	0.047	0.105	1.705*	2.855**	0.210	2.686	0.517	2.989
	(0.189)	(0.238)	(0.934)	(1.131)	(1.442)	(1.849)	(1.313)	(2.561)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	5,209
Second-degree share similar mean	0.434	0.435	0.430	0.430	0.430	0.418	0.430	0.428

Notes: All specifications are estimated using OLS, and include respondent-level industry × year, occupation × year, education × year, and parish × year fixed effects. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

where $Second\text{-}degree\ share\ similar_{ikt}$ is the share of a respondent's second-degree ties that are in the same two-digit industry *or* in the same one-digit occupation in survey year t.[21]

In line with H2, the results in Table 5 suggest that voters indeed respond more to unemployment shocks afflicting second-degree ties that are economically similar to themselves. The interaction coefficients capture the differential effect of an increase in the share of second-degree ties that became unemployed within the last year as the share of economically similar second-degree ties rises from 0% to 100%. Our estimates show that the effect of unemployment shocks to second-degree ties on subjective unemployment expectations and support for more generous unemployment insurance is substantially greater than for unemployment shocks to dissimilar second-degree weak ties. The interaction is also large and positive for self-reported vote choice, although it is not quite statistically significant. Aggregate unemployment perceptions suggest a stark contrast, as respondents respond similarly to unemployment shocks afflicting all types of second-degree ties. This lack of distinction reinforces the finding above that respondents' beliefs respond roughly equally to aggregate employment shocks affecting first- and second-degree ties, and further indicates that greater sensitivity to shocks afflicting similar people does not simply reflect information filtering by j. Together, these results suggest that voters distinguish the relevance of different types of information

[21]The analogous approach for first-degree social ties is under-powered because, by definition, most first-degree ties are from the same industrial or educational group.

that diffuse through networks comprising strong and predominantly weak ties.

Given that individuals' economic concerns and policy preferences are more sensitive to unemployment shocks to similar individuals, these findings suggest that voters are primarily motivated by personal interests. In contrast with sociotropic accounts (H4), Table 5 demonstrates that unemployment shocks afflicting all types of second-degree tie influence perceptions of national unemployment, but only shocks to similar second-degree ties affect political preferences. In sum, these findings support an egotropic interpretation of voter preferences (H3), whereby information that diffuses between even relatively weak social ties increases a voter's own concern about unemployment, which is reflected in a stronger preference for left-wing policies and political parties.

6.3 Discussion of mechanisms

Our main results show that Danish voters' economic and political beliefs and preferences are significantly affected by unemployment shocks afflicting second-degree social ties. Furthermore, our evidence examining the mechanisms suggests that this information diffuses through the intermediaries indirectly connecting respondents to the second-degree ties that became unemployed, that voters formulate 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 and political preferences 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 an unemployment shock to a second-degree tie changes their behavior in a way that alters the social expectations governing the behavior of a first-degree tie in the presence of our respondent when the second-degree tie that suffered the shock is not present. This alternative account relies on no relevant information being transferred between individuals at either step in the chain, only changes in behavior.

However, it is unlikely that an increase in the share of k's that became unemployed would change social norms in the first-degree networks that the k's and j's share in ways that influence norms in the networks that the i's and their j's share. This is especially unlikely under our design

because k and j *and* j and i live in different locations. Furthermore, the social conformity explanation struggles to explain why respondents react more to shocks afflicting second-degree ties in the same industry or occupation, given that knowledge of their similarity could only arise from information diffusion. We thus believe that our findings most likely reflect information transmission within social networks.

An important question largely beyond the scope of this study is how, and what type of, information diffuses between strong and weak social ties. On one hand, there are major benefits to our design with respect to plausibly isolating exogenous variation in unemployment shocks—at an unprecedented scale and level of detail—that could only plausibly reach an individual via at least some information diffusion. On the other, the exact nature of what is diffused is "black-boxed" beyond the general discussions described in Figure 5. Specifically, we cannot discern what second-degree ties communicate to a respondent's first-degree ties, how this information is parsed by these intermediary connections, and what politically-relevant discussions arise between our respondents and their first-degree ties as a consequence of the second-degree ties becoming unemployed. Moreover, we do not know whether discussion about unemployment and reemployment differ in frequency or form. We are therefore unable to determine whether changes in second-degree social ties' 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.[22]

7 Conclusion

We show that information diffusion within networks of strong and mostly weak ties plays an important role in shaping economic and policy beliefs and preferences, and ultimately voting behavior. Combining Denmark's extraordinarily detailed individual-level data with a cross-sectional empirical strategy exploiting unemployment shocks to second-degree social ties at scale, we address the identification and network measurement challenges faced by previous studies investigating the

[22]Instrumenting for a peer's unemployment expectations with second-degree unemployment shocks (Bramoullé, Djebbari and Fortin 2009) is likely to violate the exclusion restriction.

impact of information diffusion within social networks. By focusing on shocks that must pass through intermediary ties, our approach also helps to distinguish information diffusion from social pressure. Our findings show that voters are highly responsive to unemployment shocks afflicting second-degree ties, influencing their beliefs about both national unemployment levels and personal unemployment risk. 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 individuals being motivated primarily by their economic self-interest, voters disproportionately alter their policy preferences and vote choices in response to shocks afflicting second-degree ties that are economically similar. This induces them to support more generous unemployment insurance and vote for left-wing political parties.

Our finding, that the political significance of information diffusion within social networks suggests that the capacity of social networks for diffusion may be just as important as networks' conformity pressures, faces two limitations. First, although this study represents a rare opportunity to employ observational data that is both detailed *and*—especially given that Denmark's political and labor market institutions and experiences with the financial crisis were similar to other Western European nations—may generalize to other economic downturns (Pietryka and DeBats 2017), our findings are nevertheless specific to the context and time period examined. Further studies are thus required to assess information diffusion's effects between relatively weak ties in other contexts, on a wider range of political behaviors, and in direct comparison with social pressure's influence.

Second, an important limitation demanding future research are the questions of what types of information are transmitted between social ties and how they are transmitted. Qualitative and panel studies in the United States observing political discussion in small communities (e.g. Huckfeldt and Sprague 1995; Walsh 2004) suggest one blueprint. Such studies could be complemented by experiments focusing on small groups in settings where communication, conformity pressures, and beliefs can be controlled and monitored (e.g. Klar and Shmargad 2017). Despite lower external validity, progress in examining how and what information is passed between both strong and weak ties relies on understanding these processes in detail.

References

Ahn, T.K., Robert Huckfeldt, Alexander K. Mayer and John Barry Ryan. 2013. "Expertise and bias in political communication networks." *American Journal of Political Science* 57(2):357–373.

Alt, James E., David D. Lassen and John Marshall. 2016. "Information sources, belief updating, and the politics of economic expectations: Evidence from a Danish survey experiment." *Journal of Politics* 78(2):327–343.

Ansolabehere, Stephen, Marc Meredith and Erik Snowberg. 2014. "Mecro-Economic Voting: Local Information and Micro-Perceptions of the Macro-Economy." *Economics and Politics* 26(3):380–410.

Aral, Sinan and Marshall Van Alstyne. 2011. "The diversity-bandwidth trade-off." *American Journal of Sociology* 117(1):90–171.

Banerjee, Abhijit, Arun G. Chandrasekhar, Esther Duflo and Matthew O. Jackson. 2013. "The Diffusion of Microfinance." *Science* 341(6144).

Barberá, Pablo. 2015. "How Social Media Reduces Mass Political Polarization: Evidence from Germany, Spain, and the U.S." American Political Science Association Annual Meeting 2015. http://pablobarbera.com/static/barbera_polarization_APSA.pdf.

Bond, Robert M., Christopher J. Fariss, Jason J. Jones, Adam D.I. Kramer, Cameron Marlow, Jaime E. Settle and James H. Fowler. 2012. "A 61-million-person experiment in social influence and political mobilization." *Nature* 489(7415):295–298.

Bramoullé, Yann, Habiba Djebbari and Bernard Fortin. 2009. "Identification of peer effects through social networks." *Journal of Econometrics* 150(1):41–55.

Caldwell, Sydnee and Nikolaj Harmon. 2019. "Outside Options, Bargaining, and Wages: Evidence from Coworker Networks." Working Paper, sydneec.github.io/Website/Caldwell_Harmon.pdf.

Carlson, Taylor N. 2019. "Through the Grapevine: Informational Consequences of Interpersonal Political Communication." *American Political Science Review* 113(2):325–339.

Carrell, Scott E., Bruce I. Sacerdote and James E. West. 2013. "From natural variation to optimal policy? The importance of endogenous peer group formation." *Econometrica* 81(3):855–882.

Chandrasekhar, Arun G. and Randall Lewis. 2016. "Econometrics of sampled networks." Working paper, https://web.stanford.edu/ arungc/CL.pdf.

Chetty, Raj, Nathaniel Hendren and Lawrence F. Katz. 2016. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment." *American Economic Review* 106(4):855–902.

Cornelissen, Thomas, Christian Dustmann and Uta Schönberg. 2017. "Peer Effects in the Workplace." *American Economic Review* 107(2):425–456.

Downs, Anthony. 1957. *An Economic Theory of Democracy*. Addison Wesley.

Eagle, Nathan, Alex Sandy Pentland and David Lazer. 2009. "Inferring friendship network structure by using mobile phone data." *Proceedings of the National Academy of Sciences* 106(36):15274–15278.

European Commission. 2004. "Eurobarometer 72.2 (Nov-Dec 2004).".

Fisman, Raymond, Pamela Jakiela and Shachar Kariv. 2015. "How did distributional preferences change during the great recession?" *Journal of Public Economics* 128:84–95.

Fowler, James H., Michael T. Heaney, David W. Nickerson, John F. Padgett and Betsy Sinclair. 2011. "Causality in political networks." *American Politics Research* 39(2):437–480.

Gerber, Alan S., Donald P. Green and Christopher W. Larimer. 2008. "Social pressure and voter turnout: Evidence from a large-scale field experiment." *American Political Science Review* 102(1):33–48.

Glitz, Albrecht and Rune Vejlin. 2014. "Information Flow in Networks of Former Coworkers." Working paper, https://goo.gl/5oW4sc.

Granovetter, Mark S. 1973. "The Strength of Weak Ties." *American Journal of Sociology* 78(6):1360–1380.

Hansford, Thomas G. and Brad T. Gomez. 2015. "Reevaluating the sociotropic economic voting hypothesis." *Electoral Studies* 39:15–25.

Huckfeldt, R. Robert and John Sprague. 1995. *Citizens, politics and social communication: Information and influence in an election campaign*. Cambridge University Press.

Huckfeldt, Robert, Paul E. Johnson and John Sprague. 2004. *Political Disagreement: The Survival of Diverse Opinions within Communication Networks*. Cambridge University Press.

Inoue, Atsushi and Gary Solon. 2010. "Two-Sample Instrumental Variables Estimators." *Review of Economics and Statistics* 92(3):557–561.

Iversen, Torben and David Soskice. 2001. "An Asset Theory of Social Policy Preferences." *American Political Science Review* 95(4):875–894.

Jackson, Matthew O. 2010. *Social and Economic Networks*. Princeton University Press.

Katz, Elihu and Paul F. Lazarsfeld. 1955. *Personal influence: The part played by people in the flow of mass communications*. Free Press.

Kendall, Chad, Tommaso Nannicini and Francesco Trebbi. 2015. "How Do Voters Respond to Information? Evidence from a Randomized Campaign." *American Economic Review* 105(1):322–353.

Kiewiet, D. Roderick. 1983. *Macroeconomics and Micropolitics: The Electoral Effects of Economic Issues*. Chicago, IL: University of Chicago Press.

Kinder, Donald R. and D. Roderick Kiewiet. 1981. "Sociotropic Politics: The American Case." *British Journal of Political Science* 11(2):129–161.

Klar, Samara and Yotam Shmargad. 2017. "The effect of network structure on preference formation." *Journal of Politics* 79(2):717–721.

Larson, Jennifer M. 2017. "Networks and interethnic cooperation." *Journal of Politics* 79(2):546–559.

Manski, Charles F. 1993. "Identification of endogenous social effects: The reflection problem." *Review of Economic Studies* 60(3):531–542.

Margalit, Yotam. 2013. "Explaining social policy preferences: Evidence from the Great Recession." *American Political Science Review* 107(1):80–103.

Margalit, Yotam. 2019. "Political Responses to Economic Shocks." *Annual Review of Political Science* 22:277–295.

Marshall, John. 2019. "Signaling sophistication: How social expectations can increase political information acquisition." *Journal of Politics* 81(1):167–186.

Moene, Karl O. and Michael Wallerstein. 2001. "Inequality, Social Insurance, and Redistribution." *American Political Science Review* 95(4):859–874.

Mutz, Diana C. 2002. "Cross-cutting social networks: Testing democratic theory in practice." *American Political Science Review* 96(1):111–126.

Nickerson, David W. 2008. "Is voting contagious? Evidence from two field experiments." *American Political Science Review* 102(1):49–57.

Nielsen, Helena Skyt and Michael Svarer. 2009. "Educational Homogamy How Much is Opportunities?" *Journal of Human Resources* 44(4):1066–1086.

Pietryka, Matthew T. and Donald A. DeBats. 2017. "It's Not Just What You Have, but Who You Know: Networks, Social Proximity to Elites, and Voting in State and Local Elections." *American Political Science Review* 111(2):360–378.

Rehm, Philipp. 2011*a*. "Risk inequality and the polarized American electorate." *British Journal of Political Science* 41(2):363–387.

Rehm, Philipp. 2011*b*. "Social policy by popular demand." *World Politics* 63(2):271–299.

Sacerdote, Bruce. 2001. "Peer Effects with Random Assignment: Results for Dartmouth Roommates." *Quarterly Journal of Economics* 116(2):681–704.

Sheridan, Adam. 2019. "Learning About Social Networks from Mobile Money Transfers." Working Paper, https://sites.google.com/view/adamsheridan/home.

Siegel, David A. 2009. "Social networks and collective action." *American Journal of Political Science* 53(1):122–138.

Sinclair, Betsy. 2012. *The social citizen: Peer networks and political behavior*. University of Chicago Press.

Steinert-Threlkeld, Zachary C. 2017. "Spontaneous Collective Action: Peripheral Mobilization During the Arab Spring." *American Political Science Review* 111(2):379–403.

Walsh, Katherine Cramer. 2004. *Talking about politics: Informal groups and social identity in American life*. University of Chicago Press.

A Online Appendix

Contents

A.1 Unemployment insurance in Denmark

Danish citizens are entitled to a means-tested government benefit (*kontanthjælp*) 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 (*dagpenge*). 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 for 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 administrative data

The administrative data used in this paper is based on several Danish administrative registers which are merged using the Danish equivalent of 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 the 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 Details of survey validating first-degree social ties

To validate our definition of social networks and our claim that the first-degree social ties identified in our data are potentially sources of information about the economy, we carried out a representative survey of the Danish adult (18+) population in November 2018. The survey was conducted by YouGov and recruited 1,506 respondents.

We first asked a series of questions aiming to elicit the fraction of ties of each type—familial, vocational, and educational—that respondents had conversed with recently. These questions were specifically designed to reflect our definition of social ties in the data. We carefully clarified the meaning of conversation, fraction of ties by tie type, and the specific groups of people to think about when answering the questions. The following question is an example of one of our questions regarding familial first-degree ties and conversations within the last year: "What percentage of

immediate family members have you had a conversation with at least once within the last year?" [Open answer, allow numbers between 0 and 100.]

We also asked a second set of questions designed to understand the extent to which people talk to others about unemployment shocks afflicting someone they know. These questions probed the *content* of what was being talked about when respondents experienced unemployment in their immediate network. One example is the following question where the information communicated is just the event itself: "When someone you know becomes unemployed, do you tell other people about their particular experience?" [Never; Rarely; Sometimes; Often; Always.]

A.4 Computation of second-degree social ties

The starting point to compute the set of second-degree social ties in the Danish population is the symmetric $7,974,509 \times 7,974,509$ adjacency matrix that captures all possible first-degree ties, as defined in the main paper. Each entry is either 0 or 1, indicating a tie between i and j. In practice this was computed by first associating i with a parent or partner, work institution (from within 2 years and satisfying our other restrictions), and educational institution, and then generating second-degree ties through the process described below. In the case of parents and partners, first-degree ties 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 social ties. Matrix multiplying $\mathbf{g}$ with itself produces a matrix containing the number of *second-degree* ties between each pair of individuals, except along the diagonal, which gives the network degree or the number of first-degree ties to other individuals. Finally, we define our second-degree matrix $\mathbf{S}$ as a matrix of indicators for second but not first-degree ties between each pair of individuals, with the diagonal set to zero. In this example, there are 3 second-degree ties, 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 ties.[3]

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

In our particular application, the definition of educational ties means that the adjacency matrix is not symmetric. This is because someone who only completed high school would be linked to someone in their graduating cohort that went on to attend university, but not vice versa. Nevertheless, unreported robustness checks show that our results are robust to extending our definition of social networks to allow for high school and university-degree graduating cohorts.

A.5 Industry, occupation, and education digits

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

A.6 Variable definitions

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

[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

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."

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 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."

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.

Second-degree unemployment shock share. The share of a respondent's second-degree social ties that were registered as unemployed in the November preceding the survey, but were not registered as unemployment in the prior November.

First-degree unemployment shock share. The share of a respondent's first-degree social ties 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 respondents that identify as women.

Age. The respondent's age in years.

Children. The number of children that a respondent has.

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

Gross income (DKK, 1,000,000). Total annual income, including wage income, government transfers, and capital income, in millions of Danish Krone.

Total assets (DKK, 1,000,000). Value of total assets, including bank deposits, bonds, stocks, and property, in millions of Danish Krone.

Total debt (DKK, 1,000,000). Value of total debt, including bank loans, credit card debt, and mortgage debt, in millions of Danish Krone.

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.

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

Second-degree network size. A respondent's number of second-degree social ties (that survive our restrictions).

Second-degree share similar. The share of a respondent's second-degree social ties that are in the same two-digit industry or the same one-digit occupation as the respondent.

First-degree familial/vocational/educational tie share. Share of respondents who are connected to first-degree social ties by a familial/vocational/educational tie.

Second-degree familial/vocational/educational tie share. Share of ties (of our respondent) who are connected to second-degree social ties (of our respondent) by a familial/vocational/educational tie.

A.7 Additional results

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

A.7.1 Using j,k pairs to compute unemployment shocks

For our main analyses, we counted a k linked to i by multiple j's only once in computing the share of second-degree ties that became unemployed. We did so because each k only represents a single piece of information. However, if respondents receive information about a given k becoming unemployed from multiple j's, the respondent may still lack the information to infer that the j's are talking about the same person. To ensure that the results are not driven by this coding decision, Table A2 reports estimates where the share of second-degree ties that became unemployed is instead computed by counting j,k pairs as independent units, rather than using k as the unit. The estimates are similar, if not larger.[5]

A.7.2 Including a lagged dependent variable

As noted in footnote 20 of the main text, our preferred identification strategy principally exploits cross-sectional variation by treating surveys as repeated cross-sections. We cannot exploit purely

[5]The sample size declines in column (8) because we, for this outcome of voting in the 2011 election, are using i,j,k pairs prior to 2011 which are calculated for 2011-survey respondents, and are using survey responses from surveys in 2012 and 2013, leaving us with a sample of individuals who participated in the 2011 survey and at least one of the later surveys.

Table A2: Estimates of second-degree social tie unemployment shocks on a respondent's economic and political beliefs, preferences, and behavior, counting j,k pairs separately

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment 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)
Second-degree unemployment shock share	0.217***	0.224***	0.943***	0.737**	0.472	0.320	0.790***	1.396*
	(0.054)	(0.057)	(0.200)	(0.327)	(0.301)	(0.394)	(0.291)	(0.802)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	3,243
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.085	0.076	0.174	0.320	0.387	0.402	0.439	0.507
Outcome std. dev.	0.055	0.050	0.302	0.467	0.487	0.490	0.496	0.500
Unemployment shocks mean	0.021	0.021	0.022	0.022	0.022	0.024	0.022	0.021
Unemployment shocks std. dev.	0.014	0.015	0.016	0.016	0.016	0.018	0.016	0.014
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. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

within-respondent variation because, at least over the four-year post-financial crisis period that we study, there is limited variation in the share of their second-degree ties that became unemployed. Indeed, a regression of our main independent variable on respondent fixed effects, as well as our baseline fixed effects, yields an R^2 of 0.82 (relative to an R^2 of 0.48 if we just include our baseline fixed effects). Intuitively, this lack of variation arises because second-degree networks comprise thousands of individuals and thus, by the law of large numbers, the share of individuals that become unemployed in a given year quickly converges to a similar expected value if the underlying unemployment risks of the network remain similar across years—which is particularly likely in the short term when fixed effects are including to extract year-specific effects. Consequently, while a design including respondent fixed effects is appealing in terms of ensuring that time-invariant unobserved differences across respondents are not driving the results, a sufficiently long time series covering a period over which underlying risks vary across second-degree networks is required in order to ensure that within-respondent variation in the main independent variable is not simply noise. A longitudinal design would also reduce our sample by around half and prevent us from analyzing some outcome variables, given that many respondents only complete a single wave of the survey.

While including respondent-level fixed effects would thus defeat the purpose of our design, we follow Margalit (2013) in showing robustness to the interim position of including a lagged

Table A3: Estimates of second-degree social tie unemployment shocks on a respondent's economic and political beliefs, preferences, and behavior, including a lagged dependent variable

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment expectation (3)	Want more unemployment insurance (4)	Government should support the poor (5)	Intend to vote for left party (6)
Second-degree unemployment shock share	0.195*	0.234**	0.461	0.756*	0.646	-0.120
	(0.103)	(0.103)	(0.373)	(0.429)	(0.564)	(0.388)
Lagged dependent variable	0.327***	0.175***	0.392***	0.381***	0.319***	0.660***
	(0.031)	(0.024)	(0.016)	(0.012)	(0.014)	(0.011)
Observations	6,355	2,465	9,538	9,538	9,538	9,538
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}
Outcome mean	0.084	0.078	0.165	0.317	0.356	0.437
Outcome sd	0.052	0.036	0.289	0.465	0.479	0.496
Unemployment shock mean	0.021	0.020	0.021	0.021	0.021	0.021
Unemployment shock sd	0.011	0.010	0.012	0.012	0.012	0.012
Survey years unavailable	2010	2010, 2012				

Notes: All specifications are estimated using OLS, and include respondent-level industry × year, occupation × year, education × year, and parish × year fixed effects. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

dependent variable. Unlike unit fixed effects or first-differencing, including a lagged dependent variable does not restrict the coefficient on the lag to unity. Although this cannot guarantee the removal of time-invariant respondent effects, it also maintains relevant cross-sectional variation in our independent variable—to the extent that the coefficient differs from 1.

The results of this robustness check are reported in Table A3 for the outcomes for which a lagged dependent variable is available. The drop in sample size varies by outcome, but generally halves relative to the main estimate in Table 2. Standard errors almost double in magnitude for most outcomes. For the outcomes in columns (1)-(5), the point estimates remain similar and statistically significant, while the coefficient on the lagged dependent variable is small. For vote intention in column (6), the coefficient on the lag is much larger, leaving mostly within-respondent variation in the outcome left to explain. Consistent with the remaining variation in the outcome largely reflecting noise around the expected share of second-degree ties that became unemployed in a given year, our estimated effect of unemployment shocks on voting intention goes to zero and ceases to be statistically significant.

A.7.3 How unemployment shocks relate to respondent unemployment prospects

The article's main results in Table 2 show that respondents' beliefs about their own unemployment risks increase in the share of their second-degree ties that became unemployed. However, are such beliefs accurate, in that such beliefs translate into actual unemployment experiences?

From a theoretical standpoint, our expectations depend on the extent to which voters can condition on the information accessible to the researcher. On one hand, it is natural to believe that a higher signal will increase a voter's self-assessed risk on the margin by providing new information risks in the economy, especially since expectations of aggregate unemployment also increase. On the other hand, our empirical strategy is designed to isolate variation in second-degree unemployment shocks that is not correlated with a respondent's own characteristics, including their unemployment risk, by conditioning on the fixed effects included in equation (1). From this perspective, unemployment shocks would only influence a respondent's probability of becoming unemployed in the future in the unlikely event that the shocks alter the respondent's own behavior or the behavior of first-degree ties in ways that affect their risk of unemployment. In short, the fact that survey respondents are influenced by unemployment shocks may be consistent with voters failing to condition on the information required to estimate our statistical models.

To assess this claim empirically, we examine the relationship between second-degree unemployment shocks and a respondent's subsequent unemployment experiences. The estimates in Table A4 examines how this relationship varies as additional fixed effects are progressively included. Suggesting that the design indeed isolates exogenous variation in second-degree unemployment shocks, columns (5) and (10) show that there is no significant relationship between such unemployment shocks and an individual's own unemployment outcomes. However, the preceding estimates demonstrate that a strong positive relationship holds unconditionally and is slowly weakened by the inclusion of our fixed effects. Together, these findings suggest that voters' responses to unemployment shock signals are relatively accurate to the point of fully adjusting for all the fixed effects included in our statistical model. This is hardly surprising since the information that we possess as researchers about industry, education, occupation, and parish likely exceeds the information that

Table A4: Estimates of second-degree social tie unemployment shocks on the probability of the respondent experiencing an unemployment shock

	Unemployment shock experienced by respondent during period...									
	...t						...$t+1$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Second-degree unemployment shock share	0.403***	0.346***	0.284***	0.145*	0.028	0.409***	0.442***	0.410***	0.277**	0.168
	(0.088)	(0.075)	(0.080)	(0.084)	(0.096)	(0.088)	(0.117)	(0.124)	(0.123)	(0.158)
Observations	15,096	13,710	13,707	13,707	13,707	10,266	9,351	9,348	9,348	9,348
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.024	0.024	0.024	0.024	0.024	0.025	0.025	0.025	0.025	0.025
Outcome std. dev.	0.153	0.152	0.152	0.152	0.152	0.157	0.156	0.156	0.156	0.156
Unemployment shocks mean	0.023	0.023	0.023	0.023	0.023	0.024	0.024	0.024	0.024	0.024
Unemployment shocks std. dev.	0.016	0.016	0.016	0.016	0.016	0.017	0.017	0.017	0.017	0.017
Parish × year fixed effects		✓	✓	✓	✓		✓	✓	✓	✓
Industry × year fixed effects			✓	✓	✓			✓	✓	✓
Occupation × year fixed effects				✓	✓				✓	✓
Education × year fixed effects					✓					✓

Notes: All specifications are estimated using OLS, while the fixed effects included in each specification are not at the foot of the table. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Estimates of second-degree social tie unemployment shocks on intention to vote for a party from the governing coalition

	Intend to vote for an incumbent party (1)
Second-degree unemployment shock share	-0.286
	(0.299)
Observations	17,816
Outcome range	{0,1}
Outcome mean	0.292
Outcome standard deviation	0.455
Unemployment shocks mean	0.023
Unemployment shocks standard deviation	0.015

Notes: Specification is estimated using OLS, and include respondent-level industry × year, occupation × year, education × year, and parish × year fixed effects. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

diffuses between individuals in the population.

A.7.4 Incumbent party vote intention

Table A5 examines the effect of unemployment shock on intention to vote for an incumbent party. Contrary to sociotropic accounts, the results show that unemployment shocks afflicting second-degree social ties do not significantly affect support for the incumbent party. Unlike the 2011 vote choice, this outcome is particularly helpful in separating the personal insurance and sociotropic explanations because the vote intention variable extends across center-right and center-left governments.

A.7.5 Full results for the specification including additional respondent covariates

Panel D of Table 3 shows that our results are robust to simultaneously adjusting for nine further respondent-level predetermined socioeconomic and demographic covariates, in addition to whether a respondent themselves became unemployed: gender, age, whether single, number of children, annual income, total asset wealth, total debt, homeowner status, and the number of second-degree social ties. Table A6 reports all the regression coefficients for each of these covariates.

Table A6: Estimates of second-degree social tie unemployment shocks on respondent economic and political beliefs, preferences, and behaviors, including additional respondent covariates

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment 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)
Second-degree unemployment shocks share	0.199***	0.246***	0.380*	0.477	0.138	0.123	0.718**	1.155**
	(0.053)	(0.057)	(0.209)	(0.359)	(0.333)	(0.438)	(0.287)	(0.564)
Female	0.016***	0.011***	0.025***	0.046***	0.028**	-0.084***	0.014	0.018
	(0.001)	(0.001)	(0.007)	(0.012)	(0.012)	(0.022)	(0.014)	(0.019)
Age	-0.000***	-0.000**	-0.001*	0.004***	0.002***	0.002**	0.002***	0.003***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Single	-0.001	-0.000	0.023***	0.036***	-0.001	-0.013	-0.004	0.004
	(0.001)	(0.002)	(0.008)	(0.013)	(0.012)	(0.014)	(0.014)	(0.017)
Number of children	-0.000	0.001	-0.008***	0.005	0.002	-0.006	0.003	-0.002
	(0.001)	(0.001)	(0.002)	(0.005)	(0.005)	(0.008)	(0.006)	(0.008)
Income (DKK 1,000,000)	-0.001	-0.001	-0.054***	-0.021	-0.025**	0.032	-0.039*	-0.117***
	(0.001)	(0.002)	(0.015)	(0.018)	(0.011)	(0.023)	(0.022)	(0.031)
Total Assets (DKK 1,000,000)	-0.000	-0.000	0.006***	0.002	-0.001	-0.004	0.003	-0.010**
	(0.000)	(0.000)	(0.001)	(0.003)	(0.003)	(0.005)	(0.004)	(0.005)
Total Debt (DKK 1,000,000)	0.000	-0.000	-0.007***	-0.010***	-0.006	-0.007	-0.014***	-0.007
	(0.000)	(0.000)	(0.002)	(0.003)	(0.004)	(0.006)	(0.004)	(0.005)
Homeowner	-0.006***	-0.003	-0.031***	-0.049***	-0.040***	0.016	-0.052***	-0.026
	(0.002)	(0.002)	(0.007)	(0.012)	(0.012)	(0.022)	(0.015)	(0.017)
Whether unemployed	0.012***	0.007	0.310***	0.142***	0.094***	0.126***	0.027	0.062
	(0.004)	(0.004)	(0.018)	(0.028)	(0.029)	(0.047)	(0.025)	(0.044)
Unemployment shock	0.006*	0.010**	0.475***	0.127***	0.095***	0.068	0.079***	0.049
	(0.003)	(0.005)	(0.017)	(0.025)	(0.027)	(0.054)	(0.021)	(0.044)
Number of second-degree social ties	-0.000	0.000	-0.000***	-0.000**	-0.000***	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	5,209
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.085	0.076	0.174	0.320	0.387	0.402	0.438	0.499
Outcome standard deviation	0.055	0.050	0.302	0.467	0.487	0.490	0.496	0.500
Unemployment shocks mean	0.022	0.022	0.023	0.023	0.023	0.025	0.023	0.022
Unemployment shocks std. dev.	0.014	0.014	0.015	0.015	0.015	0.018	0.015	0.014
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. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.7.6 Additional robustness checks reported in the main paper

The robustness tests section of the main paper briefly notes a number of additional robustness checks that we conducted. This subsection provides the results of these tests in full.

First, to further allay concerns relating to geographically-dispersed common shocks, panel A of Table A7 excludes second-degree social ties from within the same region—one of Denmark's five regions—as the respondent. Although this entails dropping around half the sample (including those between whom information is most likely to flow), and thus substantially reducing estimate precision, the point estimate magnitudes are relatively robust.

Second, our main estimates exclude k's located in the same parish as the j (or j's) that link

Table A7: Additional robustness checks not reported in the main paper

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment 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: second-degree social ties living in a different region from the respondent								
Second-degree unemployment shock share	0.096**	0.106**	0.435***	0.377	0.278	-0.374	0.093	0.711
	(0.045)	(0.042)	(0.156)	(0.254)	(0.239)	(0.287)	(0.159)	(0.626)
Observations	12,992	8,662	17,798	17,798	17,798	4,806	17,798	5,204
Panel B: removing second-degree social ties living in the same municipality as the first-degree social tie linking them to the respondent								
Second-degree unemployment shock share	0.216***	0.235***	0.597***	0.503	0.298	0.192	0.736**	1.483*
	(0.053)	(0.058)	(0.203)	(0.331)	(0.312)	(0.405)	(0.299)	(0.781)
Observations	12,999	8,666	17,815	17,815	17,815	4,816	17,815	3,242
Panel C: adjusting for social tie network construction truncation								
Second-degree unemployment shock share	0.254***	0.269***	0.928***	0.633*	0.280	0.267	0.843***	1.822**
	(0.058)	(0.059)	(0.225)	(0.357)	(0.318)	(0.429)	(0.315)	(0.776)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	3,243
Panel D: removing second-degree social ties connected to the respondent by a familial second- or first-degree weak tie								
Second-degree unemployment shock share	0.130***	0.160**	0.320	0.527*	0.752**	-0.147	0.496	0.287
	(0.049)	(0.063)	(0.231)	(0.272)	(0.328)	(0.388)	(0.299)	(0.572)
Observations	12,425	8,249	16,934	16,934	16,934	4,509	16,934	3,135
Panel E: removing second-degree social ties connected to the respondent by a educational second- or first-degree weak tie								
Second-degree unemployment shock share	0.051	0.038	0.246**	0.346**	0.337**	-0.182	0.199	0.323
	(0.040)	(0.028)	(0.112)	(0.142)	(0.132)	(0.183)	(0.165)	(0.371)
Observations	12,858	8,567	17,598	17,598	17,598	4,740	17,598	3,212
Panel F: restrict sample to respondents with at most 10,000 second-degree social ties								
Second-degree unemployment shock share	0.224***	0.257***	0.515**	0.532	0.295	-0.119	0.789**	1.052*
	(0.057)	(0.062)	(0.219)	(0.351)	(0.348)	(0.475)	(0.313)	(0.597)
Observations	9,432	6,339	13,102	13,102	13,102	3,670	13,102	3,795
Panel G: restrict sample to respondents with at most 5,000 second-degree social ties								
Second-degree unemployment shock share	0.200***	0.249***	0.451*	0.360	0.301	0.087	0.982***	1.324**
	(0.063)	(0.071)	(0.255)	(0.362)	(0.398)	(0.535)	(0.312)	(0.662)
Observations	6,214	4,216	8,718	8,718	8,718	2,504	8,718	2,563

Notes: All specifications are estimated using OLS, and include respondent-level industry × year, occupation × year, education × year, and parish × year fixed effects. The placebo test in panel A is described in the main text. Panel D includes indicators for vocational or educational network truncation at the i and j levels as controls. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

them to i. However, to further address the potential concern that j experiences shocks correlated with k, panel B 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.

Third, although our networks of social ties are unique in how comprehensively they can capture weak ties, there nevertheless remains the concern that omitted weak ties could still bias our esti-

mates. We address this concern by adjusting for indicators for respondents whose networks were truncated at the *i* and *j* levels by our vocational and educational network restrictions. The results in panel C suggest that our estimates are unlikely to reflect biases resulting from incomplete networks.

Fourth, two concerns could arise if the results depended on familial second-degree ties. First, familial ties may be more accurately measured than vocational and educational ties, and thus drive the effects that we estimate. Second, familial ties could drive the results due to a higher frequency of contact, although section 3.1 in the main paper indicates that Danes may be more likely to discuss unemployment and political issues with vocational and educational than any given familial ties. To address these potential concerns, we drop second-degree ties generated by familial first-degree ties. Panel D shows that our results are not substantively affected by dropping such ties.

Fifth, we also show that the results are robust to removing all *k*'s generated by educational ties, which Figure 2 shows to be the network tie category that produces the most ties that do not somewhat regularly converse on average. This entails removing any *k* that is only a second-degree tie to *i* because either the *i*-*j* or *j*-*k* dyad is based on an educational tie. To avoid introducing biases from incomplete networks, we do not reconstruct networks without considering educational ties. The smaller point estimates in panel E suggest that educational ties are partly driving the effects that we observe.

Finally, we show that our results are robust to focusing only on respondents with relatively small second-degree networks—that is to say, respondents for whom less than 10,000 or 5,000 second-degree social ties satisfy the restrictions defining our identification strategy above. After the latter restriction, we are only left with 5,544 unique *i*'s and 8,718 unique *i*-year observations. Although the drop in sample size reduces the power of these estimates, panels F and G reinforce our main findings by reporting similar point estimate magnitudes. These tests indicate that our results are not driven by the individuals with the largest networks of second-degree ties.

Table A8: Estimates of nonlinear effects of second-degree social tie unemployment shocks on respondent economic and political beliefs, preferences, and behavior

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment 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: Linear and quadratic estimates								
Second-degree unemployment shock share	0.220**	0.163	0.485	1.402***	0.301	0.999	0.672	2.367**
	(0.098)	(0.111)	(0.362)	(0.442)	(0.516)	(0.807)	(0.475)	(1.183)
Second-degree unemployment shock share^2	0.034	0.489	1.625	-4.729***	-0.137	-6.035	0.681	-10.432
	(0.376)	(0.317)	(1.619)	(1.307)	(2.292)	(4.254)	(1.386)	(10.050)
Unemployment shocks mean	0.022	0.022	0.023	0.023	0.023	0.025	0.023	0.022
Unemployment shocks std. dev.	0.014	0.014	0.015	0.015	0.015	0.018	0.015	0.014
Panel B: Nonparametric estimates								
Second-degree unemployment shock share Q2	-0.001	-0.001	0.011	0.027**	0.014	0.022	0.021*	0.059***
	(0.001)	(0.002)	(0.008)	(0.013)	(0.013)	(0.026)	(0.011)	(0.020)
Second-degree unemployment shock share Q3	0.003	0.003	0.014*	0.045***	0.019	-0.001	0.021	0.057**
	(0.002)	(0.003)	(0.008)	(0.015)	(0.015)	(0.032)	(0.017)	(0.025)
Second-degree unemployment shock share Q4	0.007***	0.005	0.022**	0.048***	0.016	0.034	0.021	0.039
	(0.003)	(0.003)	(0.011)	(0.016)	(0.017)	(0.038)	(0.017)	(0.025)
Unemployment shocks Q1 mean	0.010	0.010	0.010	0.010	0.010	0.009	0.010	0.010
Unemployment shocks Q2 mean	0.016	0.016	0.016	0.016	0.016	0.017	0.016	0.016
Unemployment shocks Q3 mean	0.024	0.024	0.024	0.024	0.024	0.027	0.024	0.024
Unemployment shocks Q4 mean	0.038	0.038	0.041	0.041	0.041	0.048	0.041	0.039
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	5,209
Outcome mean	0.085	0.076	0.174	0.320	0.387	0.402	0.439	0.500
Outcome standard deviation	0.055	0.050	0.302	0.467	0.487	0.490	0.496	0.500

Notes: All specifications are estimated using OLS, and include respondent-level industry × year, occupation × year, education × year, and parish × year fixed effects. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.7.7 Tests for nonlinearity of effects

Table A8 reports two test of effect linearity. Panel A includes a quadratic term for the second-degree unemployment shock share. Panel B divides the distribution of the second-degree unemployment shock shares into quartiles. The mean share in the first quartile is 0.010, while the mean share in the second quartile is 0.016, the mean share in the third quartile is 0.024, and the mean share in the fourth quartile is 0.041. The mix of mostly statistically insignificant positive and negative coefficients on the quadratic term in panel A provides little evidence of systematic nonlinearity. The increasing effects by quartile in panel B minimally imply generally monotonically increasing effects, but also suggest a relatively linear relationship.

A.7.8 Heterogeneity by type of social tie

Tables A9 and A10 respectively report interactions between the share of second-degree social tie unemployment shocks and the share of type of ties between respondent and first-degree tie and between first-degree and second-degree ties. 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.

Table A9: Estimates of second-degree social tie unemployment shocks on respondent economic and political beliefs, preferences, and behavior, by type of respondent-first-degree tie relationship

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment 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)
Second-degree unemployment shock share	-1.519	-0.648	2.538	-15.009**	-2.476	3.868	-8.057	-29.954*
	(1.191)	(1.172)	(4.533)	(7.469)	(7.875)	(7.309)	(8.450)	(17.370)
First-degree familial tie share	-0.020	-0.010	-0.144**	-0.519***	-0.121	0.186	-0.290*	-0.644**
	(0.017)	(0.015)	(0.066)	(0.139)	(0.138)	(0.191)	(0.168)	(0.286)
First-degree vocational tie share	-0.023	-0.011	-0.231***	-0.550***	-0.174	0.142	-0.264*	-0.501*
	(0.016)	(0.014)	(0.063)	(0.133)	(0.128)	(0.178)	(0.158)	(0.272)
First-degree educational tie share	-0.022	-0.011	-0.204***	-0.540***	-0.125	0.082	-0.250	-0.526*
	(0.017)	(0.014)	(0.062)	(0.135)	(0.134)	(0.167)	(0.169)	(0.276)
Second-degree unemployment shock share	1.652	0.831	-1.652	14.941**	2.573	-4.620	8.805	32.403*
× First-degree familial tie share	(1.194)	(1.169)	(4.481)	(7.426)	(7.994)	(7.449)	(8.427)	(17.290)
Second-degree unemployment shock share	1.787	0.977	-2.170	16.574**	3.833	-2.960	8.871	28.791*
× First-degree vocational tie share	(1.183)	(1.177)	(4.557)	(7.338)	(7.782)	(7.170)	(8.178)	(16.857)
Second-degree unemployment shock share	2.077*	1.124	-0.888	16.025**	2.275	-1.500	8.322	31.258*
× First-degree educational tie share	(1.174)	(1.150)	(4.438)	(7.259)	(7.766)	(7.210)	(8.513)	(16.829)
Outcome mean	0.085	0.076	0.174	0.320	0.387	0.402	0.439	0.507
Outcome std. dev.	0.055	0.050	0.302	0.467	0.487	0.490	0.496	0.500
Unemployment shocks mean	0.022	0.022	0.023	0.023	0.023	0.025	0.023	0.022
Unemployment shocks std. dev.	0.014	0.014	0.015	0.015	0.015	0.018	0.015	0.013
First-degree familial tie share mean	0.182	0.187	0.186	0.186	0.186	0.197	0.186	0.171
First-degree educational tie share mean	0.403	0.404	0.403	0.403	0.403	0.404	0.403	0.413
First-degree vocational tie share mean	0.445	0.439	0.441	0.441	0.441	0.428	0.441	0.448
F-test: Shock × familial = Shock × educational (p-value)	0.007	0.178	0.274	0.302	0.824	0.092	0.602	0.631
F-test: Shock × familial = Shock × vocational (p-value)	0.304	0.524	0.482	0.058	0.138	0.202	0.941	0.115
F-test: Shock × educational = Shock × vocational (p-value)	0.057	0.438	0.117	0.677	0.250	0.416	0.639	0.280
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. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Estimates of second-degree social tie unemployment shocks on respondent economic and political beliefs, preferences, and behavior, by type of first-degree-second-degree tie relationship

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment 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)
Second-degree unemployment shock share	-5.378*	-7.160**	40.020***	-19.842	22.578	-54.335*	-7.729	13.674
	(2.746)	(3.472)	(12.513)	(17.860)	(19.896)	(31.435)	(24.959)	(55.771)
Second-degree familial tie share	-0.049	-0.043	0.182	-0.703***	-0.103	-0.347	-0.626**	-0.480
	(0.030)	(0.037)	(0.131)	(0.189)	(0.233)	(0.359)	(0.290)	(0.716)
Second-degree vocational tie share	-0.070**	-0.069**	-0.054	-0.780***	-0.130	-0.534	-0.501*	-0.203
	(0.027)	(0.034)	(0.123)	(0.190)	(0.211)	(0.345)	(0.285)	(0.619)
Second-degree educational tie share	-0.072**	-0.073**	-0.060	-0.737***	-0.104	-0.544	-0.518*	-0.230
	(0.027)	(0.034)	(0.128)	(0.194)	(0.218)	(0.339)	(0.280)	(0.641)
Second-degree unemployment shock share	5.321*	7.217**	-42.184***	20.866	-24.302	51.939	10.240	-5.751
× Second-degree familial tie share	(2.753)	(3.515)	(12.519)	(17.966)	(19.710)	(31.674)	(24.672)	(56.240)
Second-degree unemployment shock share	5.510**	7.394**	-39.257***	19.457	-22.423	53.933*	8.539	-13.452
× Second-degree educational tie share	(2.730)	(3.445)	(12.582)	(17.902)	(19.859)	(31.347)	(24.891)	(55.699)
Second-degree unemployment shock share	5.788**	7.456**	-38.729***	21.124	-21.889	55.361*	8.034	-11.628
× Second-degree vocational tie share	(2.734)	(3.455)	(12.370)	(17.705)	(19.783)	(31.440)	(24.874)	(55.573)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	3,243
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.085	0.076	0.174	0.320	0.387	0.402	0.439	0.507
Outcome std. dev.	0.055	0.050	0.302	0.467	0.487	0.490	0.496	0.500
Unemployment shocks mean	0.022	0.022	0.023	0.023	0.023	0.025	0.023	0.022
Unemployment shocks std. dev.	0.014	0.014	0.015	0.015	0.015	0.018	0.015	0.013
Second-degree familial tie share mean	0.035	0.035	0.035	0.035	0.035	0.036	0.035	0.034
Second-degree educational tie share mean	0.333	0.334	0.332	0.332	0.332	0.328	0.332	0.323
Second-degree vocational tie share mean	0.654	0.653	0.654	0.654	0.654	0.654	0.654	0.664
F-test: Shock × familial = Shock × educational (p-value)	0.509	0.772	0.043	0.311	0.353	0.683	0.425	0.102
F-test: Shock × familial = Shock × vocational (p-value)	0.074	0.637	0.007	0.861	0.217	0.492	0.373	0.102
F-test: Shock × educational = Shock × vocational (p-value)	0.124	0.817	0.571	0.031	0.630	0.245	0.610	0.442
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. Standard errors are clustered by respondent municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.