

CONTAGIOUS POLITICAL CONCERNS: HOW UNEMPLOYMENT INFORMATION PASSED BETWEEN WEAK TIES INFLUENCES DANISH VOTERS *

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While social pressure between close network ties is widely believed to influence voters, evidence that information passed between weak ties affects beliefs, policy preferences, and behavior is limited. We investigate such information diffusion by examining whether weak ties relay information about unemployment shocks in Denmark. We link surveys with rich population-level administrative data to overcome several difficulties of identifying causal effects. Mapping each respondent's familial, vocational, and educational ties, we find that unemployment shocks afflicting second-degree weak ties—individuals that voters interact with indirectly—increase a voter's self-assessed risk of becoming unemployed, perception of the national unemployment rate, support for unemployment insurance, and voting for left-wing political parties. Voters update about national aggregates from all shocks equally, whereas subjective perceptions and preferences respond primarily to unemployment shocks afflicting second-degree weak ties in similar industries. This implies that political preferences driven by information transmitted through weak ties principally reflect individual insurance—rather than sociotropic—motives.

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1 Introduction

It is widely believed that social networks 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).

Research seeking to estimate causal effects of social networks in political contexts has focused predominantly on the importance of *close ties*—typically, a few self-identified close friends or family members. Such networks may exert powerful pressures to conform with norms of political engagement through explicit threats or learned norms (e.g. Bond et al. 2012; Gerber, Green and Larimer 2008; McClendon 2014; Nickerson 2008; Sinclair 2012) or transmit information that influences citizens’ attitudes and voting behavior (e.g. Carlson 2019; Huckfeldt and Sprague 1995; Katz and Lazarsfeld 1955). While the political significance of close ties is now well-established, it is believed to largely reflect networks’ social pressure function (Sinclair 2012).

In contrast, little is known about the political significance of *weak ties*—the many acquaintances, including the few close ties, that individuals may communicate with at least occasionally. Beyond the weaker pressures to conform between such ties, this lack of academic attention likely also reflects the difficulty of using interview or survey techniques to reliably map potentially large networks of acquaintances (Eagle, Pentland and Lazer 2009). Nevertheless, weak ties may exert substantial influence because their more differentiated networks make them more likely to possess novel information than close ties (Granovetter 1973). For example, weak ties have been shown to diffuse information that facilitates employment (e.g. Caldwell and Harmon 2018; Granovetter 1973). Analogously, since voters in even the world’s best educated and politically engaged democracies are often relatively poorly informed about their economic and political environment (e.g. Delli Carpini and Keeter 1996), cheap and frequent access to information via webs of weak ties has

the potential to substantially affect voters' beliefs.

This article examines the effect of unemployment information that passes between weak ties on an individual's perception of their own unemployment prospects and the national economy's overall performance, policy preferences, and voting behavior. In the aftermath of the 2008 financial crisis, we estimate the effects of other individuals becoming unemployed within the last year in Denmark. In a context where policies to address unemployment were politically salient, such an event—to the extent that information about it diffuses between weak ties—could affect political attitudes through two main channels. On one hand, insurance interests may drive a voter's economic and social policy preferences (e.g. Iversen and Soskice 2001; Moene and Wallerstein 2001; Rehm 2011*b*). This implies that information inducing individuals to believe that they 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 eliciting economic and political beliefs, preferences, and behaviors between 2010 and 2013, we address two major obstacles to identifying the political effects of unemployment shocks transmitted through weak ties. First, our rich administrative data enables us to objectively and accurately map networks of ties through which information could pass for all living Danes since 1980. Our network of (mostly weak) ties includes: (i) nuclear family and partner; (ii) recent co-workers; and (iii) the graduating cohort of an individual's most recent degree program. Sheridan (2019) shows that money is frequently transferred between such ties in Denmark, which we further validate using a nationally representative survey that demonstrates that conversations—including about unemployment—are also common. While survey methods effectively identify close ties, our approach to mapping weak ties at scale minimizes the risk of biases introduced by omitting relevant ties—which can be severe (Chandrasekhar and Lewis 2016)—that are likely to emerge when relying on respondents' recall of weak ties.

Second, the administrative data enable us to estimate the effects of *second-degree* weak ties—individuals about whom someone could learn about through shared first-degree weak 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 weak ties, in order to distinguish information diffusion from social conformity pressures or emotional reactions that are most likely to arise when people that you actually know become 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 information must pass. Second, beyond focusing on shocks affecting second-degree weak ties, we address the concern 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 sample of second-degree weak ties to those living in different locations from either the respondent or the weak tie connecting the respondent to the individual that became unemployed; 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 weak ties. Indicating that information relatively frequently passes between weak ties, each additional unemployment shock induces voters to raise their expectations of unemployment—for both themselves and the country at large. 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. The shocks also alter 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 weak ties is a key force underlying policy preferences and voting behavior, the magnitude of our estimates could

easily account for the left-bloc's wafer-thin electoral victory in 2011. Indeed, a standard deviation increase in the share of the median respondent's second-degree weak ties becoming unemployed increases the probability of voting for a left-wing party by more than 0.2.

Two placebo tests further address concerns about common shocks. Specifically, we show: (i) no effect of unemployment shocks afflicting second-degree weak ties on respondents that do not discuss unemployment or politics with their first-degree weak ties; and (ii) no effect of unemployment shocks experienced by placebo second-degree weak ties linked to randomly selected first-degree weak ties that have similar socioeconomic characteristics to actual first-degree weak ties.

Although we cannot directly observe interactions between millions of voters, our analysis of transmission mechanisms suggests that our findings reflect information diffusion within networks of weak ties and insurance interests guiding responses to updated beliefs about unemployment. First, 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 weak ties sharing unemployment information relatively frequently, responses to first-degree weak ties becoming unemployed are around five times greater than responses to second-degree weak ties becoming unemployed. Third, increased self-assessed unemployment risks and increased support for left-wing parties primarily reflects shocks to second-degree weak ties *in the same industry* as a respondent. Such heterogeneity suggests that individual insurance interests drive political preferences. This interpretation is supported by voters not differentiating between the industries of second-degree weak ties when they update about the national unemployment rate, and not altering their policy preferences or voting behavior when second-degree weak ties *in different industries* become unemployed.

This article makes two main contributions. First, we leverage network data with unprecedented detail to demonstrate that *weak ties* play an important role in the political lives of Danish voters by transmitting information. Our findings chime with seminal studies suggesting that weak ties facilitate job opportunities (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 (McClendon 2014; 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 transmission within networks in 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 primarily influence policy preferences and voting behavior via insurance, rather than sociotropic, considerations. We overcome the difficulty of distinguishing such accounts (Ansolabehere, Meredith and Snowberg 2014) by separating personal and national unemployment expectations and differentiating sensitivity to the similarity of the industry of the individual that becomes unemployed. These results support the insurance-based theories proposed by, among others, Iversen and Soskice (2001), Moene and Wallerstein (2001), and Rehm (2011*b*). Moreover, our findings suggest that the wealth of previous findings attributed to sociotropic voting (e.g. Hansford and Gomez 2015) could instead reflect voters updating about their own prospects from the signals they receive within their social networks.

2 Information transmission through social networks

The potential for information to diffuse through networks to reach uninformed individuals is widely recognized (see Jackson 2010).¹ Indeed, citizens continue to become informed about job opportunities and increase their productivity through their social ties (e.g. Caldwell and Harmon 2018;

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

Cornelissen, Dustmann and Schönberg 2017). Although communication with close ties is more frequent due to greater exposure and motivation, the more diverse information received via occasional interactions with weak ties is most valuable (Aral and Van Alstyne 2011; Granovetter 1973). The informative role of social networks is 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). Consequently, many voters in advanced democracies acquire much of their political information from a somewhat diverse group of friends and family (Huckfeldt, Johnson and Sprague 2004; Kiewiet 1983), and particularly better-informed opinion leaders 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 close and weak ties in some form. Where individuals have imprecise prior beliefs about economic conditions, learning that another individual became unemployed could influence a voter's perception of both aggregate unemployment and their own likelihood of becoming unemployed. In line with studies demonstrating that 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 thus expect that:

- H1.** *Being exposed to information about unemployment shocks via weak ties will increase an individual's perception of aggregate unemployment, and—where the type of shock is informative about an individual's own unemployment risk—the risk of becoming unemployed themselves.*

Persistent changes in posterior beliefs about national and individual unemployment prospects could in turn alter voters' political preferences and voting behavior. This could reflect insurance or sociotropic logics, depending on the factors that drive voter decisions. Proponents of the insurance argument posit that voters facing higher individual or occupational unemployment risks will increase their support for government programs, including demanding more generous unemploy-

ment insurance in the face of greater risks of becoming unemployed (Cusack, Iversen and Rehm 2006; Iversen and Soskice 2001; Moene and Wallerstein 2001; Rehm 2011b) and voting for the left-wing political parties typically espousing such policies (Rehm 2011a). To the extent that learning of unemployment shocks afflicting others causes voters to update their own expectations of becoming unemployed, the insurance logic predicts that:

H2. *If voters are guided by insurance motivations, being exposed to information about unemployment shocks via weak 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, although such policies are costly for higher-income voters when employed.

While the insurance 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:

H3. *If voters are guided by sociotropic motivations, being exposed to information about unemployment shocks via weak 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 weak 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 and activities,² are the primary basis of social ties in Danish society. A [European Commission \(2004\)](#) survey shows that 64% of adults report having social contact with friends at least once a week, while 44% report that they meet socially with colleagues outside of work at least once a month. Furthermore, 52% report that they would rely on their social network to receive help with paperwork (related to taxes, social benefits, etc.), 73% say that they would use their social network to discuss private problems, and 40% indicate that they would use their social network to borrow money.

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. Almost all students complete a total of 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 degrees, the geographic diversity of social ties often expands at this point. In the labor market, individuals are likely to spend more time with coworkers than almost anyone else. The relevance of workplace networks is emphasized by [Glitz and Vejlin \(2014\)](#) and [Caldwell and Harmon \(2018\)](#), who find that labor market information from former coworkers affects, respectively, displaced workers' re-employment probabilities and job-to-job mobility.

Discussion of unemployment and politics more generally among family members, current and former colleagues, and cohorts from the most recently completed educational program is common. 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 col-

²Low church attendance means that religious networks are weak.

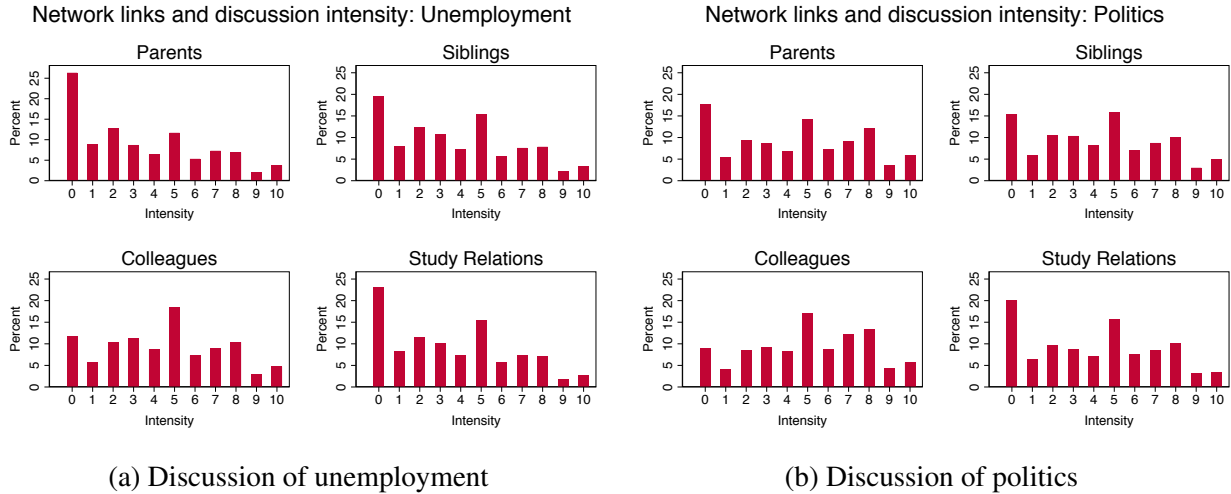


Figure 1: Frequency of discussion of political issues within social networks in the working age population (source: Danish Panel Study of Income and Asset Expectations 2015)

leagues. 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 networks 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 former fellow-students, 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

³Statistics Denmark, Statistikbanken, Flytninger, table FLY33 and table FLY66, [link](#).

⁴Gross (unlike net) unemployment counts those in active labor market programs as unemployed.

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 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 weak ties, before detailing our empirical strategy for identifying the effects of unemployment shocks afflicting second-degree weak ties that could diffuse through networks defined by individuals’ weak ties.

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 subjec-

⁵Danish Election Study, cited [here](#).

⁶The 2011 Danish Election Study poll is available [here](#). The 2013 Jyllands-Posten poll is [here](#).

⁷The insurance system and this reform are described further in Appendix section [A.1](#).

Table 1: Summary statistics from each sample

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

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

tive unemployment perceptions, policy preferences, and vote choice. The first and all subsequent waves were 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 it is slightly older, better educated, and richer, Table 1 shows that this sample broadly resembles the Danish working age population.

Second, to define unemployment shocks and weak ties between individuals, we rely on detailed individual-level administrative data for the entire population. These government-collected registers, which contain family ties, education, and income tax returns, are available annually between 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 personal and national expectations. First, an individual’s risk of becoming unemployed is based on their self-assessed probability, from 0 to

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

1, of becoming unemployed in the forthcoming year. Second, 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.

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 measures.

4.1.2 Individual unemployment shocks

To capture individual unemployment shocks that represent novel and (potentially) relevant information, we focus 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. We therefore do not distinguish

⁹In each case, “don’t know” or “none of the above” were coded as 0.

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

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

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

between unemployment duration or the number of times an individual became unemployed in a given year. Furthermore, the timing of such shocks makes it unlikely that survey respondents heard about them just before completing the surveys conducted in January and February.

4.1.3 Mapping social networks

Our administrative data presents an unprecedented opportunity to extensively map weak ties throughout the 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 could pass. For example, [Chandrasekhar and Lewis \(2016\)](#) prove that missing ties can produce non-classical measurement error that can severely upwardly bias estimates, even when nodes are missing at random. Furthermore, our estimates would be upwardly biased if unemployment shocks directly or indirectly affecting weak ties were correlated with shocks affecting other unmeasured weak ties. Such concerns are especially pertinent in our context where information about unemployment shocks is likely to pass between weak ties ([Caldwell and Harmon 2018](#); [Glitz and Vejlin 2014](#); [Granovetter 1973](#)). Indeed, [Figure 1](#) suggests that unemployment is not such a sensitive issue that individuals would be unwilling to share employment experiences. By adopting a relatively exhaustive definition of weak ties to minimize biases, our approach likely captures a lower bound on the effect of information passed between close ties. The lower-frequency interactions between most weak ties also imply a different type of treatment from studies examining close ties.

Specifically, we define an individual's *first-degree* weak ties—acquaintances with which individuals may, at least probabilistically, interact with occasionally—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.

¹³Siblings 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.

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.

The firm size restriction reflects the likelihood that individuals in large firms interact most with recent colleagues doing similar types of job 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 controlling for indicators of firm- and education-level network truncation as a robustness check.

Although the interaction between some ties may be negligible, our operationalization of weak ties captures meaningful real-world communications between many Danes. First, data from the widespread mobile money app MobilePay shows that these familial, vocational, and educational ties are all significant predictors of electronic payments between Danes (Sheridan 2019). Moreover, the importance of high school and university ties persists beyond an individual's 20s. Second, we further conducted a nationally representative survey of 1,506 Danish adults in November 2018 to validate our operationalization of weak ties.¹⁵ Figure 2 shows that respondents report having conversed with significant numbers of the weak ties that we define. 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 sustain may comprise their closest ties. The robustness checks in Appendix Table A3 show 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 ad-

¹⁴The 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.

¹⁵Appendix section A.3 describes the survey protocols.

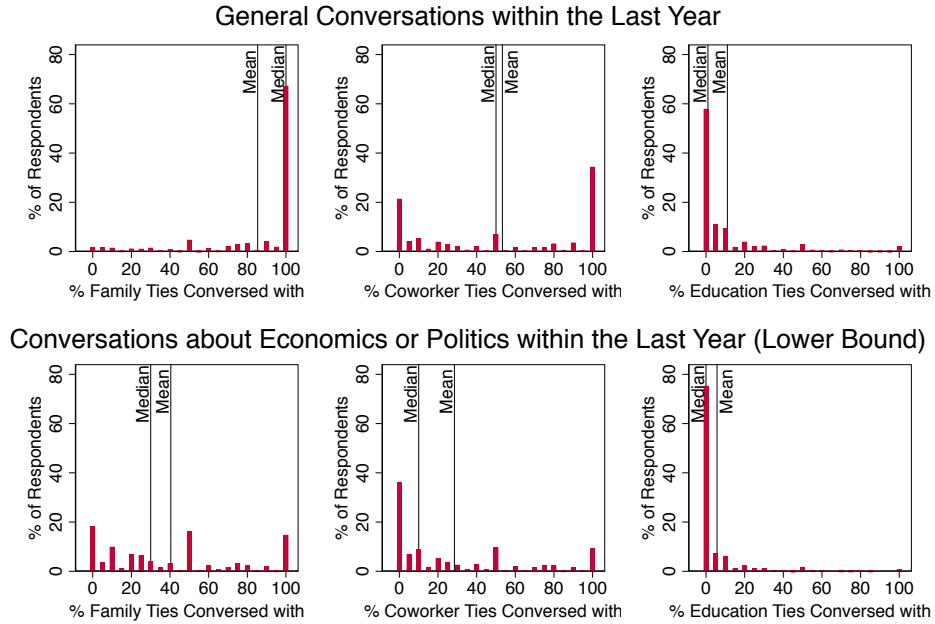


Figure 2: Histograms showing the share of first-degree weak ties that individuals converse with

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 conversated with about each topic.

jacency matrix characterizing weak 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 weak 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 weak ties, of which 2% and 8%, 74% and 49%, and 24% and 53% are familial, vocational, and educational ties, respectively. Among our respondents, 91% experience at least one unemployment shock within their first-degree network of weak ties in a given year; this rises to over 99% among second-degree ties.

4.2 Identification strategy

Our goal is to estimate the effect of information about unemployment shocks transmitted through weak ties on an individual’s economic and political beliefs, preferences, and behavior. To maintain

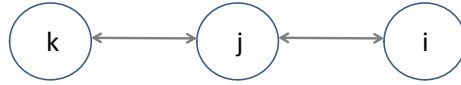


Figure 3: Illustration of undirected connections between weak ties

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 .

a reasonable probability that information about unemployment shocks reaches our 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* weak ties—individuals that are the weak ties of a respondent’s own weak ties, but are not themselves directly connected to the respondent. We further argue that, after removing nearby second-degree weak ties and controlling for fine-grained fixed effects that account for a wide array of differences between respondents, such distant shocks are exogenous to other determinants of respondent beliefs, preferences, and behavior. We now explain this identification strategy in detail.

More formally, our empirical 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 Leveraging this idea, we exploit unemployment shocks to second-degree weak ties k that are connected to i through a first-degree weak tie j . Consequently, an unemployment shock to k should only affect i through its impact on j . Figure 3 illustrates this approach, where i is our panel survey respondent, j is her first-degree weak ties, and k is her second-degree weak tie.

This approach addresses two key challenges impeding the estimation of causal effects within all kinds of 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 social effects must pass. Second, by focusing on unemployment *shocks*—rather than persisting circumstances such as parental wealth (e.g.

¹⁶Appendix section A.4 illustrates how second-degree weak ties are constructed.

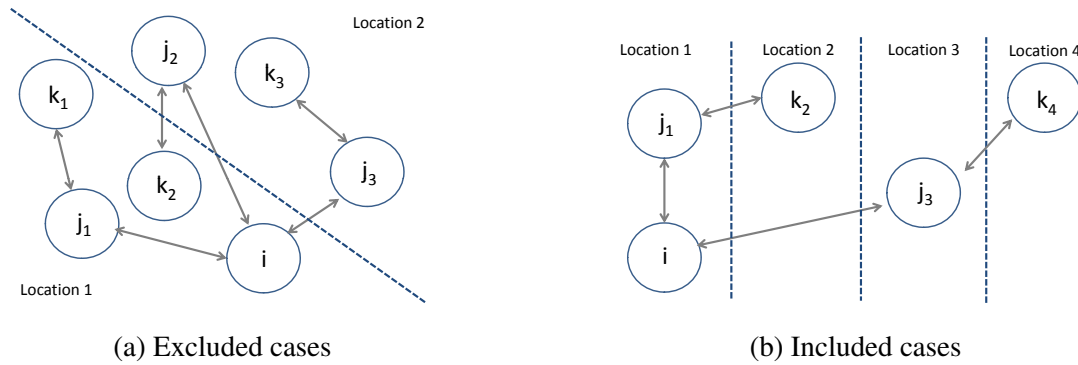


Figure 4: Illustration of excluded and included second-degree weak ties

Bramoullé, Djebbari and Fortin 2009)—and shocks with two degrees of separation, we mitigate the risk that common characteristics reflecting homophily drive k 's effect on i . Absent such common shocks, it is hard to imagine how i —who is not a first-degree weak tie of k —could be affected by k 's unemployment shock other than by becoming informed by j .

Nevertheless, a key concern is that indirectly-connected weak 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 address the spatial component of this concern by first excluding all second-degree weak ties k located in the same municipality as i .¹⁷ Second-degree weak 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 weak tie k located in a parish where any first-degree weak tie j resides.¹⁸ This excluded case is exemplified by the second-degree weak 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 . This is represented in Figure 4b. These two restrictions reduce the number of eligible second-degree weak ties by around half, from a survey respondent mean of 17,632 and a median of 7,831 second-degree weak ties in a given year, to a mean of 7,130 and a median of 4,364. Nevertheless, Table 1 shows that the socioeconomic characteristics of working age second-degree weak ties in our sample remain broadly similar to the

¹⁷Appendix Table A3 shows similar results if we further exclude k 's from i 's region.

¹⁸Appendix Table A3 reports similar point estimates when k 's located in the same municipality as j are excluded.

working age population over the 2010-2013 period.

We further address more general common shocks by including fine-grained fixed effects. Specifically, we use four sets of fixed effects at the $i \times \text{year}$ -level to restrict attention to variation in unemployment shocks that arise due to differences in network composition between individuals within the same industry, occupation, educational, and geographic groupings.¹⁹ First, i 's industry \times year fixed effects absorb common economic and political attitudes as well as vocational interactions among voters within a given two-digit industry classification in a given year. Second, i 's occupation \times year fixed effects fully control for differences across one-digit occupational classifications in a given year. Third, i 's education \times year fixed effects capture differences across time in the attitudes of voters within a given one-digit educational classification. Appendix section A.5 describes these digit classifications. Fourth, i 's parish \times year fixed effects absorb parish-specific shocks—such as common community preferences, local meetings, or localized media coverage—that could induce individuals with different networks to adopt similar unemployment concerns and political preferences. Together, these fixed effects control for many potential common shocks, and increase confidence that our estimates reflect differences in the distribution of second-degree shocks experienced by otherwise similar individuals. Further robustness checks use a variety of more demanding control strategies, sample restrictions, and placebo tests to assuage lingering concerns.

In sum, we estimate the effect of an unemployment shock to individual k on respondent i 's beliefs, attitudes, and behavior using the following OLS regression,

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

where $\text{Unemployment shock}_{kp't}$ represents an unemployment shock—being unemployed two months before the survey, having not been unemployed a year earlier—to a second-degree weak tie k of individual i located in parish p' in year t . Respectively, γ_{wt} , δ_{ot} , η_{et} , and μ_{pt} are i -level industry-year, occupation-year, education-year, and parish-year fixed effects. Standard errors are double-clustered

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

Table 2: Estimates of second-degree weak tie unemployment shocks on a respondent’s economic and political perceptions and preferences

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

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

by both i ’s municipality and k ’s municipality. We weight each survey response equally by weighting observations by the inverse of i ’s number of second-degree weak ties in survey year t .

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

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

5.1 Effects of unemployment shocks afflicting second-degree weak ties

Table 2 reports our main results, using equation (1) to estimate the effect of an unemployment shock to a second-degree weak tie on working-age Danish voters. Our 141 million observations reflect all second-degree weak ties that survive the sample restrictions described above.

Our first main finding is that voters significantly update their beliefs about their *own* unemployment risk following a shock afflicting a second-degree weak tie. Column (1) shows that each such

shock increases an individual's self-assessed probability of becoming unemployed within the next year by 0.0035 probability points, or 0.35 percentage points. This represents a 0.01 standard deviation increase in this self-assessed risk, or a 2.5% increase relative to the mean. However, because individuals have many second-degree weak ties, this estimate implies substantial effects for individuals within a typical network: a standard deviation increase in the share of second-degree weak ties becoming unemployed (1.5 percentage points) for the median respondent with 4,364 second-degree weak ties entails a 0.23 point increase in an individual's subjective probability of becoming unemployed. These results imply lasting effects consistent with voters receiving information that updates their prior beliefs. This suggests that, while voters may generally form unemployment risk perceptions based on their industry or occupation (e.g. [Cusack, Iversen and Rehm 2006](#); [Rehm 2011a](#)), such beliefs are not fixed because voters still place significant weight on the unemployment experiences of second-order weak ties that are relayed by "word of mouth."

In addition to updating their subjective unemployment expectations, columns (2) and (3) demonstrate that voters' *aggregate* unemployment beliefs respond to unemployment shocks transmitted through first-degree weak ties. Our point estimates indicate that unemployment shocks increase both an individual's current guess at the national unemployment rate and their expectation for the coming year by almost 0.001 points, or 0.1 percentage points. In both cases, a standard deviation increase in the share of second-degree weak ties becoming unemployed for the median respondent implies around a 0.06 point 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 also quite malleable.

Beyond influencing a respondent's economic outlook, these unemployment shocks also drive their policy preferences. In particular, column (4) shows that a shock to a second-degree weak tie significantly increases the probability that an individual supports more generous unemployment insurance. A standard deviation increase in the share of the median respondent's second-degree weak ties becoming unemployed entails an 0.20 point increase in the probability of supporting more generous unemployment insurance. Although they are not statistically significant, columns

(5) and (6) suggest that unemployment shocks may also increase support for redistribution toward the poor and non-market-based government stimulus. It is possible that changes in support for such measures are more limited because they are less directly targeted toward citizens expecting to become unemployed. Together, these findings suggest that unemployment shocks transmitted through 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 an unemployment shock to a second-degree weak tie significantly increases the intention to vote for a left-wing party and actually voting for a left-wing party in the 2011 election by 0.0037 and 0.005 probability points respectively. A standard deviation increase in the share of second-degree weak ties becoming unemployed for the median respondent thus entails 0.24 and 0.33 point increases in the probability of left-wing voting. These substantial effects suggest that information transmitted through networks of weak ties could alter electoral outcomes and governing coalitions, particularly in the competitive elections experienced recently in Denmark.

Thus far, these results are consistent with both the insurance and sociotropic voting motivations. Indeed, the elevated vote for the left-wing opposition party in 2011 could also reflect sociotropic voting, since the center-right was in power. However, Appendix Table A2 shows that an unemployment shock does not reduce intention to vote for the *government*, which comprised left-wing parties in 2012 and 2013 survey rounds. We provide further evidence against the sociotropic interpretation of voter responses below, showing that voters' political preferences respond primarily to concerns about their own unemployment risks.

Although social interactions between familial, vocational, and educational weak ties are all common in Denmark, it is natural to consider heterogeneity by type of weak tie. Appendix Tables A4 and A5 interact unemployment shocks with the type of ties linking a respondent to a first-degree weak tie and linking a respondent's first-degree weak tie to a second-degree weak tie. Although the effects of vocational ties are generally most pronounced, the results generally suggest that shocks

Table 3: Placebo and main robustness checks

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market- based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: placebo where no information is transferred between respondent and first-degree weak tie								
Placebo unemployment shock	0.0027 (0.0022)	-0.0001 (0.0006)	-0.0008 (0.0010)	0.0007 (0.0019)	0.0047** (0.0023)	0.0015 (0.0028)	-0.0035 (0.0029)	0.0065 (0.0044)
Observations	3,467,055	2,786,265	1,874,529	3,467,055	3,467,055	680,790	3,467,055	831,183
Panel B: placebo examining shocks to similar first-degree weak ties that the respondent does not know								
Placebo unemployment shock	0.0018 (0.0023)	0.0004 (0.0005)	0.0004 (0.0008)	0.0015 (0.0025)	-0.0054* (0.0029)	0.0005 (0.0051)	-0.0077*** (0.0026)	-0.0173** (0.0078)
Observations	147,612,755	111,546,689	73,061,520	147,612,755	147,612,755	36,066,066	147,612,755	30,094,652
Panel C: controlling for second-degree weak tie industry \times year, occupation \times year, education \times year, and parish \times year fixed effects								
Unemployment shock	0.0030*** (0.0009)	0.0005** (0.0003)	0.0008** (0.0004)	0.0019** (0.0009)	0.0019 (0.0013)	0.0008 (0.0021)	0.0048*** (0.0012)	0.0068*** (0.0015)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Panel D: controlling for respondent cohort \times year fixed effects								
Unemployment shock	0.0030*** (0.0010)	0.0009*** (0.0003)	0.0012*** (0.0004)	0.0035*** (0.0014)	0.0018 (0.0014)	0.0018 (0.0024)	0.0039*** (0.0015)	0.0058*** (0.0018)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206

Notes: All specifications are estimated using OLS, and include respondent-level industry \times year, occupation \times year, education \times year, and parish \times year fixed effects. The placebo tests in panels A and B are described in the main text. Panels C and D describe the additional fixed effects included. All observations are inversely weighted by the respondent’s number of second-degree weak ties in that year. Standard errors are double clustered by respondent municipality and second-degree weak tie municipality. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

transmitted through different types of weak ties 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 weak ties. Beyond our sample restrictions and fine-grained fixed effect structure, we address this concern using various robustness checks.

First, we conduct a placebo test examining whether unemployment shocks affect respondents that *do not* talk about unemployment and politics with their first-degree familial, vocational, or educational weak ties. By blocking this essential channel of information transmission, an unemployment shock to a second-degree weak tie should not affect these respondents’ beliefs, preferences, and behaviors—even when their first- and second-degree weak ties do speak about these issues between themselves. We test this using the survey responses documented in Figures 1a and 1b to restrict our sample to i - k dyads where i reports *never* speaking about either unemployment

or politics with the corresponding weak tie type—familial, vocational, or educational—of j that generates i 's second-degree weak tie with k .²⁰ The results in panel A add credence to our main results: we find no association between unemployment shocks and unemployment concerns, policy preferences, and political outcomes among respondents that do not discuss unemployment and politics with first-degree weak ties. This suggests that it is unlikely that common shocks, which should affect similar people that do not directly interact, are driving our findings.

Second, a further placebo test designed to detect common shocks afflicting respondents with similar types of network instead assigns respondents “fake” first-degree weak ties that are similar to a respondent’s actual weak ties. Specifically, each j was replaced by a randomly selected 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 weak tie of i . We then examine the effects of shocks affecting the k 's associated with each j' . Consistent with common shocks not driving our results, panel B reports no evidence that shocks influence respondent beliefs and preferences. The negative coefficients in columns (5), (6), and (7) are in the opposite direction to our main findings.

Third, common shocks could also arise if parish-level shocks in the location of second-degree weak ties are correlated with shocks independently affecting the respondent. Similarly, respondents could learn—through access to local media or through social networks—about general unemployment conditions in another area that are correlated with the experience of their specific second-degree weak tie’s experience. We address such concerns by including k -level industry \times year, occupation \times year, education \times year, and parish \times year fixed effects to control for the environment around each second-degree weak tie. We thus exploit only variation in unemployment shocks within the industries, occupations, educational categories, and parishes of second-degree weak ties within a given year. Panel C shows that the results are robust to including such fixed effects.

Fourth, shocks afflicting second-degree weak ties also 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 concern by

²⁰We assume that conversation between weak tie types persists over time, and thus assign 2015 conversation levels to all previous years in which the individual participates in the survey.

including (birth year) cohort \times year fixed effects, and thus exploit only variation in unemployment shocks to second-degree weak ties belonging to the same cohort in a given year. Panel D shows that the inclusion of such fixed effects does not alter our findings.

Appendix section [A.7.2](#) reports the results of eight additional checks more generally addressing potentially confounding factors or sensitivity to network construction. We show that our findings are robust to simultaneously controlling for the covariates in [Table 1](#) and a respondent's number of second-degree weak ties, further excluding second-degree weak ties from the same region as the respondent, controlling for indicators for respondents whose weak tie networks were truncated at the i and j levels by our vocational and educational restrictions, excluding respondents with more than 10,000 or 5,000 second-degree weak ties, and dropping all possible unemployment shocks to second-degree weak ties that rely on ties generated by familial or educational ties.

6 Information transmission mechanisms driving voter responses

We next illuminate the process through which unemployment shocks to second-degree weak ties influence voters. The following analyses indicate that information is transmitted through first-degree weak ties, who update similarly, and suggest that political responses are primarily driven by personal insurance, rather than sociotropic, motivations.

6.1 Information transmission through first-degree weak ties

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

We first fielded a nationally representative survey in 2018 to assess the extent of such interpersonal information transmission. When a weak tie becomes unemployed, [Figure 5](#) shows that most respondents at least occasionally relay this event to others within their weak tie network.

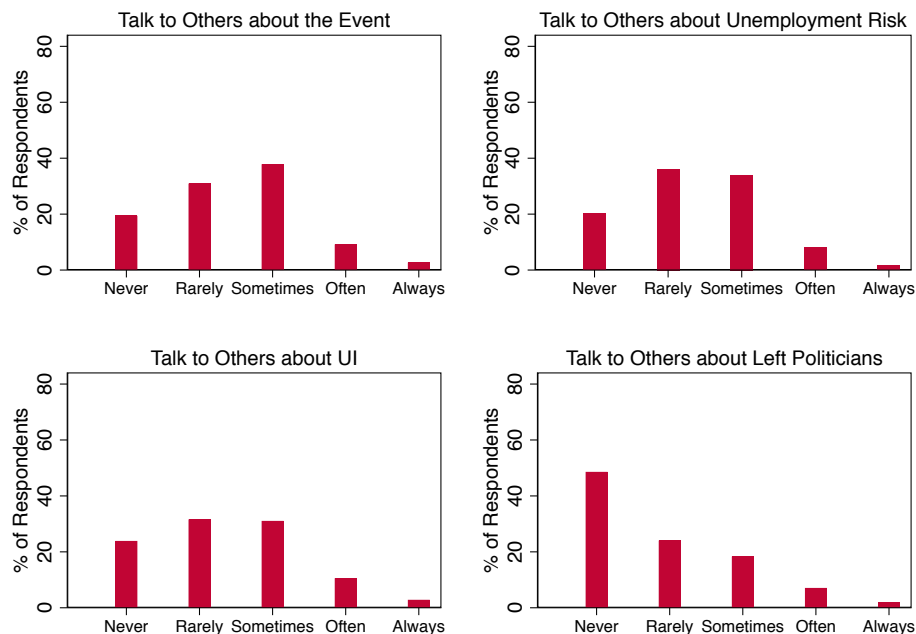


Figure 5: Discussions that individuals instigate when a first-degree tie becomes unemployed

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.”

Furthermore, respondents often instigate discussions of unemployment risk, unemployment insurance, and—to a lesser extent—the need for more left-wing politicians with others in response to a weak tie becoming unemployed. Conversely, only 9% of respondents report never instigating a discussion after a weak tie becomes unemployed. Voters thus clearly transmit politically-relevant information to their other weak ties when a weak tie becomes unemployed.

A second implication of an information transmission mechanism is that first-degree weak ties should also alter their beliefs and preferences following an unemployment shock in a similar manner to our respondents. Ideally, we would test this by estimating the effect of the same unemployment shocks to k on the intermediary j that links respondent i and their second-degree weak tie k . Unfortunately, very few of these intermediaries also participated in our panel survey. In the spirit of two-sample instrumental variable techniques (see Inoue and Solon 2010), 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 and j links and j and k links are independently sampled from the same popula-

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

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

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

tion, then we will obtain the same estimates in expectation (Inoue and Solon 2010). Table 1 shows that our respondents' first-degree weak ties are broadly similar to both our respondents and their second-degree weak ties, as required. We then approximate the first step in the direct transmission of information from k to j by estimating the following OLS regression:

$$Y_{iwoept} = \beta Unemployment\ shock_{jp''_t} + \gamma_{wt} + \delta_{ot} + \eta_{et} + \mu_{pt} + \varepsilon_{ijwoepp''_t}, \quad (2)$$

where $Unemployment\ shock_{jp''_t}$ is now an unemployment shock to j , a first-degree weak tie of i located in parish p'' . The fixed effect structure is analogous to equation (1), while we similarly remove first-degree weak ties located in the same municipality as a respondent.

The results reported in Table 4 add further credence to the information transmission mechanism through first-degree weak ties. Specifically, first-degree unemployment shocks influence respondents in the same direction as the second-degree shocks in Table 2, and most associations are also statistically significant. The main difference is with respect to magnitude: the effects of an unemployment shock to a first-degree weak tie on unemployment concerns, social policy preferences, and vote choices are approximately five times greater. In contrast, the effects of an unemployment shock to first- and second-degree weak ties on a respondent's national unemployment outlook are

similar in magnitude. This difference suggests that the differential in magnitude between the effects of first- and second-degree shocks cannot be entirely attributed to information decay arising from the greater probability that i learns about a shock to j than i learns about a shock to k through j .

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

6.2 Preferences and voting behavior are motivated by insurance interests

The greater effects of unemployment shocks afflicting first-degree weak 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 own prospects. We test the argument that unemployment shocks to similar people provide a stronger signal of an individual's prospects by estimating the following specifications:

$$Y_{iwoept} = \beta_1 \text{Unemployment shock}_{kp't} + \beta_2 \text{Same}_{ikt} + \beta_3 (\text{Unemployment shock}_{kp't} \times \text{Same}_{ikt}) + \gamma_{wt} + \delta_{ot} + \eta_{et} + \mu_{pt} + \varepsilon_{ikwoept}, \quad (3)$$

where Same_{ikt} is an indicator capturing i and k being either in the same two-digit industry or one-digit occupation in survey year t .²¹

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

²¹The analogous approach for first-degree weak ties yields broadly similar results. However, it is underpowered because, by definition, most first-degree weak ties are from the same group. For similar reasons, we do not present estimates for similar education grouping because 88% of second-degree weak ties share the same level of education as our respondents.

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

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

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

comparison of the first and third rows of panel A shows that the effects of unemployment shocks to second-degree weak ties within the same two-digit industry category on subjective unemployment expectations, support for more generous unemployment insurance, and having voted for left-wing parties are 3-4 times greater than for unemployment shocks to dissimilar second-degree weak ties. In contrast, shocks to dissimilar second-degree weak ties do not significantly influence political preferences. National unemployment perceptions again paint a stark contrast, as respondents update equally from all unemployment shocks to all types of second-degree weak ties. This lack of distinction reinforces the finding above that respondents update equally about aggregate employment from shocks to first- and second-degree weak ties becoming unemployed, and further suggests that greater sensitivity to shocks afflicting similar people does not simply reflect information filtering by j . Panel B reports similar—but less precise—results for similar occupations, although differential responses are no longer apparent for voting. Together, these results indicate that voters

distinguish the relevance of different types of information that diffuse through networks of weak ties, particularly with respect to industry of employment.

Given that policy and political responses are concentrated among unemployment shocks to similar individuals, these findings suggest that voter responses are primarily motivated by personal insurance interests. Put differently, only information transmitted through networks of weak ties that affect a respondent's own prospects induces a political reaction. In contrast with sociotropic accounts, Table 5 demonstrates that all types of unemployment shocks impact national unemployment perceptions, but only shocks to similar second-degree weak ties affect political preferences. Furthermore, Appendix Table A6 shows that respondents update their *personal* outlook less from unemployment shocks afflicting second-degree weak ties that frequently become unemployed. In addition to highlighting the primacy of voters' insurance interest, this additional result challenges the simple information decay story, which predicts that the interaction with frequency of unemployment shock should be constant across outcomes.²² In sum, these findings support an egotropic interpretation of voter preferences, whereby information transmitted between weak 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 heavily influenced by unemployment shocks afflicting second-degree weak ties. Furthermore, our evidence examining the mechanisms suggests that this information is transmitted through the individuals connecting respondents to second-degree weak ties, that voters update their beliefs in a logical fashion (upweighting similar types when considering their own unemployment prospects, but not doing this when forming aggregate unemployment projections), and base their policy preferences and voting behavior primarily on their subjective concerns.

It is difficult to see how social conformity could solely account for these findings. A strictly

²²Cases of frequent unemployment could be discussed more or less among weak ties. However, informational decay implies that the decay should constantly affect all outcomes.

social conformity explanation would require that an unemployment shock to a second-degree weak tie changes their behavior in a way that alters the social expectations governing the behavior of a first-degree weak tie in the presence of our respondent when the second-degree weak 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 a shock to k would change social norms in the networks that j and k share in ways that influence norms in the networks that i and j share. This is especially unlikely under our design because k and j and j and i live in different locations. Furthermore, the social conformity explanation struggles to explain why respondents react more to shocks afflicting second-degree weak ties in the same industry, given that knowledge of their similarity does not arise from direct interaction. Social conformity could drive this result if j reacts more to, or engages in more, pressure when they work in the same industry as i and k . However, Appendix Table A7 finds that respondents still differentially update their subjective concerns and political preferences from shocks afflicting similar k 's, even after controlling for the interaction between an unemployment shock and j and k sharing the same industry. Moreover, j and k sharing the same industry does not differentially affect our respondents' beliefs, preferences, and behavior. It is thus hard to explain how social conformity could differentially influence our respondents when j is in a different industry from i and k without j informing i that k was in the same industry. Our findings then principally reflect information transmission within social networks.

An important question largely beyond the scope of this study is how, and what type of, information is transmitted between weak 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 transmission. On the other, the exact nature of what is transferred is “black-boxed” beyond the general discussions described in Figure 5. Specifically, we cannot discern what second-degree weak ties communicate to a respondent's first-degree weak ties, how information is parsed by these intermediary connections, and what politically-relevant discussions arise between our respondents

and their first-degree weak ties as a consequence of the second-degree weak ties becoming unemployed. It is therefore not clear whether changes in second-degree weak 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.²³

7 Conclusion

We show that information diffusion across weak ties plays a key role in shaping economic and policy beliefs and preferences, and ultimately voting behavior. Combining Denmark's extraordinarily detailed individual-level data with an identification strategy exploiting unemployment shocks to second-degree weak ties at scale, we address the identification and network measurement challenges faced by previous studies investigating the impact of information diffusion within social networks. By focusing on shocks that must pass through intermediary ties, our approach helps to distinguish information transmission from social conformity pressures. Our findings show that voters are highly responsive to unemployment shocks afflicting second-degree weak ties, updating both beliefs about their personal unemployment risk and national unemployment levels. However, while perceptions of national aggregates respond to any person becoming unemployed, self assessments are only responsive to shocks afflicting those in the same industry. Consistent with an individual's personal insurance motivations, voters only alter their policy preferences and vote choices in response to concerns close to home. This induces them to ultimately support more generous unemployment insurance and vote for left-wing political parties.

Our study highlighting that the political importance of information diffusion between weak ties 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 about a major

²³Instrumenting for a peer's unemployment expectations with second-degree unemployment shocks (Bramoullé, Djebbari and Fortin 2009) is likely to violate the exclusion restriction.

global phenomenon (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 weak ties in other contexts, on a wider range of political behaviors, and in direct comparison with social pressure's influence.

Second, as we note in the discussion above, an important limitation demanding future research is the question of what types of information are transmitted between weak ties. 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 close and weak ties relies on understanding these processes in detail.

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A Online Appendix

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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.

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

²This 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 weak ties

To validate our definition of social networks and our claim that the first-degree weak ties identified in our data are sources of potentially important 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 first-degree weak ties in the data. We carefully clarified the meaning of conversation, fraction of weak tie groupings, 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 happening to 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 weak ties

The starting point to compute the set of second-degree weak ties in the Danish population is the symmetric $7,974,509 \times 7,974,509$ adjacency matrix that captures all possible first-degree connections between the weak ties 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 weak ties through the process described below. In the case of parents and partners, first-degree weak 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 weak ties. Matrix multiplying \mathbf{g} with itself produces a matrix containing the number of *second-degree* weak ties between each pair of individuals, except along the diagonal, which gives the network degree or the number of first-degree weak ties to other individuals. Finally, we define our second-degree matrix \mathbf{S} as a matrix of indicators for second but not first-degree weak ties between each pair of individuals, with the diagonal set to zero. In this example, there are 3 second-degree weak 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 weak 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 did not 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

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.”

³The number is in practice slightly lower because some connections are shared.

⁴See <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
-

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 unem-

ployment rate has been between 1.5 and 12%. What is your estimate of the current unemployment rate in Denmark? We would like your best estimate, even if you are not entirely sure.”

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

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

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

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

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

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

Voted for left party in 2011. An indicator coded 1 for respondents that reported voting for

a left party (Social Democrats, Social Liberals, Socialist People's, of Red-Green parties) in the previous election.

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

Woman. An indicator coded 1 for female respondents.

Age. The respondent's age in years.

Children. The number of children that a respondent has.

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

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

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

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

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

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

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

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

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

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

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

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

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

First-degree familial/vocational/educational weak tie. An indicator coded 1 for respondents who are connected to first-degree weak ties by a familial/vocational/educational tie.

Second-degree familial/vocational/educational weak tie. An indicator coded 1 for weak ties (of our respondent) who are connected to second-degree weak 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 Incumbent party vote intention

Table A2 examines the effect of unemployment shock on intention to vote for an incumbent party. Contrary to sociotropic accounts, the results show that unemployment shocks afflicting second-degree weak 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.7.2 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, we control for various potential *i*-level confounding variables. Indeed, panel A of Table A3 shows that our results are robust to simultaneously controlling for the variables in Table 1 in the main paper as well as the respondent's number of second-degree weak ties.

Second, to further allay concerns relating to geographically-dispersed common shocks, panel B excludes second-degree weak ties from within the same region—one of Denmark's five regions—as the respondent. Although this entails dropping around half the sample, and thus substantially reducing estimate precision, the point estimate magnitudes are robust.

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

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

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

Third, our main estimates exclude k 's located in the same parish as the j (or j 's) that link them to i . However, to further address the potential concern that j experiences shocks correlated with k , panel C 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.

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

Fifth, two concerns could arise from a reliance 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 weak ties generated by familial first-

Table A3: Additional robustness checks not reported in the main paper

	Own unemployment expectation	Guess national unemployment rate	National unemployment rate expectation	Want more unemployment insurance	Government should support the poor	Support non-market- based stimulus	Intend to vote for left party	Voted for left party in 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: controlling for covariates (variables in Table 1 and number of second-degree weak ties)								
Unemployment shock	0.0018* (0.0010)	0.0008*** (0.0002)	0.0011*** (0.0004)	0.0022 (0.0015)	0.0006 (0.0014)	0.0008 (0.0026)	0.0034** (0.0014)	0.0046** (0.0020)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Panel B: second-degree weak ties living in a different region from the respondent								
Unemployment shock	0.0047*** (0.0017)	0.0009* (0.0005)	0.0012* (0.0007)	0.0022 (0.0018)	0.0022 (0.0018)	-0.0020 (0.0024)	0.0033** (0.0016)	0.0047* (0.0028)
Observations	101,102,172	75,781,837	50,014,975	101,102,172	101,102,172	25,320,335	101,102,172	30,061,802
Panel C: removing second-degree weak ties living in the same municipality as the first-degree weak tie linking them to the respondent								
Unemployment shock	0.0030*** (0.0010)	0.0009*** (0.0003)	0.0011*** (0.0004)	0.0025 (0.0016)	0.0015 (0.0014)	0.0012 (0.0027)	0.0036** (0.0017)	0.0053** (0.0026)
Observations	118,526,080	88,686,037	58,393,777	118,526,080	118,526,080	29,840,043	118,526,080	23,584,797
Panel D: controlling for weak tie network construction truncation								
Unemployment shock	0.0044*** (0.0010)	0.0010*** (0.0003)	0.0012*** (0.0004)	0.0031** (0.0014)	0.0015 (0.0014)	0.0014 (0.0025)	0.0038** (0.0016)	0.0054** (0.0023)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	41,432,206
Panel E: removing second-degree weak ties connected to the respondent by a familial first-degree weak tie								
Unemployment shock	0.0038*** (0.0011)	0.0009*** (0.0003)	0.0012*** (0.0004)	0.0028* (0.0016)	0.0012 (0.0016)	0.0020 (0.0027)	0.0036** (0.0016)	0.0039 (0.0026)
Observations	136,543,459	102,160,667	67,286,388	136,543,459	136,543,459	34,382,792	136,543,459	27,188,066
Panel F: removing second-degree weak ties connected to the respondent by an educational second- or first-degree weak tie								
Unemployment shock	0.0061** (0.0025)	0.0010 (0.0008)	0.0007 (0.0005)	0.0086*** (0.0027)	0.0083*** (0.0030)	-0.0069 (0.0081)	0.0049 (0.0039)	0.0052 (0.0066)
Observations	55,866,735	42,186,677	27,825,635	55,866,735	55,866,735	13,680,058	55,866,735	11,349,774
Panel G: restrict sample to respondents with at most 10,000 second degree weak ties								
Unemployment shock	0.0027** (0.0011)	0.0010*** (0.0003)	0.0013*** (0.0005)	0.0028* (0.0016)	0.0016 (0.0018)	-0.0008 (0.0031)	0.0042** (0.0018)	0.0047* (0.0024)
Observations	52,638,472	38,790,852	25,757,794	52,638,472	52,638,472	13,847,620	52,638,472	14,966,178
Panel H: restrict sample to respondents with at most 5,000 second degree weak ties								
Unemployment shock	0.0027** (0.0014)	0.0011*** (0.0004)	0.0015** (0.0006)	0.0021 (0.0019)	0.0018 (0.0022)	0.0006 (0.0038)	0.0058*** (0.0022)	0.0066** (0.0030)
Observations	21,115,818	15,550,408	10,403,150	21,115,818	21,115,818	5,565,410	21,115,818	6,169,746

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

degree weak ties. Panel E shows that our results are not substantively affected by dropping such weak ties.

Sixth, 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 weak 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 results in panel F report generally larger—but, unsurprisingly, noisier—point—estimates.

Finally, we show that our results are robust to focusing only on respondents with relatively small second-degree networks of weak ties—less than 10,000 or 5,000 second-degree weak ties satisfying the restrictions defining our identification strategy above. After the latter restriction, we are only left with 5,325 unique i 's and 10,155 unique i -year observations. Although the drop in sample size reduces the power of these estimates, panels G and H 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 weak ties.

A.7.3 Heterogeneity by type of weak tie

Tables A4 and A5 respectively show interactions between unemployment shocks and the type of links between respondent and first-degree weak tie and between first-degree and second-degree weak 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, although familial ties are perhaps most important when considering the i - j link.

A.7.4 Heterogeneity by frequency of second-degree weak tie unemployment

Table A6 shows the interaction between unemployment shocks and the number of times that the second-degree weak tie has become unemployed in the past ten years. The results suggest that, at least for an individual's own unemployment concerns and their support for unemployment insurance, that shocks to individuals likely to become unemployed have weaker effects on respondents. There is no evidence of differential effects on aggregate unemployment projections. This again suggests information decay does not account for these findings, and sociotropic voting does not account for political preferences. Rather, the results suggest that voters are motivated by their personal insurance interest.

Table A4: Heterogeneity of second-degree weak tie unemployment shocks on respondent economic and political perceptions and preferences, by type of respondent-first-degree weak tie relationship

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

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

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

	Own unemployment expectation (1)	Guess national unemployment rate (2)	National unemployment rate expectation (3)	Want more unemployment insurance (4)	Government should support the poor (5)	Support non-market-based stimulus (6)	Intend to vote for left party (7)	Voted for left party in 2011 (8)
Unemployment shock	-0.0066 (0.0041)	-0.0001 (0.0007)	0.0016 (0.0014)	-0.0245*** (0.0056)	-0.0110* (0.0063)	-0.0116 (0.0121)	-0.0200** (0.0080)	-0.0043 (0.0109)
Second-degree familial weak tie	-0.0235*** (0.0041)	-0.0005 (0.0009)	0.0006 (0.0009)	-0.0312*** (0.0070)	-0.0226** (0.0091)	0.0080 (0.0132)	-0.0303*** (0.0095)	-0.0208 (0.0156)
Second-degree vocational weak tie	-0.0261*** (0.0034)	-0.0008 (0.0008)	-0.0003 (0.0007)	-0.0333*** (0.0060)	-0.0208*** (0.0073)	0.0025 (0.0114)	-0.0269*** (0.0086)	-0.0167 (0.0124)
Second-degree educational weak tie	-0.0314*** (0.0036)	-0.0015* (0.0008)	-0.0003 (0.0007)	-0.0334*** (0.0070)	-0.0208*** (0.0079)	-0.0030 (0.0112)	-0.0290*** (0.0084)	-0.0276** (0.0129)
Unemployment shock × Second-degree familial weak tie	0.0055 (0.0050)	0.0005 (0.0010)	-0.0018 (0.0017)	0.0344*** (0.0064)	0.0098 (0.0077)	0.0005 (0.0167)	0.0297*** (0.0076)	0.0267 (0.0182)
Unemployment shock × Second-degree vocational weak tie	0.0102** (0.0042)	0.0011 (0.0008)	-0.0006 (0.0016)	0.0272*** (0.0053)	0.0135** (0.0060)	0.0157 (0.0119)	0.0207** (0.0083)	0.0046 (0.0107)
Unemployment shock × Second-degree educational weak tie	0.0108*** (0.0037)	0.0011* (0.0007)	0.0000 (0.0008)	0.0262*** (0.0058)	0.0106 (0.0070)	0.0115 (0.0125)	0.0266*** (0.0067)	0.0150 (0.0106)
Observations	140,509,875	105,142,551	69,271,133	140,509,875	140,509,875	35,367,324	140,509,875	27,960,832
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.1380	0.0849	0.0759	0.3146	0.3893	0.4204	0.4799	0.5444
Outcome standard deviation	0.2638	0.0543	0.0470	0.4644	0.4876	0.4936	0.4996	0.4980
Unemployment shock mean	0.0185	0.0182	0.0179	0.0185	0.0185	0.0194	0.0185	0.0180
Second-degree familial weak tie mean	0.0282	0.0284	0.0287	0.0282	0.0282	0.0278	0.0282	0.0276
Second-degree vocational weak tie mean	0.6736	0.6719	0.6729	0.6736	0.6736	0.6788	0.6736	0.6842
Second-degree educational weak tie mean	0.3499	0.3525	0.3524	0.3499	0.3499	0.3420	0.3499	0.3405
F-test: shock × familial = Shock × vocational (<i>p</i> -value)	0.2765	0.2151	0.1020	0.0721	0.4668	0.3800	0.0937	0.2003
F-test: shock × familial = Shock × educational (<i>p</i> -value)	0.2293	0.5074	0.2634	0.0514	0.8953	0.4899	0.5128	0.4045
F-test: shock × educational = Shock × vocational (<i>p</i> -value)	0.8681	0.9643	0.7048	0.7606	0.4166	0.4778	0.1489	0.1251
Survey years unavailable		2010	2010, 2012			2011, 2012, 2013		2010, 2011

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

Table A6: Heterogeneity of second-degree weak tie unemployment shocks on respondent economic and political perceptions and preferences, by frequency of unemployment shocks to second-degree weak ties

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

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

A.7.5 Social conformity interpretation check

Table A7 shows the interaction between unemployment shocks and similarity of respondent and second-degree weak tie type, controlling for similarity of first-degree weak tie type and second-degree weak tie type. This tests whether our main similarity finding between i and k remains robust in cases where j and k are also in the same industry. This is an informative check because it is unlikely that a purely social conformity explanation could drive our heterogeneous effects by same industry, given that i and k do not know each other (and presumably that i does not know that k is in the same industry without being informed of this by j), unless j also being in the same industry as i and k enhances the effects of conformity. The results show that the interaction estimates for i - k similarity are similar to those in Table 5, and thus support the information transmission interpretation. Although the interactions in columns (4) and (8) are no longer statistically significant, they remain relatively large and positive. Moreover, the interaction between j and k never significantly increases the effect of an unemployment shock, further suggesting that social conformity driven by j differentially changing their behavior when a k in their industry becomes unemployed is unlikely to explain our findings.

Table A7: Heterogeneity of second-degree weak tie unemployment shocks on respondent economic and political perceptions and preferences, by similarity of respondent and second-degree weak tie, controlling for similarity of first-degree weak tie and second-degree weak tie

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

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