A   Online Appendix

Contents

A.1 Unemployment insurance in Denmark

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

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

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

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

A.2   Accessing the Danish administrative data

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

A.3   Details of survey validating first-degree social ties

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

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

3

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

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

A.4 Computation of second-degree social ties

The starting point to compute the set of second-degree social ties in the Danish population is the symmetric $7,974,509 \times 7,974,509$ adjacency matrix that captures all possible first-degree ties, as defined in the main paper. Each entry is either 0 or 1, indicating a tie between i and j. In practice this was computed by first associating i with a parent or partner, work institution (from within 2 years and satisfying our other restrictions), and educational institution, and then generating second-degree ties through the process described below. In the case of parents and partners, first-degree ties were also retained.

To illustrate our computation, consider a 5×5 adjacency matrix $\mathbf{g}$ relating 5 individuals to each other. In our example, persons 1 and 2, 1 and 4, 2 and 3, and 3 and 5 are all first-degree social ties. Matrix multiplying $\mathbf{g}$ with itself produces a matrix containing the number of *second-degree* ties between each pair of individuals, except along the diagonal, which gives the network degree or the number of first-degree ties to other individuals. Finally, we define our second-degree matrix $\mathbf{S}$ as a matrix of indicators for second but not first-degree ties between each pair of individuals, with the diagonal set to zero. In this example, there are 3 second-degree ties, between individuals 1 and 3, 2 and 5, and 2 and 4. In the Danish population, the number is far higher because the average

individual has approximately 150^2 second-degree ties.[3]

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

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

A.5 Industry, occupation, and education digits

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

A.6 Variable definitions

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

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

[4]See http://www.dst.dk/en/Statistik/Publikationer/VisPub?cid=22256.

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

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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

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."

Own unemployment expectation. The probability, as a fraction, assigned by the respondent to the possibility that they will experience a period of unemployment in the forthcoming year. Respondents were asked the following question: "What is your assessment of the probability that you will experience a period without a job during the year of [current year]? I would like you to provide a number between 0 and 100, where 0 means that you think that the event certainly does not occur and 100 means that you think that the event certainly occurs."

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

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

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

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

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

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

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

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

Woman. An indicator coded 1 for respondents that identify as women.

Age. The respondent's age in years.

Children. The number of children that a respondent has.

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

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

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

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

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

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

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

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

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

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

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

our restrictions).

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

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

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

A.7 Additional results

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

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

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

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

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

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment expectation (3)	Want more unemployment insurance (4)	Government should support the poor (5)	Support non-market-based stimulus (6)	Intend to vote for left party (7)	Voted for left party in 2011 (8)
Second-degree unemployment shock share	0.217*** (0.054)	0.224*** (0.057)	0.943*** (0.200)	0.737** (0.327)	0.472 (0.301)	0.320 (0.394)	0.790*** (0.291)	1.396* (0.802)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	3,243
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.085	0.076	0.174	0.320	0.387	0.402	0.439	0.507
Outcome std. dev.	0.055	0.050	0.302	0.467	0.487	0.490	0.496	0.500
Unemployment shocks mean	0.021	0.021	0.022	0.022	0.022	0.024	0.022	0.021
Unemployment shocks std. dev.	0.014	0.015	0.016	0.016	0.016	0.018	0.016	0.014
Survey years unavailable	2010	2010, 2012				2011, 2012, 2013		2010, 2011

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

A.7.2 Including a lagged dependent variable

As noted in footnote 20 of the main text, our preferred identification strategy principally exploits cross-sectional variation by treating surveys as repeated cross-sections. We cannot exploit purely within-respondent variation because, at least over the four-year post-financial crisis period that we study, there is limited variation in the share of their second-degree ties that became unemployed. Indeed, a regression of our main independent variable on respondent fixed effects, as well as our baseline fixed effects, yields an R^2 of 0.82 (relative to an R^2 of 0.48 if we just include our baseline fixed effects). Intuitively, this lack of variation arises because second-degree networks comprise thousands of individuals and thus, by the law of large numbers, the share of individuals that become unemployed in a given year quickly converges to a similar expected value if the underlying unemployment risks of the network remain similar across years—which is particularly likely in the short term when fixed effects are including to extract year-specific effects. Consequently, while a design including respondent fixed effects is appealing in terms of ensuring that time-invariant unobserved differences across respondents are not driving the results, a sufficiently long time series covering a period over which underlying risks vary across second-degree networks is required in order to ensure that within-respondent variation in the main independent variable is not simply noise. A longitudinal design would also reduce our sample by around half and prevent us from

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

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

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

analyzing some outcome variables, given that many respondents only complete a single wave of the survey.

While including respondent-level fixed effects would thus defeat the purpose of our design, we follow Margalit (2013) in showing robustness to the interim position of including a lagged dependent variable. Unlike unit fixed effects or first-differencing, including a lagged dependent variable does not restrict the coefficient on the lag to unity. Although this cannot guarantee the removal of time-invariant respondent effects, it also maintains relevant cross-sectional variation in our independent variable—to the extent that the coefficient differs from 1.

The results of this robustness check are reported in Table A3 for the outcomes for which a lagged dependent variable is available. The drop in sample size varies by outcome, but generally halves relative to the main estimate in Table 2. Standard errors almost double in magnitude for most outcomes. For the outcomes in columns (1)-(5), the point estimates remain similar and statistically significant, while the coefficient on the lagged dependent variable is small. For vote intention in column (6), the coefficient on the lag is much larger, leaving mostly within-respondent variation in the outcome left to explain. Consistent with the remaining variation in the outcome largely

reflecting noise around the expected share of second-degree ties that became unemployed in a given year, our estimated effect of unemployment shocks on voting intention goes to zero and ceases to be statistically significant.

A.7.3 How unemployment shocks relate to respondent unemployment prospects

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

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

To assess this claim empirically, we examine the relationship between second-degree unemployment shocks and a respondent's subsequent unemployment experiences. The estimates in Table A4 examines how this relationship varies as additional fixed effects are progressively included. Suggesting that the design indeed isolates exogenous variation in second-degree unemployment shocks, columns (5) and (10) show that there is no significant relationship between such unemployment shocks and an individual's own unemployment outcomes. However, the preceding estimates demonstrate that a strong positive relationship holds unconditionally and is slowly weakened by

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

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

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

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

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

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

the inclusion of our fixed effects. Together, these findings suggest that voters' responses to unemployment shock signals are relatively accurate to the point of fully adjusting for all the fixed effects included in our statistical model. This is hardly surprising since the information that we possess as researchers about industry, education, occupation, and parish likely exceeds the information that diffuses between individuals in the population.

A.7.4 Incumbent party vote intention

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

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

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment expectation (3)	Want more unemployment insurance (4)	Government should support the poor (5)	Support non-market-based stimulus (6)	Intend to vote for left party (7)	Voted for left party in 2011 (8)
Second-degree unemployment	0.199***	0.246***	0.380*	0.477	0.138	0.123	0.718**	1.155**
shocks share	(0.053)	(0.057)	(0.209)	(0.359)	(0.333)	(0.438)	(0.287)	(0.564)
Female	0.016***	0.011***	0.025***	0.046***	0.028**	-0.084***	0.014	0.018
	(0.001)	(0.001)	(0.007)	(0.012)	(0.012)	(0.022)	(0.014)	(0.019)
Age	-0.000***	-0.000**	-0.001*	0.004***	0.002***	0.002**	0.002***	0.003***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Single	-0.001	-0.000	0.023***	0.036***	-0.001	-0.013	-0.004	0.004
	(0.001)	(0.002)	(0.008)	(0.013)	(0.012)	(0.014)	(0.014)	(0.017)
Number of children	-0.000	0.001	-0.008***	0.005	0.002	-0.006	0.003	-0.002
	(0.001)	(0.001)	(0.002)	(0.005)	(0.005)	(0.008)	(0.006)	(0.008)
Income (DKK 1,000,000)	-0.001	-0.001	-0.054***	-0.021	-0.025**	0.032	-0.039*	-0.117***
	(0.001)	(0.002)	(0.015)	(0.018)	(0.011)	(0.023)	(0.022)	(0.031)
Total Assets (DKK 1,000,000)	-0.000	-0.000	0.006***	0.002	-0.001	-0.004	0.003	-0.010**
	(0.000)	(0.000)	(0.001)	(0.003)	(0.003)	(0.005)	(0.004)	(0.005)
Total Debt (DKK 1,000,000)	0.000	-0.000	-0.007***	-0.010***	-0.006	-0.007	-0.014***	-0.007
	(0.000)	(0.000)	(0.002)	(0.003)	(0.004)	(0.006)	(0.004)	(0.005)
Homeowner	-0.006***	-0.003	-0.031***	-0.049***	-0.040***	0.016	-0.052***	-0.026
	(0.002)	(0.002)	(0.007)	(0.012)	(0.012)	(0.022)	(0.015)	(0.017)
Whether unemployed	0.012***	0.007	0.310***	0.142***	0.094***	0.126***	0.027	0.062
	(0.004)	(0.004)	(0.018)	(0.028)	(0.029)	(0.047)	(0.025)	(0.044)
Unemployment shock	0.006*	0.010**	0.475***	0.127***	0.095***	0.068	0.079***	0.049
	(0.003)	(0.005)	(0.017)	(0.025)	(0.027)	(0.054)	(0.021)	(0.044)
Number of second-degree	-0.000	0.000	-0.000***	-0.000**	-0.000***	0.000	-0.000	-0.000
social ties	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	5,209
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.085	0.076	0.174	0.320	0.387	0.402	0.438	0.499
Outcome standard deviation	0.055	0.050	0.302	0.467	0.487	0.490	0.496	0.500
Unemployment shocks mean	0.022	0.022	0.023	0.023	0.023	0.025	0.023	0.022
Unemployment shocks std. dev.	0.014	0.014	0.015	0.015	0.015	0.018	0.015	0.014
Survey years unavailable	2010	2010, 2012				2011, 2012, 2013		2010, 2011

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

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

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

A.7.6 Additional robustness checks reported in the main paper

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

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

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

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

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

Fifth, we also show that the results are robust to removing all k's generated by educational ties, which Figure 2 shows to be the network tie category that produces the most ties that do not

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

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

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

somewhat regularly converse on average. This entails removing any k that is only a second-degree tie to i because either the i-j or j-k dyad is based on an educational tie. To avoid introducing biases from incomplete networks, we do not reconstruct networks without considering educational ties. The smaller point estimates in panel E suggest that educational ties are partly driving the effects that we observe.

Finally, we show that our results are robust to focusing only on respondents with relatively small

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

A.7.7 Tests for nonlinearity of effects

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

A.7.8 Heterogeneity by type of social tie

Tables A9 and A10 respectively report interactions between the share of second-degree social tie unemployment shocks and the share of type of ties between respondent and first-degree tie and between first-degree and second-degree ties. Note that some ties can reflect multiple types. The results, and the F tests at the foot of the table, suggest that different types of tie produce relatively similar effects.

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

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

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

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

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment expectation (3)	Want more unemployment insurance (4)	Government should support the poor (5)	Support non-market-based stimulus (6)	Intend to vote for left party (7)	Voted for left party in 2011 (8)
Second-degree unemployment shock share	-1.519 (1.191)	-0.648 (1.172)	2.538 (4.533)	-15.009** (7.469)	-2.476 (7.875)	3.868 (7.309)	-8.057 (8.450)	-29.954* (17.370)
First-degree familial tie share	-0.020 (0.017)	-0.010 (0.015)	-0.144** (0.066)	-0.519*** (0.139)	-0.121 (0.138)	0.186 (0.191)	-0.290* (0.168)	-0.644** (0.286)
First-degree vocational tie share	-0.023 (0.016)	-0.011 (0.014)	-0.231*** (0.063)	-0.550*** (0.133)	-0.174 (0.128)	0.142 (0.178)	-0.264* (0.158)	-0.501* (0.272)
First-degree educational tie share	-0.022 (0.017)	-0.011 (0.014)	-0.204*** (0.062)	-0.540*** (0.135)	-0.125 (0.134)	0.082 (0.167)	-0.250 (0.169)	-0.526* (0.276)
Second-degree unemployment shock share × First-degree familial tie share	1.652 (1.194)	0.831 (1.169)	-1.652 (4.481)	14.941** (7.426)	2.573 (7.994)	-4.620 (7.449)	8.805 (8.427)	32.403* (17.290)
Second-degree unemployment shock share × First-degree vocational tie share	1.787 (1.183)	0.977 (1.177)	-2.170 (4.557)	16.574*** (7.338)	3.833 (7.782)	-2.960 (7.170)	8.871 (8.178)	28.791* (16.857)
Second-degree unemployment shock share × First-degree educational tie share	2.077* (1.174)	1.124 (1.150)	-0.888 (4.438)	16.025** (7.259)	2.275 (7.766)	-1.500 (7.210)	8.322 (8.513)	31.258* (16.829)
Outcome mean	0.085	0.076	0.174	0.320	0.387	0.402	0.439	0.507
Outcome std. dev.	0.055	0.050	0.302	0.467	0.487	0.490	0.496	0.500
Unemployment shocks mean	0.022	0.022	0.023	0.023	0.023	0.025	0.023	0.022
Unemployment shocks std. dev.	0.014	0.014	0.015	0.015	0.015	0.018	0.015	0.013
First-degree familial tie share mean	0.182	0.187	0.186	0.186	0.186	0.197	0.186	0.171
First-degree educational tie share mean	0.403	0.404	0.403	0.403	0.403	0.404	0.403	0.413
First-degree vocational tie share mean	0.445	0.439	0.441	0.441	0.441	0.428	0.441	0.448
F-test: Shock × familial = Shock × educational (p-value)	0.007	0.178	0.274	0.302	0.824	0.092	0.602	0.631
F-test: Shock × familial = Shock × vocational (p-value)	0.304	0.524	0.482	0.058	0.138	0.202	0.941	0.115
F-test: Shock × educational = Shock × vocational (p-value)	0.057	0.438	0.117	0.677	0.250	0.416	0.639	0.280
Survey years unavailable	2010	2010, 2012				2011, 2012, 2013		2010, 2011

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

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

	Guess national unemployment rate (1)	National unemployment rate expectation (2)	Own unemployment expectation (3)	Want more unemployment insurance (4)	Government should support the poor (5)	Support non-market-based stimulus (6)	Intend to vote for left party (7)	Voted for left party in 2011 (8)
Second-degree unemployment shock share	-5.378* (2.746)	-7.160** (3.472)	40.020*** (12.513)	-19.842 (17.860)	22.578 (19.896)	-54.335* (31.435)	-7.729 (24.959)	13.674 (55.771)
Second-degree familial tie share	-0.049 (0.030)	-0.043 (0.037)	0.182 (0.131)	-0.703*** (0.189)	-0.103 (0.233)	-0.347 (0.359)	-0.626** (0.290)	-0.480 (0.716)
Second-degree vocational tie share	-0.070** (0.027)	-0.069** (0.034)	-0.054 (0.123)	-0.780*** (0.190)	-0.130 (0.211)	-0.534 (0.345)	-0.501* (0.285)	-0.203 (0.619)
Second-degree educational tie share	-0.072** (0.027)	-0.073** (0.034)	-0.060 (0.128)	-0.737*** (0.194)	-0.104 (0.218)	-0.544 (0.339)	-0.518* (0.280)	-0.230 (0.641)
Second-degree unemployment shock share × Second-degree familial tie share	5.321* (2.753)	7.217** (3.515)	-42.184*** (12.519)	20.866 (17.966)	-24.302 (19.710)	51.939 (31.674)	10.240 (24.672)	-5.751 (56.240)
Second-degree unemployment shock share × Second-degree educational tie share	5.510** (2.730)	7.394** (3.445)	-39.257*** (12.582)	19.457 (17.902)	-22.423 (19.859)	53.933* (31.347)	8.539 (24.891)	-13.452 (55.699)
Second-degree unemployment shock share × Second-degree vocational tie share	5.788** (2.734)	7.456** (3.455)	-38.729*** (12.370)	21.124 (17.705)	-21.889 (19.783)	55.361* (31.440)	8.034 (24.874)	-11.628 (55.573)
Observations	13,000	8,667	17,816	17,816	17,816	4,816	17,816	3,243
Outcome range	[0,1]	[0,1]	[0,1]	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.085	0.076	0.174	0.320	0.387	0.402	0.439	0.507
Outcome std. dev.	0.055	0.050	0.302	0.467	0.487	0.490	0.496	0.500
Unemployment shocks mean	0.022	0.022	0.023	0.023	0.023	0.025	0.023	0.022
Unemployment shocks std. dev.	0.014	0.014	0.015	0.015	0.015	0.018	0.015	0.013
Second-degree familial tie share mean	0.035	0.035	0.035	0.035	0.035	0.036	0.035	0.034
Second-degree educational tie share mean	0.333	0.334	0.332	0.332	0.332	0.328	0.332	0.323
Second-degree vocational tie share mean	0.654	0.653	0.654	0.654	0.654	0.654	0.654	0.664
F-test: Shock × familial = Shock × educational (p-value)	0.509	0.772	0.043	0.311	0.353	0.683	0.425	0.102
F-test: Shock × familial = Shock × vocational (p-value)	0.074	0.637	0.007	0.861	0.217	0.492	0.373	0.102
F-test: Shock × educational = Shock × vocational (p-value)	0.124	0.817	0.571	0.031	0.630	0.245	0.610	0.442
Survey years unavailable	2010	2010, 2012				2011, 2012, 2013		2010, 2011

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