

Unemployment Expectations, Information, and Voting: Experimental and Administrative Micro-Evidence

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November 2, 2013

DRAFT: PLEASE DO NOT QUOTE

ABSTRACT

Voters differ in their assessment of the economy, both retrospectively and prospectively. In this paper, we examine whether voters' subjective forecasts of the economy and their assessments of their individual unemployment risk affect how they vote, or if their forecasts reflect, rather than cause, their partisan leanings. We employ a unique Danish dataset comprising panel surveys, a survey experiment, and detailed administrative registry data on individuals. We demonstrate considerable heterogeneity in individuals' forecasts of the aggregate unemployment rate and show how these forecasts are formed by voter characteristics and voters' own expectations about the risk of unemployment in combination with local and industry economic conditions. We employ a randomly allocated survey treatment that influences unemployment forecasts in an instrumental variable approach. Based on this, we reject endogeneity of economic forecasts and show, in a least squares setting, that sociotropic economic forecasts have a causal effect on voting behavior. Finally, our results are consistent with individual unemployment risk affecting both subjective forecasts of the aggregate economy and voting. However, the reverse is not true: controlling for fine-grained data on individual economic circumstances, we do not find significant effects from the aggregate economy and partisan identity on perceptions of individual unemployment risk.

Prepared for presentation at the Political Economy Conference, National Bureau of Economic Research, Cambridge, MA, November 15, 2013. The authors thank the Danish Social Science Research Council and the Sapere Aude program under the Danish Council for Independent Research for financial support, Amalie Sofie Jensen and Gregers Nytoft Rasmussen for research assistance, and Michael Becher, Pablo Beramendi, Andy Healy, John Marshall, Erik Snowberg, and Randy Stevenson for comments. Earlier versions were presented at annual meetings of the European Political Science Association, Barcelona, June 2013 and the American Political Science Association, Chicago, August 2013.

Does Jones's opinion, Jones being a weaver in a textile mill, come from the attitude of his boss, the competition of new immigrants, his wife's grocery bills, or the ever present contract with the firm which is selling him a Ford car and a house and lot on the installment plan? Without a special inquiry you cannot tell.
--Walter Lippmann, quoted by Kiewiet and Lewis-Beck (2013)

Voters differ in their assessment of the economy, both retrospectively and prospectively. In this paper, we examine whether voters' subjective forecasts of both the national and their personal economy affect how they vote, or if their forecasts are a reflection, rather than a cause, of their partisan leanings.

Voting based on economic outcomes, by convention "economic voting", allows voters to hold elected governments accountable even without detailed knowledge about policies or policymakers, if voters observe the economy and evaluate it, and use the evaluations to decide whether to support incumbents. However, if economic evaluations are "endogenous", i.e. reflect partisan or ideological concerns, whether in the observation, sources, nature, or interpretation of economic conditions, then the relationship between economy and evaluations, let alone electoral outcomes, is not causal and the real economy does not affect vote choice in the way models of retrospective voting posit. Models that fail to account for this endogeneity, as is well known, will produce biased estimates.

To disentangle the causal pathways between individuals' expectations about aggregate unemployment, individual perceptions of own unemployment risk, and voting patterns, we employ a unique Danish dataset comprising panel surveys, a random survey experiment, and administrative registry data on individuals. This allows us to combine – at the individual level – information about beliefs, political preferences, vote choice, and subjective expectations with results from a random survey experiment and detailed and exact administrative data on respondents' personal histories of education, occupation and unemployment as well as contextual

measures of unemployment in occupational and geographical labor markets, constructed by us from administrative data.

In the survey data we examine how individuals form beliefs and expectations about broader economic conditions (the “sociotropic” component of economic voting) in order to make judgments about the competence of politicians, and ultimately to decide how to vote. The beliefs are based on both objective measures of local and workplace economic conditions and subjective impressions about their personal risk of unemployment.

An important feature of the analysis is our ability to exploit third-party reported administrative register micro-data at the individual level, allowing for a very fine-grained level of analysis, reflecting both small-area demography and detailed occupational unemployment rates as information sources as well as individual unemployment histories. For all our Danish Joneses, we know that they weave and where they do it, how many unemployed are in their neighborhood and if they are of foreign origin, their housing wealth and whether they have a mortgage or other consumer debt, what their incomes and employment histories are for several years past, for themselves and for their spouses. We merge this with the survey data on economic expectations and political choices, along with respondents’ self-assessed employment prospects, a condition correlated with dependence on different sources of information.

That these detailed data are third-party reported, from employers, tax authorities and Statistics Denmark, and at the same time exist for the entire population, provides us with two distinct advantages. First, since survey respondents’ reports of both factual beliefs and individual circumstances may be contaminated by biases (Healy and Malhotra 2013), including social desirability or expressive partisan responses (Bullock et al. 2013), measurement error can be non-classical (Kreiner et al. 2013) with real consequences for analyses of political attitudes and

behavior (Hariri and Lassen, 2013); utilizing data without such potential (and often unknown) errors allows us to essentially bypass concerns of survey bias for key variables in the relationship between individual economic circumstances, perceptions of the aggregate economy, and the vote. Second, we construct geographic and occupational averages of unemployment from individual-level data covering the entire population, mitigating concerns about classical measurement error in independent variables and potential attenuation bias.¹

Our key contribution is to use the random survey treatments to generate exogenous variation in aggregate unemployment perceptions and to use this variation in an instrumental variables framework to make statements about causal effects of such perceptions on voting. We first show that individuals' forecasts of the aggregate unemployment rate are shaped by voter characteristics and voters' own histories of unemployment, expectations about the personal risk of unemployment, and relative levels of unemployment in the area where they live and in the occupation in which they work. We then randomly allocate survey treatments that significantly influence unemployment forecasts: these treatments include telling people the unemployment rate and giving them a prediction about fiscal consolidation. Using an instrumental variables approach, we reject the endogeneity of economic forecasts with respect to party and show, in a least squares setting, that aggregate unemployment perceptions have a causal effect on vote choice. Finally, we also show that individual unemployment risk affects both subjective forecasts of the aggregate economy and voting, controlling for fine-grained data on individual economic circumstances, but that there are no reverse effects from forecasts of the aggregate economy and partisan identity on perceptions of individual unemployment risk.

¹ See Healy and Lenz (2013) for a recent demonstration in a political economy context.

The next part of the paper reviews the political context, recent developments in Denmark, and the theory and empirical models of economic voting. Part II describes our empirical specification, paying close attention to our identification strategy, and the data, including survey questions, experimental treatments, and the registry materials. Part III provides the estimation results for aggregate unemployment expectations and voting intention in January 2011 and the subsequent actual vote in September 2011, as well as individual unemployment expectations. Part IV concludes.

I. Context: Politics and Economic Voting

I.A Political context

Since WWII, Denmark has had alternating centre-right and centre-left governments. In 2001, a centre-right government led by the Liberal Party, under Prime Minister Anders Fogh Rasmussen, and also including the Conservatives, took over after eight years of Social-Democratic led coalition governments. The centre-right government was reelected in 2004 and again in September 2007. In 2008, the PM was appointed general secretary of NATO and was succeeded, without elections, by the new head of the Liberal Party, Lars Løkke Rasmussen.

The financial crisis hit Denmark in late 2008, with unemployment increasing from 1.5 %, its lowest point, in October 2008 to a stable level between 4-4.5 % throughout 2011. Going into the crisis, the Danish public sector had a considerable primary surplus and one of the lowest debt levels of the OECD, but effects of discretionary policy measures were limited by the fact the Denmark is a small open economy highly dependent on the greater European economy. The

government held the scheduled election in September 2011 and lost to a (pre-election) coalition led by the Social Democrats, and including Social Liberals and The Socialist People's Party.²

I.B Theoretical context: economic information and retrospective voting

The accumulated corpus of research on "economic" or "retrospective" evaluation and voting claims that: citizens observe and evaluate what has happened in deciding about whom to support, and in particular whether to reward or punish incumbents (Key 1966). Fiorina (1981) brought subjective evaluations back to the center of analysis, raising the issue that these evaluations might well be endogenous with respect to party choice rather than, or as well as, vice-versa. Were these evaluations of a personal or more general, "sociotropic" sort regarding some collectivity rather than the individual or household (Kinder and Kiewiet, 1981; Kiewiet 1983; Kramer 1983)? Three recent articles in the *Annual Review of Political Science* describe half a century of careful refinement in the study of economic voting.³

Establishing the exogeneity of economic evaluations with respect to partisanship (Kiewiet and Lewis-Beck 2012) remains a major problem. The literature (Erikson 2004; Evans and Anderson 2006; Lewis-Beck et al. 2008; Evans and Pickup 2010; Gerber and Huber 2010; Nadeau et al. 2013) employs a variety of panel survey designs⁴ in an effort to establish temporal

² Studies of economic voting have a long tradition in Denmark. For a recent standard economic voting analysis of Denmark, see Lewis-Beck, Stubager, and Nadeau (2013) who find evidence for sociotropic economic evaluations in voting in the Danish elections since 1987, including 2011. Their "personal" question is "How is you and your family's economic situation today compared to 3-4 years ago?" and "sociotropic" is "How do you think Denmark's economic situation is today compared to 3-4 years ago?" See also Stubager et al. (2013).

³ Lewis-Beck and Stegmeier (2000) establish the total effect of economic conditions on subjective economic evaluations and voting. Anderson (2007) summarizes context-conditional effects of demographic variables, geography, and political institutions. Economics and psychology collide in Healy and Malhotra's (2013) review of specification, research design, and experiments. Kramer's (1971) and Fair's (1978) specification and estimation of observable data and Lewis-Beck's (1988) comparative analyses were important advances.

⁴ For example, Fraile and Lewis-Beck (2013) use survey-based (lagged) variables in a panel to create an instrumental variable for past ideology or partisanship.

priority and untangle the relationship between partisanship, attribution of responsibility or blame, and perception of economic conditions. Even the use of objective local information (Leigh 2005; Anderson and Roy 2011; Hansford and Gomez 2013) does not resolve the issue if people's location choices are endogenous with respect to potentially vote-relevant local economic variables such as employment prospects. Tilley and Hobolt's (2011) experiments test (1) whether a treatment asserting government responsibility affects the evaluation of "the economy" and (2) whether a treatment about relative performance (better/worse than others; or no treatment) affects beliefs about how the responsibility of government for the economy. They find modest effects in each case, a little bigger in the latter. We too use a randomized survey experiment to generate exogenous variation in information but also combine the results with a survey panel. Our instrumental variables estimation addresses causal inference issues raised by, among others, Gabel and Scheve (2007) and Imai et al. (2011).

Economic perceptions are heterogeneous with respect to socioeconomic variables and group memberships (Duch et al. 2000) and context (Weatherford 1983; Duch and Stevenson 2011; Hopkins 2012). From Books and Prysby (1999) to Reeves and Gimpel (2011) and Wright (2012), analysts suggest differentiating the local from the national economy, less by asking for subjective evaluations of local context as by showing that local rather than national variables inform perceptions through personal experience or visibility (Hansford and Gomez 2013).⁵ Our survey data allow us to examine what sort of information respondents say they value more. Our micro-data allow us to examine simultaneously the impact of economic conditions at multiple levels of aggregation, while addressing "error" and bias in the subjective personal economy data

⁵ Besley and Case (1995) suggested that the difference between voters local and surrounding larger context would indicate how well respondents felt they were doing relative to some reference group, and the gubernatorial results in Wright (2012) resemble the Besley-Case expectations.

(Huber et al. 2012; Healy and Malhotra 2013).⁶

We model the opinion formation process starting from observables. This reduces measurement error: our contextual variables are the “truth”, more or less.⁷ We assume economic evaluations can be personal or sociotropic, and that either can influence the other. Most survey evidence finds larger sociotropic effects but personal experience plays more of a role in, for instance, the perception of corruption (Deegan-Krause et al. 2011).⁸ What people think about the economy necessitates using survey measures that could reflect motivated reasoning (Taber and Lodge 2006), self-selection (Bartels 2002; Hansford and Gomez 2013), or simply a mix of what people “know about the economy and what they would like to be true about it” (Prior 2013). We address this issue but lack further experimental manipulations bearing on all possible sources of error (Bullock, Green, and Ha 2010).

II. Empirical Strategy and Data

Our study focuses on the relationship among three key variables: Individual expectations with respect to aggregate unemployment in one year’s time (AUE), individual assessments of the risk of personally experiencing unemployment over the coming year (IUE), and partisan identification, measured by vote and vote intention. The next section presents our empirical

⁶ Whether or not Lewis-Beck, Martini, and Kiewiet (2013) are correct that “American voters perceive the economy clearly, with little error”, Van der Brug et al.’s (2007) criticism that “[...]studies estimating the effects of subjective evaluations cannot be taken seriously as proper estimates of the effects of economic conditions” no longer holds.

⁷ We show how important “errors” are, and underscore the effects of “benchmarking” (Ansolabehere, Meredith, and Snowberg 2012a), who also point out that national conditions resemble something that is the responsibility of the political candidates being chosen, but the local conditions are more likely to apply to the voter, but less likely to be directly affected by choices of the politician.

⁸ Evaluations are forward- or backward-looking (Mackuen et al. (1992) but both are likely to contain at least some of the same information. Institutional contexts where responsibility is clearer (Powell and Whitten 1993) or power is more concentrated (Becher and Donnelly 2013) facilitate voter inferences when voters need not take account of post-election coalition possibilities (Duch and Stevenson 2008) or candidate strategies (Jacobson 1989; Smith 2003). Our one-country one-election limitations exclude these considerations.

strategy. We then proceed to describe the survey data, the administrative register data, and the link between the two.

II.A Empirical strategy

We model the vote choice of individual i as a function of i 's aggregate unemployment expectations (AUE_i) and controls X , (explained in more detail below), including a constant:

$$vote_i = \beta AUE_i + X_i' \gamma_{vote} + \varepsilon_i. \quad (1)$$

$vote_i$ is a binary variable. We operationalize this in different ways below: as the intention to vote for the incumbent, measured before the election, and as the actual vote for the incumbent, reported after the election in a second (panel) study. The variable ε_i is an error term. Throughout, we estimate linear probability models.⁹

A key concern in estimating the effect of perceptions of the economy on voter behavior is that omitted contextual variables can bias estimates of sociotropic concerns. To address this, we employ very fine-grained controls, including occupational and local unemployment rates and income and unemployment data from administrative registers linked to survey responses. These data are explained in more detail below. Another key concern is that voter perceptions may be driven by, rather than drive, partisan identification and vote choice. This could be an issue if, for example, people who vote for the incumbent (right wing) government do this not because they think this will imply lower unemployment but rather because they are politically right wing and consequently think that the unemployment rate is lower than do people who are left wing. The same threat of endogeneity with respect to party (but in the opposite direction) – exists for left

⁹ For robustness, we also estimated probit and, for the IV-analysis, iv-probit models, shown in Appendix Table 2, panel (b). We get qualitatively similar results, but cannot say which non-linear model would ultimately be right, so we stick to the linear probability model for transparency and tractability.

wing voters. To identify the effect of perceptions of the aggregate economy on voting, we need, short of a full structural framework for the formation of perceptions and expectations, an instrument that affects expectations over future unemployment without being correlated with background characteristics and which does not affect the vote choice directly.

To this end, we carry out a survey experiment. We randomize respondents into a control group and two treatment conditions, with one treatment group being told the actual level of unemployment and the other being informed about plans for fiscal consolidation.¹⁰ Subsequently, we ask respondents for their best estimate of the national unemployment rate a year from now. This is the variable that we call AUE .

We use the two treatment indicators and the interaction of the consolidation treatment with gender (see below) as instruments for AUE in an instrumental variables model estimated by two-stage least squares. The resulting model is

$$\begin{aligned} vote_i &= \beta AUE_i + X_i' \gamma_{vote} + \varepsilon_i \\ AUE_i &= \delta Z_i + X_i' \gamma_{AUE} + v_i \end{aligned} \tag{2}$$

where Z_i is the vector of treatment assignments and an interaction term and v_i is the error term.

For the components of Z_i to be valid instruments, they must be exogenous, correlated with AUE and not affect vote directly (or through other variables than AUE). Exogeneity follows from random assignment and the correlation with AUE is tested in the empirical analysis below. If the treatment causes voters to vote differently, or affects voter intention, this will be through the effect on AUE .

¹⁰ First, we ask respondents for their best estimate of the national unemployment rate to allow us to measure benchmarking effects (Ansolabehere et al 2012a) but we do not use this variable in the analysis in this paper,

Under the assumption of valid instruments, we can test for endogeneity of AUE by a Hausman-style test, essentially comparing the estimates from OLS and IV. If these estimates are close, we reject endogeneity and proceed by OLS, as the IV-estimator, while consistent, is inefficient. Throughout, both in the OLS and IV-estimations, we correct standard errors for clustering at the parish level (2500 residents on average). We return to this below.

Throughout we control for individual unemployment expectations, IUE, the personal probability the respondent attaches to experiencing a spell of unemployment in the next year. It was elicited before the survey experiment, and so is unaffected by the treatments. Previewing results a bit, we find that a larger perceived risk of personally experiencing unemployment affects both beliefs about the aggregate economy and the decision whether to vote for the incumbent. At the same time we find no evidence in supplementary analyses for reciprocal effects. That is, once very detailed measures of individual circumstances are accounted for, aggregate expectations and partisan identification do not seem to affect the individual's expected personal risk of unemployment.

II.B Survey data

Data on unemployment perceptions and expectations, political attitudes, political information, and voting come from the Danish Panel Study of Income and Asset Expectations (Kreiner, Lassen and Leth-Petersen, 2013a), a randomized panel survey of approximately 6,000 Danes. The survey was carried out by a professional survey firm, also responsible for the official Danish labor force surveys, and subsequently matched at the person level to administrative data and de-identified at Statistics Denmark. The data used in this project was collected as the second round of the panel, in January 2011, except for the variable capturing actual vote reported in the election of September 2011, which was taken from the third wave of the survey, collected in

January 2012 for those respondents agreeing to be re-interviewed. Throughout, individual level variables refer to 2010, the most recent year before the election, while expectations and beliefs about future levels of unemployment refer to 2011.¹¹

We employ the following questions (in Danish, translated here for presentation purposes):

- Q11: Individual unemployment expectations (IUE): How likely is it that you will experience a period without a job in 2011?

(Treatment)

- Q40: What do you think the unemployment rate will be a year from now (AUE)?
- Q46: What did you vote in the latest general election, held on November 13, 2007?
- Q47: What would you vote if there was a general election tomorrow?

(New survey round, one year later)

- Q44: What did you vote in the latest general election, held on September 15, 2011?

Vote choice variables include all eight parties then and now represented in the Danish legislature, and categories for respondents wishing not to disclose how they voted, not voting, handing in blank votes, or not remembering what they voted. From this coding we construct the binary variables “vote intention”, the current expressed intention in January 2011, “vote”, the reported actual vote in September 2011, and “past vote”, from 2007, which all take the value 1 if

¹¹ The first wave of the panel has approximately 6,000 respondents, taken from a large randomized from the Danish Central Person Registry among people with any amount of labor income in the period 1998-2004. The response rate was 55 %. Attrition to the second round was 31 %, and new respondents were taken from the larger random sample. The attrition to the third round was 21 %. Results are similar for respondents regardless of date of entry into the sample.

involving the parties incumbent in January 2011 and 0 otherwise.¹² Descriptive statistics are shown in Table 1.

- - Table 1 here - -

II.B.1: The survey experiment and Aggregate Unemployment Expectations

Survey respondents were randomized into two different information treatments and one control group. Following the elicitation of respondents' perception of current unemployment rate, the experiment was carried out as follows:

- 1/3: No information provided.
- 1/3: "According to Statistics Denmark the rate in November 2010 was 4.2 percent."
[hereafter, "truth" treatment]
- 1/3: "In 2011, the government plans to continue its recovery plan which, among other things, means that public spending cannot rise." [hereafter, "consolidation" treatment]

Tests of mean differences across control variables confirm the randomization procedure (not shown).

The distribution of responses is shown in Figure 1, estimated by kernel densities. The red line shows the distribution for the control group, with clear evidence of considerable heterogeneity in AUE. The blue curve shows the density for the sample told the actual unemployment rate. Telling respondents the actual unemployment rate ("benchmarking") has a strong and significant downward effect on AUE. The distribution of AUEs for the subsample told the actual rate is clearly much more compressed than that of the control group.

¹² Results are robust to excluding voters who report not voting or not disclosing voting behavior in any of the three categories.

- - Figure 1 here - -

The consolidation treatment, in contrast, has no effect on average, but this masks considerable gender heterogeneity in the treatment effect. Men respond by weakly lowering their expectations about future unemployment, but there is a strong and significant upward effect on women's aggregate unemployment expectations. This suggests that cuts in spending are seen as more of a threat to employment among (more frequently part time or publicly employed) women than among men. This difference is shown in the figure between the green and orange curves which show the densities for men and women, respectively, who experienced the consolidation treatment. There is no corresponding heterogeneity in the truth treatment.

The fiscal consolidation treatment thus raises two challenges: First, the interaction between the treatment and an exogenous characteristic (gender) correlated with unobserved and endogenous characteristics (such as nature of employment) that in turn can be correlated with party choice means that we do not block all 'backdoor paths' (Morgan and Winship, 2007; Pearl 2000). Second, the differential effects of the consolidation treatment raise concerns about the monotonicity assumption underlying our IV-approach (Morgan and Winship, 2007). For these reasons, we focus on the truth treatment and the control group in the IV-analysis. We can do this without introducing bias, due to the randomization of the treatments.

II.B.2. Individual unemployment expectations.

Individual unemployment expectations show much less variation than respondents' assessments of future aggregate unemployment. Figure 2 contains a histogram of responses. Almost half of the sample says that they perceive the probability of experiencing a period of unemployment in the coming year to be zero. There is some bunching at round numbers, in particular at fifty, and

eight percent of the sample think they will experience unemployment within the next year with probability one.

- - Figure 2 here - -

The register data allows us to examine the validity of individual forecasts, as expressed in January 2011, against actual experiences with unemployment over the year of 2011 as reported not by individuals themselves but by register data for 2011. The raw numbers show that IUE is very informative about actual risk of unemployment: Of the 2,501 who assign zero probability to experiencing unemployment in 2011, only 2 per cent do, while 19 per cent of the 2,567 who assign positive probability to being unemployed in fact do experience unemployment. A multiple regression confirms that IUE has strong predictive power for future unemployment even controlling for recent and historical unemployment experiences.

II.C Administrative register data

All Danes are registered by the state using their Central Person Registry (CPR) number. The CPR-number is used in any interaction with all levels of government and governmental agencies, and makes it possible for the government to track individuals across different administrative registers. Most information is either annual by construction (e.g. calendar year tax return information, fraction of year spent unemployed) or measured at one point in time every year (e.g. bank deposits, measured on December 31st for tax purposes). People can be classified as being in the labor force (self-employed, employed or unemployed) or not in the labor force (public assistance program, age pensions, early retirement program, leave etc).

The data is administrative in the sense that they are not constructed for research purposes; instead, they are created in the public sector to keep track of people across programs

and ensure compliance with program rules or to determine tax liability. As a consequence, the data covers the entire population and means that respondents in surveys, and people more generally, can always be linked to register data. At the same time, these data are used by Statistics Denmark, the Danish statistical agency, to construct official statistics of incomes, demographics, and labor markets, to name but a few.

In the present analysis, we utilize the following individual and household-level variables: age, gender, first or second generation immigrant status, education, marital status, tax return data on past and current income and wealth, including home ownership, past and current unemployment, current labor market status, occupation and registered address. The registered address makes it possible to assign respondents to administrative, geographical areas such as parishes and municipalities. All of these variables are third-party reported, that is, not reported by individuals themselves, eliminating measurement and survey errors and biases.¹³

We construct yearly and monthly measures of unemployment in a given geographical area by simply tallying up the number of registered unemployed, in- or excluding those in active labor market programs, and dividing by the number of people in the geographical area's labor force, also constructed from individual level data. Since the register data covers the entire Danish population, the resulting measures are not estimates but true population averages, and, as such, free of classical measurement error.

In this paper, we use parishes, which are the most disaggregated administrative units in Denmark. There are 98 municipalities in Denmark, with population averaging around 56,000,

¹³ See Kreiner, Lassen and Leth-Petersen (2013b) for an analysis showing that income as measured in surveys, carried out in the present panel, is subject to non-classical measurement errors in that the difference between survey reported income and register-based income measures are systematically related to respondent characteristics, and Hariri and Lassen (2013) for a demonstration of what that means for estimated relationships between income and political attitudes.

and there are almost 2,200 parishes, with population averaging about 2,500. In our sample, we have respondents from all municipalities, but only from 1416 parishes. Unemployment in municipalities ranges from 3 to about 12 % (Figure 3, left panel), but the differences in parish unemployment are more pronounced, ranging from .6 to 24.8 percent (Figure 3, right panel). In the empirical analysis below, we find no effects of municipal unemployment (not shown), but large effects for parish unemployment. In this sample at least, choosing the lowest possible level of aggregation is consequential.

- - Figure 3 here - -

Occupational unemployment is defined in a similar way, again using administrative statistics. An individual's occupation is defined as the occupation, classified using a two-digit ISCO-coding, in which the individual worked the most hours in the past year. People currently unemployed or no longer in the labor force are assigned their most recent registered occupation. We tally up the number of unemployed in an occupation relative to the total labor force of that occupation, based on register data, and this measure is then linked to respondents. Occupational unemployment is shown in Figure 4; only occupations in which there was at least one survey respondent are included in the figure.

- - Figure 4 here - -

The differences in unemployment across occupations are almost as pronounced as those among parishes, ranging from 1.6 to 30.4 percent. While the unemployed can obviously seek employment outside of their previous occupation, the numbers emphasize the very different challenges facing people without a job and, as we show below, these differences manifest themselves in perceptions of individual unemployment risk.

Finally, we include two measures of individual unemployment experiences. First, we include the share of the previous year (2010) spent unemployed. The distribution of unemployment shares is shown in Figure 5, with the insert in the top right corner showing the distribution among those with some unemployment. Most people in the sample (91.4 %) did not experience unemployment, consistent with the full-time equivalent unemployment rate being 4.5 % at the time of the survey. Among those who did experience unemployment, the distribution was quite uneven, with most people experiencing only short spells.

- - Figure 5 here - -

- - Figure 6 here - -

Figure 6 shows a similar picture for historical unemployment, averaged over the period 1998-2009. Not surprisingly, many fewer people (53.7 %) people in the sample stayed free of unemployment over a decade compared to a year, but most of those with some unemployment were not without a job for very long.

II.D Sample selection and descriptive statistics

We limit the sample in one inconsequential way. First, we censor respondents who answered the question on AUE in excess of 50 percent, on the basis that such high numbers may not reflect their true assessments. While this may seem like a high threshold of exclusion, one should note that some occupations and geographical locations, as shown in the figures above, had unemployment rates of more than twenty percent in the period we consider. A few respondents gave higher values, but did also provide unrealistic answers to many other questions, and so were dropped from the sample. In practice, we could include them or set the threshold lower, and in either case, the results remain basically unchanged. In robustness analyses (Appendix Table 1,

panel (a)), we also exclude people not in the labor force, based on their labor market status from the register data. The reason for this is that assessments about risk of individual unemployment make little sense for people who receive permanent disability insurance or are in various pension schemes, including early retirement; on the other hand, people transiting in and out of public assistance programs may be registered as being outside the labor force in November, when labor market status is determined for census purposes, even if they have actually been in the labor force most of the year. The exclusion decisions make no qualitative difference to the overall results for aggregate unemployment expectations.

Table 1 above shows descriptive statistics for all variables used in the empirical analysis. The descriptive statistics suggest a well-balanced sample, with each of the random treatments administered to approximately one-third of respondents. Gross income is measured in 100,000s, with the mean approximately equal to 54,000 euros. Note that gross income can be negative due to large negative capital income, but censoring income at both ends again makes no difference to results. Also, note that experienced unemployment is counted in 1,000s and that an IUE of 100 percent is not at all unreasonable, since the question asked for the perceived probability that one would experience a period out of work within the next year. Since in principle that period could be as little as a few days of unemployment, certainty is a possibility. The Danish system of so-called flexicurity allows workers daily or weekly unemployment, meaning that brief experiences of unemployment are not at all uncommon in seasonal work.

Finally, we access the de-identified data through a secure connection to research data servers located at Statistics Denmark, having been granted permission by the Danish Data Protection Agency.

III. Estimation Results

III.A Aggregate and Individual Unemployment Expectations, and Vote intention

Table 2 reports the estimation results for vote intention for the incumbent as measured in January 2011 as the dependent variable, about half a dozen questions after the elicitation of aggregate unemployment expectations (AUE). Columns 1-3 report, for the basic specification, the first stage of the IV regression, the second (main) stage and the OLS estimates of the main equation, respectively. Each triple of columns repeats this presentation of results. Columns 4-6 add individual unemployment expectations (IUE) as an explanatory variable. This causes the number of observations to drop by 577, or some 11 per cent of the sample; sixty percent of the missing observations are from people outside of the labor market, for whom the question may have made little sense, and the remaining missing observations are people who simply did not respond to that question. Columns 7-9 keep IUE and add a binary explanatory variable indicating whether the respondent voted for the incumbent in the previous election.

- - Table 2 here - -

The result in column 1 shows the result of the experimental treatment on voters' aggregate unemployment expectations and, moreover, documents that AUE varies systematically with voter characteristics and their labor market setting. The coefficient of the treatment reflects what was shown in Figure 1. The truth treatment, telling people the actual unemployment rate, reduces AUE by 1.5, ten times the standard error: a strong, negative effect. Unemployment expectations also vary systematically with background characteristics: women and (first and second generation) immigrants think the aggregate unemployment rate will be higher, while more educated and higher income respondents expect it to be lower. The labor market matters as well. Respondents in high unemployment occupations report significantly higher AUE than those in low unemployment occupations. The local unemployment rate, measured at the parish level,

does significantly influence AUE in this specification, but the addition of further controls (particularly IUE, below) renders this effect insignificant, though it remains positive.¹⁴

We use the strong effect of the treatment in the IV-regression, shown in column 2. The F-test statistic for the survey treatment in the first stage is 82.2. Based on this, we can assess the effect of AUE on intention to vote for the incumbent. This effect is negative but also insignificant, suggesting little or no forward-looking sociotropic voting.¹⁵ However, under the assumption of a valid instrument, we can test for endogeneity of AUE by a Hausman-style test, essentially comparing the estimates of AUE under IV and OLS. We cannot reject exogeneity of AUE, a result which continues to hold for all specifications. Hence, because in this case IV is inefficient, we proceed to look at the results from the OLS regressions, shown in column 3, which for ease of presentation also contains the consolidation treatment cases.¹⁶

Here there is indeed significant evidence of forward-looking sociotropic voting: comparing two individuals, someone who thinks unemployment will be 10 percentage points higher is more than six percentage points less likely to vote for the incumbent. The other results are also of interest. Higher parish-level unemployment decreases the probability for a vote for the incumbent. High occupational unemployment rates also have a direct negative effect. Both these effects are in addition to the (negative) effects of local and occupational unemployment

¹⁴ In results not reported, we find that this is because the effect is significant and large for the subset of voters who agree with a statement that they see little value in being politically informed when making personal economic decisions, while it is insignificant for voters who value political knowledge (and probably are more knowledgeable). We regard this not as a causal effect but also as accurate self-description and validation by a subset of respondents: “I don’t need political information to make personal economic decisions, I just look around”. We leave further analysis of this condition for subsequent analysis.

¹⁵ This is not because column 1 contains controls in addition to the treatments. Restricting the specification to the experimental variable produces similar results.

¹⁶ The larger number of cases in the OLS regressions reduce standard errors. Even so, in this and most specifications, the demanding pairwise test proposed by Guggenberger (2010) supports exogeneity, as do further robustness tests based on bootstrapping the Hausman test in the presence of clustered standard errors.

through AUE. The effect of unemployment in the last year is negligible, and this is true even when excluding the share of time spent unemployed in the previous decade which does have a significant negative effect on its own. Demographics, income and education have the effects that conform to our expectations. High income and home ownership are significant predictors of intending to vote for the incumbent. High education diminishes incumbent support. The effect of age is nonlinear: the young are less likely to support the incumbent but the effect eventually reverses among those who are older, who are more likely to support the incumbent. Recall that the incumbents are parties of the Right: immigrants are less likely to support the incumbents.

Columns 4-6 add individual unemployment expectations (IUE). Column 4 shows that the respondent's reported probability of experiencing a period of unemployment in the coming year has a significant and positive effect on AUE. Since respondents were asked this question prior to the treatment and the AUE question, the answer is not confounded with the intervention. IUE moreover has a direct, negative effect on intention to vote for the incumbent: increasing the probability of experiencing some unemployment in the coming year from 0 to 100 % decreases the likelihood of voting for the incumbent by 5 percentage points. Moreover, the magnitude and significance of this effect do not depend on the specification choice between columns 5 and 6. Including IUE increases the standard errors of estimates for other variables with which it is correlated (see below) but otherwise the estimates in column 6 resemble those in column 3.

Finally, columns 7-9 include past vote for the incumbent as an explanatory variable. Unsurprisingly, past vote for the incumbent is a strong predictor of intending to vote for the incumbent in the next election. The inclusion of past vote in the first stage confirms the correlation between partisanship and AUE. Results for other variables that affect AUE are largely unchanged. Obviously, this does not imply that partisanship affects AUE directly, since

both AUE and partisanship could still be influenced by a common set of variables. Including partisanship does not change the IV-regression or the test for endogeneity. Our results continue to suggest that AUE is exogenous and, hence, that OLS should be used. These OLS results, reported in column 9, shows that past partisanship subsumes many of the effects on incumbent support noted earlier. Importantly, the AUE effect remains statistically significant, though smaller. Individual unemployment expectations also continue to affect vote intention, consistent with this measure not being fixed and indeed likely to vary over the course of the financial crisis, which, as above, was not an issue in the previous election in September 2007. The effects of gender, age, and education remain but are muted. Parish and occupational unemployment, income, immigrant status, education and home ownership all cease to be significant predictors of vote intention, suggesting that these variables were already causing the past vote.

III.B Aggregate and Individual Unemployment Expectations, and Actual Vote

Table 3 repeats the full set of specifications from Table 2 but with actual vote in the September 2011 election as dependent variable instead of vote intention. Theoretically, the link between AUE and vote could be weaker, since respondents may have updated their expectations between the time of the previous survey (January 2011) and the election. On the other hand, the vote intention data includes a number of respondents who were previously undecided about vote intention. If they have now made a vote decision consistent with their earlier AUE, this could strengthen the relationship, or weaken it if the “undecided” simply reverted to the party they voted for last time regardless of their January AUE. Finally, there is panel attrition, with the number of responses in the main sample down from 5,958 to 4,805, a decrease of 20 per cent.

- - Table 3 here - -

Throughout the Table, most results do not change qualitatively from what we discussed in Table 2, so we will be brief. We continue to reject endogeneity. The results on the control variables are also broadly similar (but somewhat less precisely estimated) to those found for vote intention. This might reflect panel attrition (increasing standard errors) or the stronger effect of past vote on actual vote (the coefficient is larger by a third), since many past voters for the incumbent who declared themselves undecided in the January survey of vote intentions in fact vote for the incumbent in September. However, again importantly, in spite of the diminution of the effects of other controls, the contribution of personal unemployment expectations voting remains evident: it retains significance and is of the same magnitude as it was in Table 2.¹⁷ However, across the OLS specifications, the estimated effect of AUE is generally smaller than the IV estimate, but similar in magnitude to what we reported for vote intention in Table 2, except in column 9, where it is both smaller than in Table 2 and statistically significant only at the 5 per cent level. We return to this below. All in all, the OLS estimates are still consistent with forward looking sociotropic voting, as they were with vote intention in January 2011.

So far, the IV analysis (but not the OLS) has excluded the fiscal consolidation treatment due to its heterogeneous effects conditional on gender. Since such heterogeneity may be due to unobserved employment status, this raises concerns about “back door paths” or violation of the exclusion restriction. That this concern is real, is confirmed by including the fiscal consolidation treatment as an additional instrument; for example, in the case of specification (9) in Table 2, the Hansen J-statistic for overidentification is borderline significant. However, once we allow heterogeneous treatment effects by including an interaction between the

¹⁷ We will eventually be able to say whether that relationship is affected by whether their expectations were fulfilled, or just set a mood, but the unemployment data for 2011 is not yet available.

consolidation treatment and gender, there is no longer clear evidence of violating the exclusion restriction. Appendix Table 1, panel (c) analyzes vote intention and actual vote, controlling for both IUE and past vote, with the full set of instruments. The F-test statistic there for the first stage is lower, but still high. There is no evidence of overidentification. We continue to be unable to reject exogeneity of AUE. The main results for IUE and AUE are qualitatively unchanged from those presented in Tables 2 and 3. This is also true if we exclude the consolidation treatment cases from the OLS analyses in those tables (results not shown).

III.C The origins of Individual Unemployment Expectations

What are the roots of IUE, the probability an individual attaches to a personal spell of unemployment in the next year? This probability, IUE, is correlated with AUE, but was ascertained first in the survey, well before the unemployment perception and expectation questions and experimental treatment. Table 4 reports results from a Tobit estimation of the causes of IUE. We use Tobit as IUE is a probability constrained to be between 0 and 100 and, moreover, has many responses at the extremes.

- - Table 4 here - -

The first column includes AUE and standard controls. Income, age and, to a lesser extent, education all affect IUE. Respondents' forecast of the national unemployment rate is significantly positively correlated with IUE. This continues to hold in the second column, where we include past vote for the incumbent, which is significantly negatively correlated with IUE. Both of these results raise the concerns noted throughout the literature that perceptions of individual unemployment risks are affected by assessments of the greater economy (Lewis-Beck and Stegmeier 2000) as well as partisan identification as expressed here by past vote (Fraile and

Lewis-Beck 2013). However, the inclusion of fine-grained local and individual controls, in column 3, changes these conclusions.

Personal unemployment experiences, in the past and earlier years, strongly predict individual unemployment forecasts.¹⁸ Finally, home ownership, an indicator for stable income since all homeowners with a mortgage have to be approved by mortgage institutions, is significantly negatively correlated with IUE. Together, the inclusion of these controls dramatically influences the estimated coefficients of AUE and partisan identification. The coefficient on AUE is reduced almost by a third from column 2, the coefficient on partisan identification by a half, and both are insignificant. This remains true for alternative samples in robustness analyses. The overall picture that emerges is that individual unemployment expectations are largely determined by individual and contextual labor market variables, which also affect aggregate unemployment expectations. That is, IUE affects AUE and partisan choices, but is less affected by those variables.¹⁹

IV. Discussion

The main result is that sociotropic economic evaluations have a *causal* effect on vote intention and, subsequently, on actual voting, with important consequences for the theory of economy-based accountability. The evaluation measure that we call AUE is forward looking and has a precise meaning: it is what voters believe will be the unemployment rate in the country a year

¹⁸ Current unemployment in respondents' occupation is has a positive, but imprecisely estimated, effect; in robustness analyses this is sometimes significant, sometimes not. Parish unemployment is not significant, but as noted above, the insignificant average effect conflates a significant positive effect for people unconcerned with political information and a negative but insignificant effects for those who value political information – and who presumably seek such information beyond the parish.

¹⁹ An instrumental variables tobit using the truth treatment as instrument also yields an insignificant coefficient on AUE and does not allow us to reject exogeneity here either.

hence. Many pieces of our research strategy contribute to this result, including careful analysis of endogeneity. Repeated tests across estimators, samples, and instrument variables strategies show that we cannot reject the hypothesis that the unemployment forecast is exogenous with respect to partisanship. Therefore the use of IV is inefficient and we should use a standard least squares specification. There, AUE is strongly significant throughout, when controlling for individual unemployment risk and past partisanship.

We also observe interesting results about AUE, which is affected by priming individuals with an “authoritative” version of the current rate of unemployment. AUE depends on a respondent’s personal unemployment history and on local and occupational unemployment rates, as well as demographic characteristics like immigrant status, gender, education, and income. Moreover, personal unemployment experiences, both in the past year and over a longer term, strongly affect individual unemployment forecasts. Current unemployment in respondents’ occupation also has a positive, if less precisely estimated, effect.²⁰ Home ownership is significantly negatively correlated with IUE. These factors eliminate any effects of AUE and past partisan voting: both are insignificant, suggesting that observing significant results in regressions without such detailed controls may be the outcome of joint determinants in the form of such individual unemployment experiences.

How much should we make of the fact that between January and September the personal effect continues to affect partisan choice, while the sociotropic effect fades somewhat? Literally this means that those people making a national forecast that moved them to intend to vote against the incumbents are more likely to have shifted back to fundamentals. Put another

²⁰ See note 18 about people unconcerned with political information.

way, their personal forecasts have had more durable political consequences.²¹ It means that in order to understand sociotropic voting, we have to see that while the national forecast is a function of the "right" things, that is, it is meaningful and based on economic information, it might be more likely to be transitory than the personal effect.

That would also be consistent with the results of many studies that track time series of unemployment and political opinion that find robust correlations over time. The correlation is there at the time of each interview, whether between economic evaluations and vote intention at the time, or between evaluations and recalled vote in post-election surveys. This result is different: the effects of the national forecast are evident in contemporaneous vote intention, but, perhaps partly because they were induced by an experimental treatment, the political effects of feelings about the national economy have faded somewhat eight months later, while the effects of personal expectations remain.

Finally, however, that result can also be interpreted to serve as a real caution against inferring too much from results in a single survey. Post-election studies that ask about AUE and IUE may not project directly back to the time before the election when voters were making up their minds. In the same way, we may not learn as much as we would like from pre-election surveys of vote intention and AUE (Kayser and Wlezian 2011), because those surveys may elicit correlations that prove ephemeral.

²¹ Margalit (2013) finds that the effects of a spell of unemployment on attitudes about redistribution are transitory. Gerber et al. (2011) show how fast campaign effects pass.

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Figure 1: Aggregate unemployment expectations

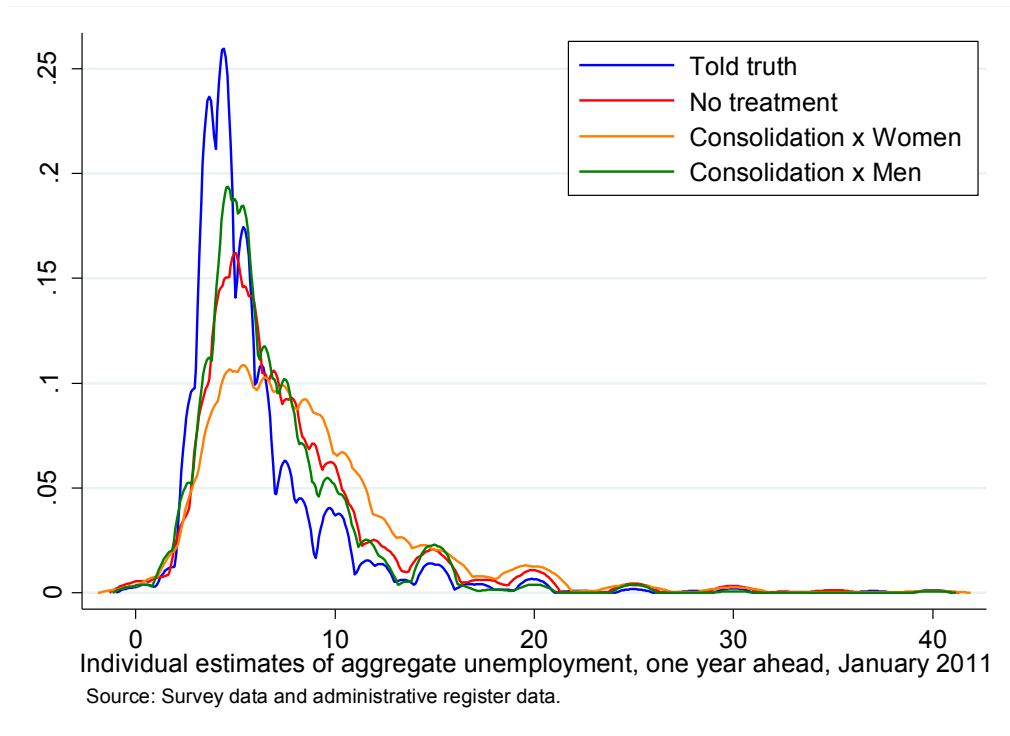


Figure 2: Individual unemployment expectations

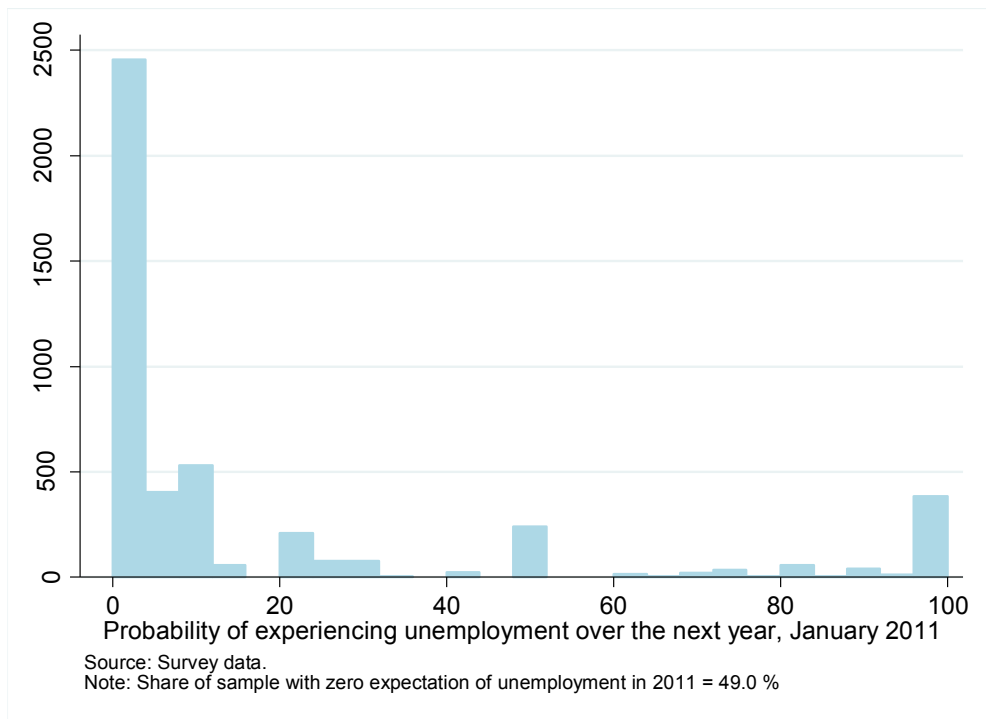


Figure 3: Municipal and parish unemployment rates, 2010.

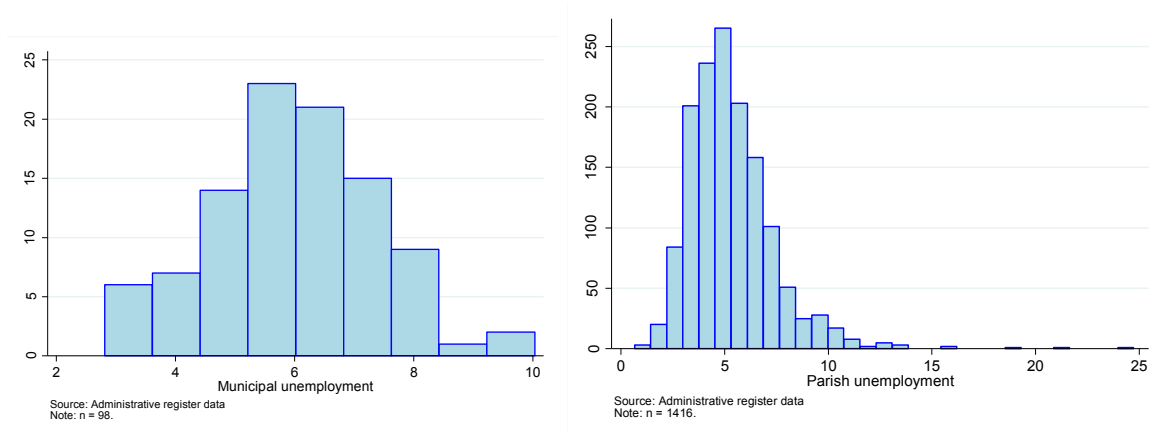


Figure 4: Occupational unemployment rates, 2010.

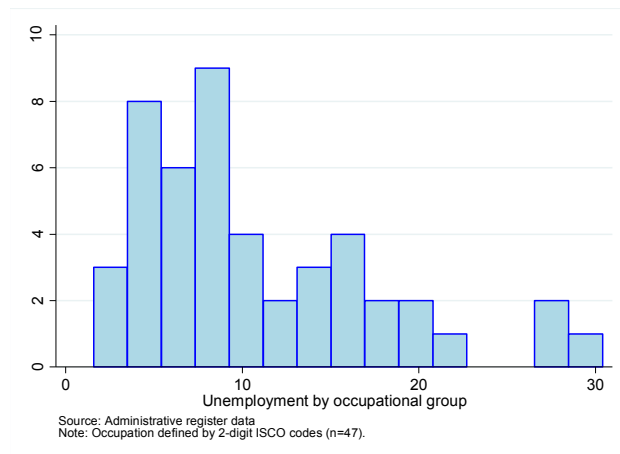


Figure 5: Recent (2010) individual unemployment experiences

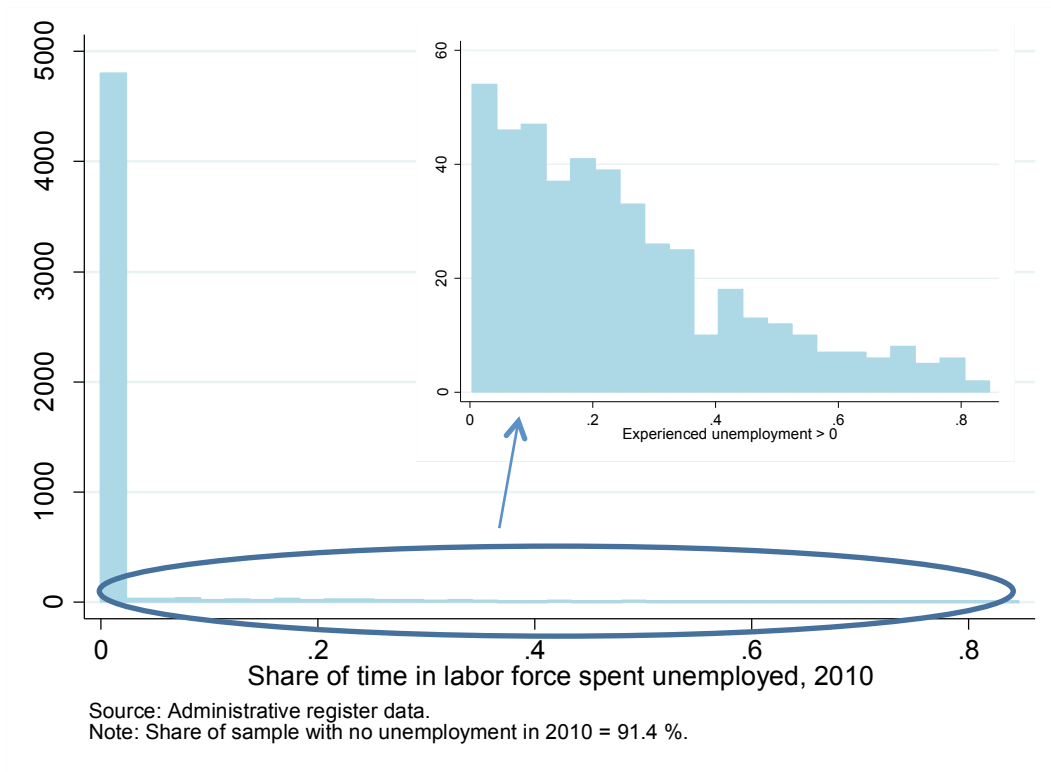


Figure 6: Recent (2010) individual unemployment experiences

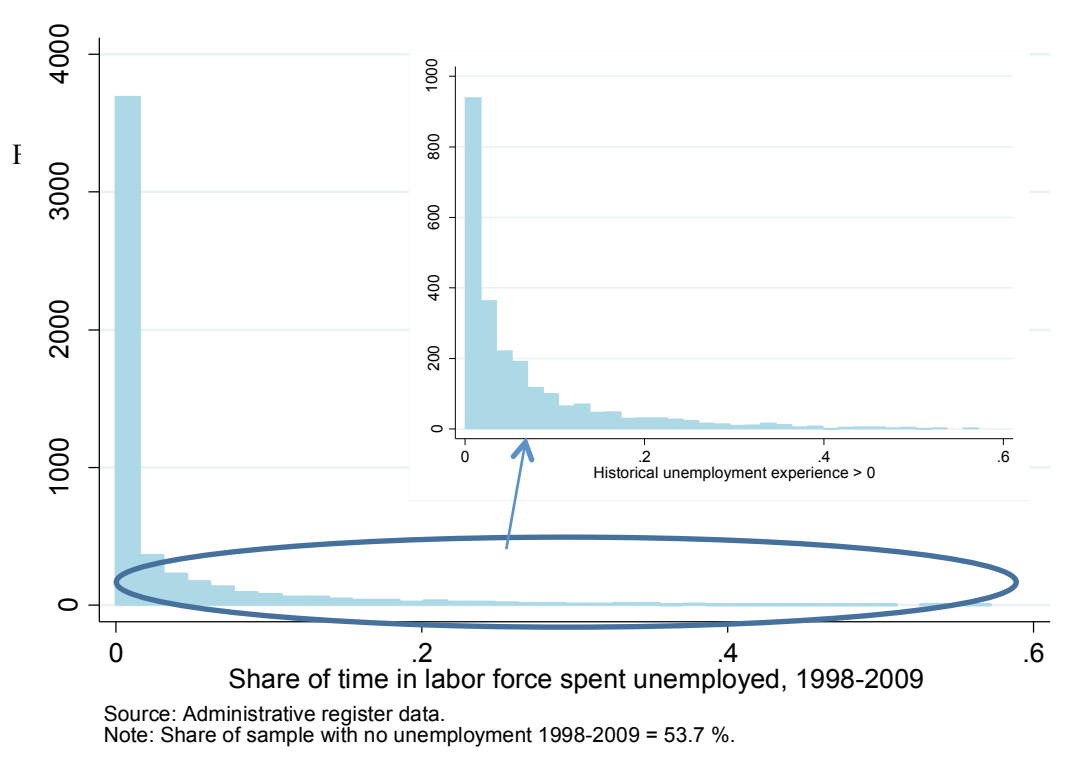


Table 1. Descriptive statistics

	mean	sd	min	max	N	Date of record
Incumbent vote	0.32	0.47	0	1	4810	January 2012
Incumbent vote intent	0.21	0.40	0	1	5965	January 2011
Voted for incumbent in previous election	0.35	0.48	0	1	5964	January 2011
Agg. Unempl. Expectations, AUE	7.36	5.29	0	50	5965	January 2011
Treatment: Truth	0.33	0.47	0	1	5965	January 2011
Treatment: Consolidation	0.33	0.47	0	1	5965	January 2011
Indiv. Unempl. Expectations, IUE	18.44	31.66	0	100	5048	January 2011
Unemployment rate, parish §	5.44	2.05	0.65	24.75	5965	January 2011
Unemployment rate, occupation §	9.06	5.75	1.57	30.40	5965	Average 2010
Woman §	0.51	0.50	0	1	5965	N/A
Age §	46.27	11.45	23	65	5965	N/A
Gross income (100,000s) §	3.98	2.50	-13.71	48.89	5965	Sum 2010
Immigrant or descendant §	0.04	0.20	0	1	5965	N/A
Share of year unemployed (full year = 1000) §	25.88	106.99	0	929	5965	Average 2010
Share of decade unemployed (all the time = 1000) §	31.08	70.55	0	573	5965	Av. 1998-2009
Education: Short §	0.44	0.50	0	1	5965	Dec. 31, 2010
Education: Medium §	0.23	0.42	0	1	5965	Dec. 31, 2010
Education: Long §	0.10	0.31	0	1	5965	Dec. 31, 2010
Home ownership §	0.64	0.48	0	1	5965	Dec. 31, 2010

§ Data from administrative registry data.

Table 2. Aggregate and individual unemployment expectations and vote intention: OLS and IV estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	IV	OLS	OLS	IV	OLS	OLS	IV	OLS
	AUE	Vote intent	Vote intent	AUE	Vote intent	Vote intent	AUE	Vote intent	Vote intent
Agg. Unempl. Expectations (AUE)		0.0066 [0.0088]	-0.0063*** [0.0007]		0.0068 [0.0109]	-0.0058*** [0.0008]		0.0017 [0.0092]	-0.0034*** [0.0007]
Indiv. Unempl. Expectations (IUE)				0.0094** [0.0033]	-0.0006* [0.0002]	-0.0006*** [0.0002]	0.0092** [0.0034]	-0.0004* [0.0002]	-0.0004** [0.0001]
Unemployment rate, parish §	0.1024* [0.0498]	-0.0136*** [0.0033]	-0.0110*** [0.0025]	0.0550 [0.0457]	-0.0103** [0.0036]	-0.0091*** [0.0027]	0.0446 [0.0463]	-0.0014 [0.0030]	-0.0001 [0.0024]
Unemployment rate, occupation §	0.0573*** [0.0162]	-0.0027* [0.0013]	-0.0024* [0.0009]	0.0577*** [0.0169]	-0.0033* [0.0014]	-0.0029** [0.0010]	0.0553** [0.0168]	-0.0010 [0.0011]	-0.0007 [0.0008]
Woman §	1.2272*** [0.1740]	-0.0907*** [0.0170]	-0.0746*** [0.0109]	1.2048*** [0.1875]	-0.1012*** [0.0194]	-0.0890*** [0.0115]	1.1422*** [0.1864]	-0.0433** [0.0160]	-0.0372*** [0.0099]
Age §	0.1067 [0.0656]	-0.0190*** [0.0050]	-0.0137*** [0.0039]	0.1246+ [0.0753]	-0.0169** [0.0058]	-0.0097* [0.0044]	0.1191 [0.0754]	-0.0117* [0.0049]	-0.0094* [0.0038]
Age^2	-0.0013+ [0.0007]	0.0002*** [0.0001]	0.0002*** [0.0000]	-0.0016+ [0.0008]	0.0002** [0.0001]	0.0001* [0.0000]	-0.0015+ [0.0008]	0.0001* [0.0001]	0.0001* [0.0000]
Gross income §	-0.1125*** [0.0306]	0.0177*** [0.0044]	0.0122*** [0.0032]	-0.0778* [0.0312]	0.0149*** [0.0045]	0.0108*** [0.0033]	-0.0674* [0.0309]	0.0059 [0.0036]	0.0041 [0.0026]
Immigrant or descendant §	0.7487 [0.5497]	-0.0685** [0.0266]	-0.0637** [0.0196]	0.9174 [0.6152]	-0.0777** [0.0291]	-0.0669** [0.0203]	0.8439 [0.6178]	-0.0122 [0.0260]	-0.0128 [0.0188]
Share of year unemployed §	0.0030* [0.0015]	-0.0001+ [0.0001]	0.0000 [0.0001]	0.0027 [0.0017]	-0.0001+ [0.0001]	0.0000 [0.0001]	0.0027 [0.0017]	-0.0001 [0.0001]	0.0000 [0.0001]
Share of year unemployed, average 1998-2009 §	-0.0002 [0.0014]	-0.0001 [0.0001]	-0.0002*** [0.0001]	-0.0002 [0.0017]	-0.0001 [0.0001]	-0.0002** [0.0001]	-0.0004 [0.0017]	0.0001 [0.0001]	-0.0000 [0.0001]
Education: Short §	-0.1062 [0.2260]	0.0468** [0.0169]	0.0469*** [0.0136]	-0.2553 [0.2593]	0.0541** [0.0188]	0.0499*** [0.0150]	-0.2139 [0.2588]	0.0185 [0.0152]	0.0163 [0.0124]
Education: Medium §	-0.4044+ [0.2400]	-0.0392* [0.0189]	-0.0422** [0.0152]	-0.4884+ [0.2767]	-0.0288 [0.0210]	-0.0361* [0.0160]	-0.5067+ [0.2760]	-0.0162 [0.0170]	-0.0187 [0.0134]
Education: Long §	-0.9483*** [0.2751]	-0.0208 [0.0262]	-0.0413* [0.0199]	-1.0056** [0.3113]	-0.0320 [0.0280]	-0.0479* [0.0205]	-1.0295** [0.3122]	-0.0175 [0.0222]	-0.0242 [0.0169]
Home ownership §	-0.1485 [0.1959]	0.0349* [0.0150]	0.0371** [0.0118]	-0.1604 [0.2083]	0.0332* [0.0163]	0.0409** [0.0125]	-0.1088 [0.2090]	-0.0103 [0.0131]	-0.0004 [0.0105]
Treatment: Actual unemployment rate	-1.4647*** [0.1616]			-1.3180*** [0.1756]			-1.3262*** [0.1754]		
Voted for incumbent in last election							-0.5689** [0.1830]	0.4699*** [0.0168]	0.4560*** [0.0131]
Observations	3,970	3,970	5,958	3,357	3,357	5,041	3,357	3,357	5,041
R-squared	0.0569	0.0296	0.0524	0.0562	0.0327	0.0587	0.0587	0.3264	0.3255
Sample	Full	Full	Full	Full	Full	Full	Full	Full	Full
F 1st stage		82.20			56.35			57.17	
Test for endogeneity		2.019			1.357			0.334	
p-value		0.155			0.244			0.563	

Robust standard errors corrected for clustering at the parish level in brackets.

*** p < .001, ** p < .01, * p < .05, + p < .10.

A constant was included in all regressions, but is not reported.

Regressions were carried out in Stata 12.1 using OLS and ivreg2.

§ Data from administrative registry data.

Table 3. Aggregate and individual unemployment expectations and voting: OLS and IV estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	IV	OLS	OLS	IV	OLS	OLS	IV	OLS
	AUE	Actual vote	Actual vote	AUE	Actual vote	Actual vote	AUE	Actual vote	Actual vote
Agg. Unempl. Expectations (AUE)		0.0128	-0.0054***		0.0125	-0.0053***		0.0066	-0.0023*
		[0.0143]	[0.0012]		[0.0142]	[0.0014]		[0.0111]	[0.0011]
Indiv. Unempl. Expectations (IUE)				0.0067*	-0.0005	-0.0005*	0.0066+	-0.0003	-0.0004*
				[0.0034]	[0.0003]	[0.0002]	[0.0034]	[0.0002]	[0.0002]
Unemployment rate, parish §	0.0498	-0.0187***	-0.0165***	0.0488	-0.0187***	-0.0143***	0.0403	-0.0073*	-0.0030
	[0.0455]	[0.0043]	[0.0033]	[0.0455]	[0.0043]	[0.0035]	[0.0460]	[0.0032]	[0.0026]
Unemployment rate, occupation §	0.0680***	-0.0032+	-0.0022+	0.0683***	-0.0032+	-0.0028*	0.0666***	-0.0006	-0.0002
	[0.0174]	[0.0019]	[0.0012]	[0.0174]	[0.0019]	[0.0013]	[0.0173]	[0.0015]	[0.0010]
Woman §	1.2421***	-0.0836**	-0.0461**	1.2283***	-0.0822**	-0.0677***	1.1716***	-0.0008	0.0065
	[0.1982]	[0.0259]	[0.0142]	[0.1979]	[0.0257]	[0.0154]	[0.1996]	[0.0207]	[0.0123]
Age §	0.0653	-0.0109	-0.0035	0.0818	-0.0121	0.0007	0.0759	-0.0039	0.0017
	[0.0767]	[0.0075]	[0.0051]	[0.0777]	[0.0076]	[0.0060]	[0.0775]	[0.0060]	[0.0050]
Age^2	-0.0009	0.0001	0.0000	-0.0011	0.0001	-0.0000	-0.0010	0.0000	-0.0000
	[0.0008]	[0.0001]	[0.0001]	[0.0008]	[0.0001]	[0.0001]	[0.0008]	[0.0001]	[0.0001]
Gross income §	-0.0767*	0.0211***	0.0164***	-0.0616+	0.0199***	0.0146***	-0.0521	0.0072+	0.0040
	[0.0339]	[0.0047]	[0.0034]	[0.0340]	[0.0047]	[0.0036]	[0.0339]	[0.0041]	[0.0028]
Immigrant or descendant §	0.2668	-0.1792***	-0.1667***	0.2948	-0.1811***	-0.1793***	0.2265	-0.0900**	-0.0877***
	[0.4261]	[0.0352]	[0.0267]	[0.4286]	[0.0352]	[0.0279]	[0.4318]	[0.0284]	[0.0225]
Share of year unemployed §	0.0017	-0.0002*	-0.0000	0.0012	-0.0002+	-0.0000	0.0011	-0.0001	0.0000
	[0.0015]	[0.0001]	[0.0001]	[0.0015]	[0.0001]	[0.0001]	[0.0015]	[0.0001]	[0.0001]
Share of year unemployed, average 1998-2009 §	-0.0014	-0.0002+	-0.0003***	-0.0017	-0.0002	-0.0003**	-0.0019	-0.0001	-0.0001
	[0.0017]	[0.0001]	[0.0001]	[0.0017]	[0.0001]	[0.0001]	[0.0017]	[0.0001]	[0.0001]
Education: Short §	-0.0012	0.0680**	0.0532**	0.0182	0.0666**	0.0693***	0.0542	0.0196	0.0231
	[0.2772]	[0.0237]	[0.0174]	[0.2779]	[0.0237]	[0.0190]	[0.2770]	[0.0183]	[0.0151]
Education: Medium §	-0.5139+	-0.0428	-0.0669***	-0.5024+	-0.0438	-0.0599**	-0.5140+	-0.0316	-0.0387*
	[0.2755]	[0.0278]	[0.0195]	[0.2748]	[0.0277]	[0.0209]	[0.2744]	[0.0212]	[0.0160]
Education: Long §	-0.7477*	-0.0724*	-0.0655*	-0.7508*	-0.0724*	-0.0664*	-0.7669*	-0.0557*	-0.0375+
	[0.3337]	[0.0334]	[0.0260]	[0.3341]	[0.0334]	[0.0269]	[0.3349]	[0.0262]	[0.0210]
Home ownership §	-0.0858	0.0636**	0.0729***	-0.0816	0.0632**	0.0716***	-0.0442	0.0138	0.0252+
	[0.2207]	[0.0219]	[0.0155]	[0.2199]	[0.0218]	[0.0170]	[0.2193]	[0.0173]	[0.0137]
Voted for incumbent in last election							-0.4667*	0.6105***	0.6110***
							[0.1896]	[0.0180]	[0.0138]
Observations	2,688	2,688	4,805	2,688	2,688	4,049	2,688	2,688	4,049
R-squared	0.0536	0.0370	0.0574	0.0552	0.0390	0.0657	0.0572	0.4156	0.4263
Sample	Full	Full	Full	Full	Full	Full	Full	Full	Full
F 1st stage		47.81			48.13			48.58	
Test for endogeneity		1.401			1.345			0.487	
p-value		0.236			0.246			0.485	

Robust standard errors corrected for clustering at the parish level in brackets.

*** p < .001, ** p < .01, * p < .05, + p < .10.

A constant was included in all regressions, but is not reported.

Regressions were carried out in Stata 12.1 using OLS and ivreg2.

§ Data from administrative registry data.

Table 4. The relationship between Aggregate and Individual Unemployment Expectations

	(1)	(2)	(3)
	Individual unemployment Expectations		
Agg. Unempl. Expectations (AUE)	0.4091*	0.3735+	0.2682
	[0.2029]	[0.2033]	[0.1943]
Woman §	-2.7265	-3.3238	-1.8306
	[2.0432]	[2.0459]	[2.0228]
Age §	-3.5383***	-3.5286***	-4.2756***
	[0.8620]	[0.8590]	[0.8589]
Age^2	0.0362***	0.0360***	0.0451***
	[0.0099]	[0.0099]	[0.0099]
Gross income §	-5.5301***	-5.3567***	-3.4926***
	[0.8838]	[0.8716]	[0.7375]
Immigrant or descendant §	0.6325	-0.2247	-2.3995
	[4.8204]	[4.8091]	[4.9436]
Education: Short §	-7.8040**	-7.2639**	-4.8852+
	[2.6752]	[2.6764]	[2.5907]
Education: Medium §	-3.7978	-3.8511	-0.8526
	[2.8815]	[2.8750]	[2.8487]
Education: Long §	1.0729	0.8805	2.4719
	[3.3674]	[3.3563]	[3.2320]
Voted for incumbent in last election		-5.8431**	-2.8739
		[2.0017]	[1.9587]
Home ownership §			-6.5412**
			[2.2340]
Unemployment rate, parish §			-0.3661
			[0.4297]
Unemployment rate, occupation §			0.1487
			[0.1709]
Share of year unemployed §			0.0664***
			[0.0165]
Share of year unemployed, average 2007-2009 §			0.1248***
			[0.0299]
Share of year unemployed, average 1998-2006 §			0.0664***
			[0.0146]
Observations	5,076	5,076	4,997
Sample	Full	Full	Full
Estimator	Tobit	Tobit	Tobit

Robust standard errors corrected for clustering at the parish level in brackets.

*** p < .001, ** p < .01, * p < .05, + p < .10.

A constant was included in all regressions, but is not reported.

Regressions were carried out in Stata 12.1 using tobit.

§ Data from administrative registry data.

Appendix Table 1: Robustness, voting regressions

	(a) Labor force sample							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	IV	OLS	IV	OLS	IV	OLS	IV	OLS
	Intent	Intent	Intent	Intent	Vote	Vote	Vote	Vote
Agg. Unempl. Expectations (AUE)	0.0029 [0.0102]	-0.0058*** [0.0008]	-0.0005 [0.0096]	-0.0033*** [0.0008]	0.0098 [0.0132]	-0.0045** [0.0014]	0.0075 [0.0113]	-0.0022+ [0.0012]
Indiv. Unempl. Expectations (IUE)			-0.0005* [0.0002]	-0.0005** [0.0002]			-0.0004 [0.0003]	-0.0004+ [0.0002]
Voted for incumbent in last election			0.4607*** [0.0171]	0.4528*** [0.0133]			0.6035*** [0.0183]	0.6070*** [0.0140]
Observations	3,376	5,067	3,185	4,784	2,711	4,083	2,563	3,860
R-squared	0.0447	0.0556	0.3221	0.3216	0.0440	0.0612	0.4063	0.4211
Sample	Labor force	Labor force	Labor force	Labor force	Labor force	Labor force	Labor force	Labor force
F 1st stage	63.32		52.32		58.34		47.58	
Test for endogeneity	0.715		0.103		1.047		0.621	
p-value	0.398		0.748		0.306		0.431	
	(b) Probit estimation							
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	IV_Probit	Probit	IV_Probit	Probit	IV_Probit	Probit	IV_Probit	Probit
	Intent	Intent	Intent	Intent	Vote	Vote	Vote	Vote
Agg. Unempl. Expectations (AUE)	0.0201 [0.0303]	-0.0344*** [0.0048]	0.0108 [0.0448]	-0.0260*** [0.0057]	0.0353 [0.0312]	-0.0182*** [0.0044]	0.0211 [0.0475]	-0.0102+ [0.0056]
Indiv. Unempl. Expectations (IUE)			-0.0024* [0.0011]	-0.0025** [0.0010]			-0.0016 [0.0011]	-0.0018* [0.0009]
Voted for incumbent in last election			1.7436*** [0.0840]	1.7545*** [0.0562]			1.8319*** [0.0695]	1.8503*** [0.0503]
Observations	3,970	5,958	3,357	5,041	3,193	4,805	2,688	4,049
Sample	Full	Full	Full	Full	Full	Full	Full	Full
Test for endogeneity	2.67		0.65		2.23		0.26	
p-value	0.1025		0.4208		0.1351		0.6067	
	(c) IV analysis with heterogenous treatment effects							
	(17)	(18)	(19)	(20)	(21)	(22)		
	1st stage	IV	OLS	1st stage	IV	OLS		
	Intent	Intent	Intent	Vote	Vote	Vote		
Agg. Unempl. Expectations (AUE)		-0.0077 [0.0067]	-0.0035*** [0.0007]		0.0002 [0.0081]	-0.0023* [0.0011]		
Indiv. Unempl. Expectations (IUE)	0.0072** [0.0026]	-0.0004* [0.0002]	-0.0004* [0.0002]	0.0047+ [0.0028]	-0.0004* [0.0002]	-0.0004+ [0.0002]		
Voted for incumbent in last election	-0.6402*** [0.1429]	0.4537*** [0.0137]	0.4564*** [0.0131]	-0.5167*** [0.1517]	0.6126*** [0.0142]	0.6113*** [0.0137]		
Treatment: Actual unemployment rate	-1.3036*** [0.1749]			-1.2347*** [0.1784]				
Treatment: Fiscal consolidation	-0.0867 [0.1988]			0.0539 [0.2096]				
Woman*Fiscal consolidation treatment	0.6626* [0.3016]			0.5616+ [0.3195]				
Observations	5,048	5,048	5,048	4,054	4,054	4,054		
Sample	Full	Full	Full	Full	Full	Full		
F 1st stage		29.65			27.45			
Test for endogeneity		0.668			0.0784			
p-value		0.414			0.681			
J-statistic		3.528			0.768			
p-value		0.171			0.779			

Robust standard errors corrected for clustering at the parish level in brackets.

*** p < .001, ** p < .01, * p < .05, + p < .10.

A full set of controls was included in all regressions, but results are not reported.

Regressions were carried out in Stata 12.1 using OLS, ivreg2, ivprobit and probit.

§ Data from administrative registry data.

Appendix Table 2. Robustness, IUE

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual unemployment Expectations			Individual unemployment Expectations		
Agg. Unempl. Expectations (AUE)	0.4548*	0.4289*	0.2868	0.5189*	0.4959*	0.3320
	[0.1931]	[0.1932]	[0.1840]	[0.2501]	[0.2514]	[0.2374]
Woman §	-2.9628	-3.4026+	-0.9718	-3.4795	-3.8852	-2.0218
	[1.9353]	[1.9472]	[1.9300]	[2.3431]	[2.3684]	[2.3448]
Age §	-1.5627+	-1.5540+	-2.5051**	-2.1291*	-2.1620*	-3.1362**
	[0.8062]	[0.8046]	[0.7886]	[0.9940]	[0.9917]	[1.0095]
Age^2	0.0123	0.0122	0.0236**	0.0203+	0.0206+	0.0318**
	[0.0092]	[0.0092]	[0.0090]	[0.0114]	[0.0114]	[0.0115]
Gross income §	-4.1107***	-3.9858***	-2.2468***	-6.4414***	-6.2867***	-4.5963***
	[0.7378]	[0.7286]	[0.5841]	[0.8787]	[0.8776]	[0.8251]
Immigrant or descendant §	2.3343	1.7030	-0.4773	-1.2157	-1.9199	-3.8691
	[4.6113]	[4.6038]	[4.6089]	[5.6902]	[5.6558]	[5.7805]
Education: Short §	-7.9049**	-7.4980**	-5.2861*	-8.3544**	-8.0004**	-5.9660*
	[2.5945]	[2.6005]	[2.5332]	[3.0755]	[3.0696]	[3.0307]
Education: Medium §	-4.1586	-4.1945	-0.5923	-2.6179	-2.6790	-0.8679
	[2.7921]	[2.7907]	[2.7650]	[3.5501]	[3.5414]	[3.5229]
Education: Long §	-0.3184	-0.4927	1.5044	3.2506	3.0622	3.4247
	[3.1529]	[3.1491]	[3.0110]	[4.1774]	[4.1766]	[4.0852]
Voted for incumbent in last election		-4.3728*	-1.6476		-4.3336+	-1.4940
		[1.8963]	[1.8575]		[2.3182]	[2.3177]
Home ownership §			-3.8012+			-1.7218
			[2.0706]			[2.6819]
Unemployment rate, parish §			-0.5295			-0.3461
			[0.4242]			[0.5386]
Unemployment rate, occupation §			0.4906**			0.0529
			[0.1626]			[0.2074]
Share of year unemployed §			0.0720***			0.0681***
			[0.0167]			[0.0206]
Share of year unemployed, average			0.1166***			0.0983**
			[0.0312]			[0.0370]
Share of year unemployed, average			0.0624***			0.0640***
			[0.0143]			[0.0179]
Observations	4,812	4,812	4,744	3,380	3,380	3,327
Sample	Labor force	Labor force	Labor force	Excl FCT	Excl FCT	Excl FCT
Estimator	Tobit	Tobit	Tobit	Tobit	Tobit	Tobit

Robust standard errors corrected for clustering at the municipal level in brackets.

*** p < .001, ** p < .01, * p < .05, + p < .10.

A constant was included in all regressions, but is not reported.

Regressions were carried out in Stata 12.1 using tobit.

Sample "Excl FCT" excludes fiscal consolidation treatment sample, as in IV-regressions in Tables 2 and 3.