CREDIBLE SOURCES AND SOPHISTICATED VOTERS: WHEN DOES NEW INFORMATION INDUCE ECONOMIC VOTING?*

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When does new economic information cause voters to re-evaluate the government's competence, and ultimately vote economically? Since politically-relevant information is often conveyed by actors with incentives to influence voter perceptions, the credibility of information sources can vary significantly. This article randomly varies whether voters receive an aggregate unemployment forecast from the central bank, government or main opposition party using a survey experiment in Denmark with access to detailed panel data. We find that politically sophisticated voters discern differences in institutional credibility and the political cost of the signal, and update their unemployment expectations accordingly. Despite failing to differentiate political costs, unsophisticated voters still substantially update their expectations. However, while sophisticated voters intend to engage in substantial prospective economic voting, unsophisticated voters do not link their new unemployment expectations to their vote intention. These findings suggest that economic information supports economic voting most when it is credible and reaches sophisticated voters.

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Obtaining and processing politically-relevant information is an essential feature of how voters select governments and hold them to account (e.g. Manin, Przeworski and Stokes 1999). This is particularly true for economic voting, where aggregate economic information is critical for voters evaluating the competence of their government (Anderson 1995; Fearon 1999). Our goal is to examine the interaction between three key aspects of politically-relevant information: the credibility of its source, the ability of voters to recognize costly signals as more credible, and the extent to which voters translate their updated beliefs into political choices.

Since much of the information available to voters is biased,[1] economic information may only affect voter perceptions of economic performance when they regard it as credible. Even then, many voters lack the cognitive capacity to translate such perceptions into vote choices (Duch and Stevenson 2008; Gomez and Wilson 2001, 2006). Given that political actors devote significant time and money to such efforts, relatively incredible information could substantially affect economic voting if voters fail to discern differences in credibility.

In this article, we analyze the conditions under which providing aggregate unemployment forecasts causes different types of voters to re-evaluate the government's competence, and act politically on their beliefs by engaging in prospective economic voting. Our survey experiment, embedded in a rich Danish panel survey conducted in the aftermath of the financial crisis when macroeconomic concerns were the main political issue, focuses on the interaction between varying types and levels of information credibility and the political sophistication of voters. Like Gomez and Wilson (2006) and Luskin (1987), we regard political sophistication as both awareness of politically-relevant economic information and the cognitive ability to associate information and political choice. Using a novel context-specific measure, we define political sophistication by the accuracy of a voter's pre-treatment current unemployment estimate. We demonstrate that this measure is both highly correlated with standard proxies for political sophistication and is the key factor

[1]For example, Chiang and Knight (2011), Ladd and Lenz (2009), and Nadeau et al. (1999) document that voters do understand that sources of information may be biased.

even when such proxies are controlled for.

The existing literature primarily focuses on differences in credibility emanating from differences in institutional expertise or trust (see Gilens and Murakawa 1994; Lupia and McCubbins 1998; Mondak 1994). We incorporate these insights and also designed our treatments to capture the political incentives of an information source. Building on the logic of costly signals (Spence 1973), we argue that a message becomes more credible when the source has incentives to have stated otherwise.[2] Although sending such messages can be politically costly, Grose, Malhotra and Van Houweling (forthcoming) show that U.S. Senators often, and successfully, seek to explain to voters why their roll call votes deviate from constituent preferences. In our context, an opposition claim that the economy is doing well is more credible than an identical government claim because such a claim is costly since it may hurt the opposition's election prospects. Conversely, the government has a clear incentive to tell voters that the economy is performing well. To capture both institutional credibility and costly signals, we randomly assign voters to receive identical unemployment forecasts from either the Danish Central Bank (DCB), the government, or the main opposition party. We thus focus on prospective economic voting, where the selection motive for evaluating government competence is particularly sensitive to variation in the quality of performance signals (Ashworth 2012; Fearon 1999).

Models of prospective economic voting rely heavily on information affecting voter perceptions of incumbent competence and well-informed voters possessing the will and capacity to vote according to perceptions of economic competence (Healy and Malhotra 2013). Scholars of U.S. politics have argued that the most politically aware voters may respond least to such information because they already possess strong priors (e.g. Converse 1962; Zaller 1992, 2004). On the other hand, it may be that only such sophisticated voters possess the cognitive skills and political knowledge required to detect differences in source credibility and vote on the basis of this information. Since we examine a valence issue about which even the most aware voters are imperfectly in-

[2]In other political contexts, see Adolph (2014), Fearon (1997), or Gilligan and Krehbiel (1987).

formed, and given the complexity of the Danish political system, political sophistication may be necessary for voters to detect differences in source credibility and ultimately vote economically. In contrast with more ideological issues, where individual partisanship is likely to moderate voter beliefs and interpretations of the information that they receive (e.g. Druckman, Peterson and Slothuus 2013; Gaines et al. 2007; Jerit and Barabas 2012; Zaller 1992), voters are more likely to internalize information about valence issues like unemployment when they deem such information credible.

We first examine how the source and content of unemployment projections affect unemployment expectations. We find that all voters significantly update their unemployment expectations in response to our treatments. However, only sophisticated voters are able to differentiate between information sources. Among such voters, a DCB or opposition forecast that the economy is performing well reduces unemployment expectations significantly more than an equivalent government forecast, while a DCB or government forecast that the economy is performing badly increases unemployment expectations significantly more than an equivalent opposition forecast. While unsophisticated voters substantially downgrade their initially pessimistic expectations and regard the DCB as more credible, they fail to discern differences in message credibility across political parties. Previous vote choices, which are uncorrelated with political sophistication, do not differentially impact belief updating across different treatments.

Using our treatments to instrument for unemployment expectations, our instrumental variable (IV) estimates also show that new economic information translates into economic voting. A percentage point decrease in unemployment expectations increases the probability that the average complier intends to vote for Denmark's coalition government by 3.5 percentage points. This effect, which only affects the parties of the Prime Minister and Minister for the Economy and Interior, could have altered the outcome of Denmark's recent close elections. Providing further evidence of economic voting, lower unemployment expectations increase confidence in the government, but do not affect support for non-government left-wing parties or attitudes toward redistributive or unemployment insurance policies. Given the difficulty of identifying the effects of economic per-

formance on support for the government, our findings exploiting experimental variation in a large nationally representative sample provide strong causal evidence for economic voting.

However, economic voting is only induced among sophisticated voters. For sophisticated voters, a percentage point decrease in unemployment expectations increases the likelihood of voting for the government by 6 percentage points. Despite substantially updating their beliefs, unsophisticated voters do not translate lower unemployment expectations into support for the government. Since unsophisticated voters are disproportionately swing voters, who change their vote choices and vote intentions across time, this difference cannot be attributed to such voters being strong partisans. Similarly, differences in political preferences do not explain this difference.

Ultimately, our findings suggest that more sophisticated voters better understand the differing incentives of parties to send certain types of messages to voters, and update accordingly. Even though more sophisticated voters update less on average, given that their priors are more accurate, changes in their posterior beliefs are more important for vote choice. We thus conclude that economic information supports economic voting to the extent that it is credible and reaches sophisticated voters. Furthermore, this finding may explain why parties tend to target their messages at politically-engaged voters who may act upon the new information (Adams and Ezrow 2009; Gilens 2005).

1 When does new economic information spur economic voting?

The idea that governments may be rewarded or sanctioned by voters on the basis of their economic performance is well-established (see Anderson 2007; Lewis-Beck and Stegmaier 2000). Its logic is that voters impose sanctions retrospectively on the basis of economic outcomes to deter re-election seeking politicians from choosing suboptimal policies (Barro 1973; Ferejohn 1986), or prospectively use the available information to select the most competent candidate (Fearon 1999).[3]

[3]Voter motives could be either sociotropic or self-interested, or prospective or retrospective. As Ansolabehere, Meredith and Snowberg (2014) have shown, parsing out these effects is challenging. This is

Both backward- and forward-looking information can help to evaluate the competence of office-holders, but the presence of reliable information is essential (Manin, Przeworski and Stokes 1999).

To the extent that voting is economic, most studies conclude that it is macroeconomic "sociotropic" aggregates rather than individual-specific "pocketbook" calculations that drive this relationship (e.g. Kiewiet 1983; Lewis-Beck and Stegmaier 2000). Despite its appealing simplicity, the evidence that economic success translates into higher likelihoods of an incumbent being re-elected is mixed (Anderson 2007), and researchers have struggled to provide compelling evidence of a causal relationship (Healy and Malhotra 2013).

The conditions under which an individual votes economically can be demanding (Healy and Malhotra 2013). Specifically, prospective economic voting requires that voters obtain economic information, use credible information to evaluate the incumbent's economic competence, and re-elect sufficiently competent incumbents (compared to the alternatives). Even assuming that voters possess the necessary information, economic voting may still not occur if: (1) receiving new information does not affect voter appraisals; or (2) well-informed voters lack the motivation or cognitive capacity to link their vote to their appraisal. We focus on these two conditions, and thus examine when the provision of new economic information affects economic voting.

1.1 Source credibility

Most politically-relevant information is conveyed by agents with well-understood ideological biases and incentives to distort perceptions of the true state of the world (e.g. Baron 2006; Besley and Prat 2006; Gentzkow and Shapiro 2006; Huckfeldt 2001).[4] For example, Larcinese, Puglisi and Snyder (2011) have shown that pro-Democrat newspapers in the U.S. are more likely to report high unemployment under Republican Presidents. Accordingly, voters must evaluate the information they receive in terms of the credibility of the information source.

because the same information can be used for all such goals.

[4]Voters receiving biased information is also a demand side phenomenon (see Mullainathan and Shleifer 2005). We focus on supply by varying the sources voters are provided with.

A large literature in social and political psychology has argued that the perceived expertise and trustworthiness of an information source is a key determinant of whether a voter internalizes a message's content (e.g. Gilens and Murakawa 1994; Mondak 1994). In particular, when individuals do not seriously engage with the arguments they receive, Petty and Cacioppo (1981) theorize that "peripheral" persuasion may still occur if the source of the message is regarded as credible. Given the low day-to-day salience of politics for many voters, source cues are frequently relied upon by voters—especially when the information source is knowledgeable and trusted (Lupia and McCubbins 1998). Consequently, independent sources with context-specific expertise, such as independent central banks staffed by highly-trained economists and providing detailed technical data,[5] are more likely to affect voter beliefs than political parties.

While the importance of institutional credibility is relatively clear, the interaction between the source and content of a message has not been studied. We argue that an information source can also attain greater credibility if voters understand the source's incentives to send a particular message. If the credibility of a signal increases with the perceived cost to the sender, a message becomes more credible when the source has incentives to have stated otherwise. In Spence's (1973) seminal example of a costly signal, the level of education an individual attains can only signal high ability to employers if the cost of such education is too large for a low ability worker to attain. Similarly, Chiang and Knight (2011) and Ladd and Lenz (2009) find that "surprising" newspaper endorsements disproportionately affect vote intentions.

The role of costly signals is particularly clear in the case of election-motivated political parties talking about the economy. Governments have strong incentives to play up their competence at dealing with the economy, and often point to effective policies and macroeconomic forecasts to support their claims. Conversely, opposition parties typically emphasize government failures and argue that they would instead do a better job. Consequently, voters that recognize that opposition

[5]Tetlock (2010) describes the demand for expert information. Barro and Gordon (1983) describe how independent central banks can credibly solve politicians' commitment problems.

politicians face strong incentives to downgrade the government's economic performance should regard an opposition claim that the government is managing the economy well as more credible than an identical government claim. Similarly, government statements pointing to poor economic performance are more credible than identical opposition claims. Our treatments exploit this logic.

1.2 Voter sophistication

Which types of voters are able to link political context to the cost and credibility of providing certain types of information, and update accordingly? An influential literature has argued that sophisticated voters—those that are both politically informed and possess the cognitive skills required to evaluate the information they receive—are least sensitive to politically-relevant information because they possess strong priors on political issues due to strong partisan biases or because they are already well informed on the issue (e.g. Converse 1962; Lodge and Taber 2013; Zaller 1992, 2004). Recent studies in the U.S. show that a voter's partisan biases affect their willingness to internalize new information (e.g. Jerit and Barabas 2012; Boudreau and MacKenzie 2014; Bullock 2011; Gerber and Huber 2010), while European studies have indicated that poorly informed voters are most sensitive to new information (e.g. Duch 2001). When faced with credible information, to the extent that such voters can discern it, we might therefore expect the least sophisticated voters to update their beliefs most. Provided such beliefs are internalized, this could translate into significant changes in political behavior.

However, this account ignores the possibility that differences in the source and content of messages—and thus their credibility—may not be perceived equally by sophisticated and unsophisticated voters. Although unsophisticated voters may be especially susceptible to new information because they are politically unaware, this lack of awareness—in conjunction with lower cognitive capacity—may prevent such voters from evaluating a source's credibility. As Duch and Stevenson (2008) and Gomez and Wilson (2001, 2006) have shown, less educated and politically informed voters struggle to detect subtle factors that are relevant for attributing government re-

sponsibility for economic performance. Similarly, voters vary in their ability to differentiate subtle differences in source credibility. Due to a relatively strong understanding of the parties in and out of office and their incentives to win office at the next election (e.g. Prior 2013), sophisticated voters are more likely to recognize differences in source credibility and update their beliefs accordingly. On the other hand, unsophisticated voters may fail to grasp differences in the costliness of different messages and even fail to differentiate expert from non-expert sources.

These differences may be particularly pronounced for valence issues containing factual information. Since there is consensus among voters that lower unemployment, for example, is regarded as good (Slothuus and De Vreese 2010), credible information is likely to play a key role in changing the beliefs of all voters. For such valence issues, the interpretation of numeric information through a partisan lens is likely to be less salient (Gerber and Green 1998). In contrast, partisan biases may be more important for ideologically-charged issues such as immigration (Druckman, Peterson and Slothuus 2013) or welfare policy (Slothuus and De Vreese 2010), where there is also greater scope for a disjuncture between fact-based beliefs and interpretations (Gaines et al. 2007).

Furthermore, even when economic information does affect politically unsophisticated voters, it is not clear that their political behavior will change. First, models of survey responses argue that such voters simply sample from recent pieces of information without considering their political implications (Zaller 1992). However, while more sophisticated voters may update their beliefs less, their posterior beliefs regarding government competence are more important for their vote choice (Delli Carpini and Keeter 1996). Second, even if unsophisticated voters do internalize new information, they may lack the cognitive capacity to translate it into political action (Gomez and Wilson 2006). This is particularly true when assigning responsibility in institutional contexts characterized by an open economy and multiple loci of decision-making power or coalitions governments, where even the most willing economic voter may struggle to assign responsibility for economic performance (e.g. Anderson 1995; Duch and Stevenson 2008; Powell Jr. and Whitten 1993). Together, these considerations imply that even if unsophisticated voters receive politically relevant

information, it may not affect their political behavior.[6]

2 Research design

We examine the political effects of providing politically-relevant economic information in Denmark, a country where left-right differences over economic policy remain the salient political division and governments have oscillated between center-left and center-right coalitions. In 2011, Social Democrat Helle Thorning-Schmidt became Denmark's first female Prime Minister, having narrowly led the left bloc—containing the Social Democratic, Social Liberal and Socialist People's parties as coalition partners, and supported by the Red-Green Alliance—to victory over a center-right coalition led by the Liberals that had held office since 2001.

Dissatisfaction with the government's economic performance was the major issue in the 2011 election (Stubager 2012).[7] Having sustained very low levels of aggregate (gross) unemployment throughout the 2000s, reaching nearly 2% in early 2008, unemployment almost trebled to around 6% by the 2011 election.[8] Sharp increases in the budget deficit also left Denmark with hard fiscal choices regarding welfare and pension reform. The center-right's austerity policies were widely blamed for the failure to produce a stronger economic recovery.[9] Despite this, the left bloc only barely achieved a parliamentary majority in 2011; the seat distribution in Denmark's legislative assembly is shown in the Online Appendix. The shift in political power primarily reflected the rise

[6]Alternatively, although unsophisticated voters are typically poorer, it is also possible that such voters care about orthogonal policy issues. We test this alternative explanation empirically.

[7]The Danish Election Study polls, available here, show that the economy was the most importance issue for voters, while nearly 20% specifically cited unemployment. The study also shows that left-wing voters thought the labor market was the biggest issue, while right-wing voters thought the economy in general was the biggest issue. Voters similarly divided over whether a left or right coalition would best fight unemployment.

[8]Gross unemployment is the definition used by the government and Statistics Denmark (and reported in the media), and is based on administrative records. Net unemployment, which excludes those in active labor market programs, is around one percentage point lower.

[9]Although Denmark's government did not cause the financial crisis, governments are often held responsible for exogenous shocks (Duch and Stevenson 2008), or for failing to respond effectively.

of the Social Liberals at the expense of the Conservatives.

The Danish economy struggled to improve after the 2011 election, and unemployment rates became more politically salient. In January 2013, unemployment reached 6.5%. Importantly for our study, the DCB estimated that this rate could rise to just below 7% by January 2014.[10] The share of Danes regarding unemployment as the biggest political problem rose from 18% at the 2011 election to 20% by November 2012, and 36% by late 2013.[11] Moreover, within-coalition tensions between the economically liberal Social Liberals and the socialist Socialist People's parties increased, and culminated with the Socialist People's Party leaving the coalition in January 2014 over unpopular plans to privatize the country's state-owned energy company.

2.1 Data and experimental design

To evaluate the conditions under which economic information with varying levels of credibility affects individual beliefs and economic voting, we embedded a survey experiment in the 2013 wave of the Danish Panel Study of Income and Asset Expectations, an annual panel survey of around 6,000 working age Danes conducted every January/February.[12] The panel, conducted by telephone since 2010, asks about the respondent's financial position, behavioral dispositions and political preferences. Furthermore, the survey data has been linked by Statistics Denmark to the Danish Central Person Registry, a rich administrative dataset containing wide-ranging government information about all Danes. The combination of panel political data and detailed respondent histories allows us to describe differential responses to politically-relevant information in detail.

Treatments. We examine source credibility by varying the source of simple unemployment forecasts, as well as the forecast itself. Respondents were randomly assigned to one of eight

[10]The Online Appendix provides additional detail about DCB forecasts.

[11]See the DR Nyheder November 2012 poll here, and Jyllands-Posten December 2013 poll here.

[12]The first wave randomly chose c.6,000 working age respondents from the Central Person Registry. Annual attrition is around 20%. The sample has been replenished with respondents randomly chosen from the Registry, and remains representative of the working age population. The final data set made available for research was anonymized.

different groups with around 700 members each. The control group received no information, while six treated groups were read the following statement:

> "Assume that that the [DCB/government/Liberals] estimates that unemployment in 2013 will be [almost 7%/around 5%]."[13]

Respondents were therefore informed that the DCB, the government or main opposition party forecast that unemployment over the next year will be "almost 7%" or "around 5%". As noted above, the true DCB forecast for gross unemployment was almost 7%. However, because only the DCB has publicly stated this, ethical considerations required that our other primes begin with "assume that...". In order to examine the extent to which such wording weakens the treatment, our final treatment group was truthfully told "The DCB estimates unemployment in 2013 to be almost 7%." We compare this treatment to the analogous "assume" version, and will show no statistical difference in the distribution of unemployment expectations.

Unemployment projections, typically one and two years in the future, from both the DCB and the government are frequently communicated in print and electronic media. This information is communicated either in the form of predicted (gross) unemployment percentages or as the predicted number of full-time equivalent unemployed. News reporting of such projections often, if not always, notes the direction of the change relative to current unemployment levels.[14] This last feature is implicit in our measure, where subjects are themselves first asked to state their own belief about the current unemployment rate before being treated.

These sources vary considerably in their credibility among voters of all political stripes. The DCB is highly regarded by voters, and is not perceived as right-wing or an instrument of government, while political parties are viewed with skepticism. Among our control group, 67% of respondents trusted or greatly trusted the DCB while only 17% and 27% trusted or greatly trusted

[13] Survey treatments and questions are translated from Danish; see Online Appendix for Danish phrasing. It is important to emphasize that in Danish the prime translates as a prospective estimate.

[14] We provide links to two examples from two major newspapers: Berlingske and Politiken.

the government and Liberals respectively.[15] Eurobarometer data indicates that trust in Denmark's political parties is very similar to the European Union mean (European Commission 2011).

Outcome variables. We consider two types of outcomes: unemployment expectations and economic voting. To capture unemployment expectations we asked respondents "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."[16] This question was asked immediately after respondents received their treatment, and the 20 respondents who answered that the unemployment rate would exceed 50% were removed.[17] Unlike more partisan issues, Lenz (2012) finds no evidence of reactivity biases for valence issues like unemployment. Summary statistics are provided in the Online Appendix.

Economic voting is measured by vote intention and evaluations of the government, although we also consider various placebo tests examining other outcomes. We code indicator variables for intending to vote for Denmark's main political parties, as well as groups for the governing coalition (Social Democrats, Social Liberals and Socialist People's parties) and right-wing parties. To reduce concerns about experimental demand biases, vote intention was elicited 18 questions—10-20 minutes later, after detours through unrelated questions—after the treatment was administered. Because turnout in Denmark regularly exceeds 85%,[18] and 72% of respondents ultimately reported voting for the party they intended to vote for eight months prior to the 2011 election, vote intention represents a good approximation for what would happen if an election was held immediately. To assess voter perceptions of government competence, we asked respondents how much confidence they have in the government. Respondents were provided a five-point scale ranging from little great mistrust (1) to great trust (5) in the government.[19]

[15]These numbers are in line with mass surveys conducted by Statistics Denmark: in 2011, they found that while 82% trusted the DCB, only 59% trusted Parliament. See report summary here.

[16]From a Bayesian perspective (see the Online Appendix), this response can be thought of as an individual's posterior unemployment belief (updated after receiving new information).

[17]These individuals were very evenly spread across treatment conditions, with between 2 and 4 omitted respondents in each group. Including these observations does not affect the results.

[18]See the Institute for Democracy and Electoral Assistance.

[19]This question was asked 11 questions after the treatment was administered.

Political sophistication. To capture both political awareness and cognitive skills, political sophistication is measured by a respondent's estimate of the current unemployment rate. We operationalize a sophisticated voter as one whose (pre-treatment) current unemployment estimate is within 1.5 percentage points of the true 6.5% level. We thus count around half the sample as sophisticated, while 70% describe themselves as well informed about Danish politics.[20] Our measure not only accurately captures awareness of politically-relevant economic information, but the Online Appendix shows that it also represents a "sufficient statistic" for political engagement and cognitive skills in two important respects. First, the absolute difference between the current unemployment rate and the respondent's estimate is significantly negatively correlated with frequency of watching the news, regular discussion of politics, income, education, the number of correct answers on a math test, and a respondent's self-reported level of political information. Second, our measure of political sophistication captures the effect of other measures of political sophistication on unemployment expectations. In particular, the Online Appendix shows that when we interact the absolute difference between the respondent's estimate and the true unemployment rate with our treatments, the previously-significant baseline effects of standard proxies for political sophistication, and their interactions with our treatments, all cease to be statistically significant. In addition, we show in the Online Appendix that our measure of political sophistication is uncorrelated with measures of partisanship based on previous vote choice.

2.2 Identification and estimation

Given its random assignment, treatment status is well balanced across pre-treatment covariates. The Online Appendix confirms such balance across 16 political and socioeconomic variables frequently included in observational studies regressing political preferences on a set of covariates. Our empirical analysis can now straight-forwardly identify the causal effects of the treatments.

[20] We obtain very similar results using other cutoffs such as guessing within one or two percentage points, or when using the absolute deviation from the true current unemployment estimate.

To estimate the average treatment effect on the treated of each information treatment on unemployment expectations, we estimate the following equation using ordinary least squares (OLS):

$$\textit{Unemployment expectation}_i = Z_i\alpha + \varepsilon_i, \tag{1}$$

where α represents the effect of each treatment contained in our vector of treatment conditions, Z_i. To examine how the effects of our treatments vary across sophisticated and unsophisticated voters, we split our sample and estimate the effects for different types of voter separately.[21] Robust standard errors are reported throughout.

To identify our ultimate quantity of interest—the effect of unemployment expectations on economic voting—we instrument for unemployment expectations using our information treatments. By exploiting only random variation induced by our randomly assigned treatments, this IV strategy overcomes the concern that economic expectations may be correlated with omitted variables that also affect political preferences. Since voters with different prior beliefs about the unemployment rate may update their beliefs in different directions in response to the same treatment, a benefit of this approach is the ability to scale the reduced form effect of each treatment by its impact upon the average voter's unemployment expectations. This means that the most credible sources will be weighted more heavily. Using equation (1) as the first stage, we thus estimate the average causal effect among compliers—individuals for whom our information treatments induced respondents to change their unemployment expectations—across different unemployment expectation levels.[22] Accordingly, we estimate the following equation using two stage least squares (2SLS):

$$Y_i = \tau\textit{Unemployment expectation}_i + \delta\textit{Current unemployment estimate}_i + \xi_i, \tag{2}$$

[21] We obtain very similar results when interacting our treatments with voter sophistication, but split the sample to simplify interpretation when comparing many treatment effects across groups.

[22] More formally, we estimate the local average causal response—the linearized causal effect of unemployment expectations, weighted toward areas where the density function of complier responses is greatest (Angrist and Imbens 1995).

where Y_i is vote intention, confidence in the government, or a placebo outcome. We include the respondent's pre-treatment estimate of the current unemployment rate to enhance efficiency, although the Online Appendix shows that this choice does not affect our results. We again examine heterogeneity using subsamples.

The key additional assumption underpinning the IV estimates is the exclusion restriction. This requires that our instruments only affect our outcomes through unemployment expectations.[23] Perhaps the most plausible risk of violating this assumption arises where information treatments prime respondents to think more carefully about government performance and policies (beyond the effect of changing beliefs about unemployment expectations), inducing bias if such thinking systematically affects support for the government. We assess this possibility by looking at whether belief in the importance of political information for either private economic decisions or as part of the respondent's job differs across treatment groups (or comparing the control to all treated respondents), and find no difference (see the Online Appendix).

3 Effects of information source on unemployment expectations

We first examine how our information treatments affect the unemployment expectations of the average voter. Addressing our main hypothesis, we then show that the average effects mask the key role played by voter sophistication in explaining systematic differences in how different types of voters respond to economic information with varying levels of credibility.

3.1 Belief updating on average

Figure 1 plots the distribution of unemployment expectation responses by treatment condition. Before turning to our main results, it is clear from Panel A that the "assume" wording does not

[23] Although some respondents update in different directions in response to our treatments, the discussion in the Online Appendix shows that the monotonicity assumption is unproblematic.

affect the distribution of the DCB 7% projection responses.[24] This suggests that the statement wording is not biasing voter responses. Henceforth we pool the DCB 7% treatment groups.[25]

The leftward shift in density associated with all treatments indicates that all information sources reduce unemployment expectations on average across respondents. The reduction reflects systematic pessimism in a population where the mean control group member expected an unemployment rate of 9.0%. Despite its optimism relative to the true DCB claim, the 5% treatments dragged expectations below the 7% treatment groups. In all cases, the information treatments reduced the variance of the distributions, providing further evidence that the treatments affected respondents.[26]

Consistent with previous findings regarding differences in credibility due to higher trust and greater expertise, receiving information from political parties caused the average voter to update their beliefs less than receiving information from the DCB. The DCB treatments also induced more similar responses from voters (i.e. a smaller standard deviation in responses), especially compared to the opposition treatments. Although it could have been the case that simply being primed by a source increased confidence in the source, the Online Appendix shows that receiving a treatment does not affect trust in either political party.

The government and opposition source treatments also reduced unemployment expectations. Panel B clearly shows a downward shift in modal unemployment expectations for both treatments. Surprisingly, given that the opposition has a political incentive to criticize government economic performance, the Liberal party's projections did not cause voters to differentially change their beliefs relative to the predictably optimistic government message. Averaging across the full sample, we therefore find no evidence that voters are sensitive to costly signals.

Estimating equation (1), Column (1) in Table 1 confirms our graphical analysis. Receiving

[24]Tests comparing the mean and variance of the distributions cannot reject the null hypothesis of identical sample moments.

[25]This similarity may not extend to other treatments; however, any bias is likely to be downward.

[26]Distributional tests confirm that the variance reduction is statistically significant. Although these belief shifts could in part reflect anchoring biases (Tversky and Kahneman 1974), it is hard to see how such explanations could explain the changes in economic voting we document below.

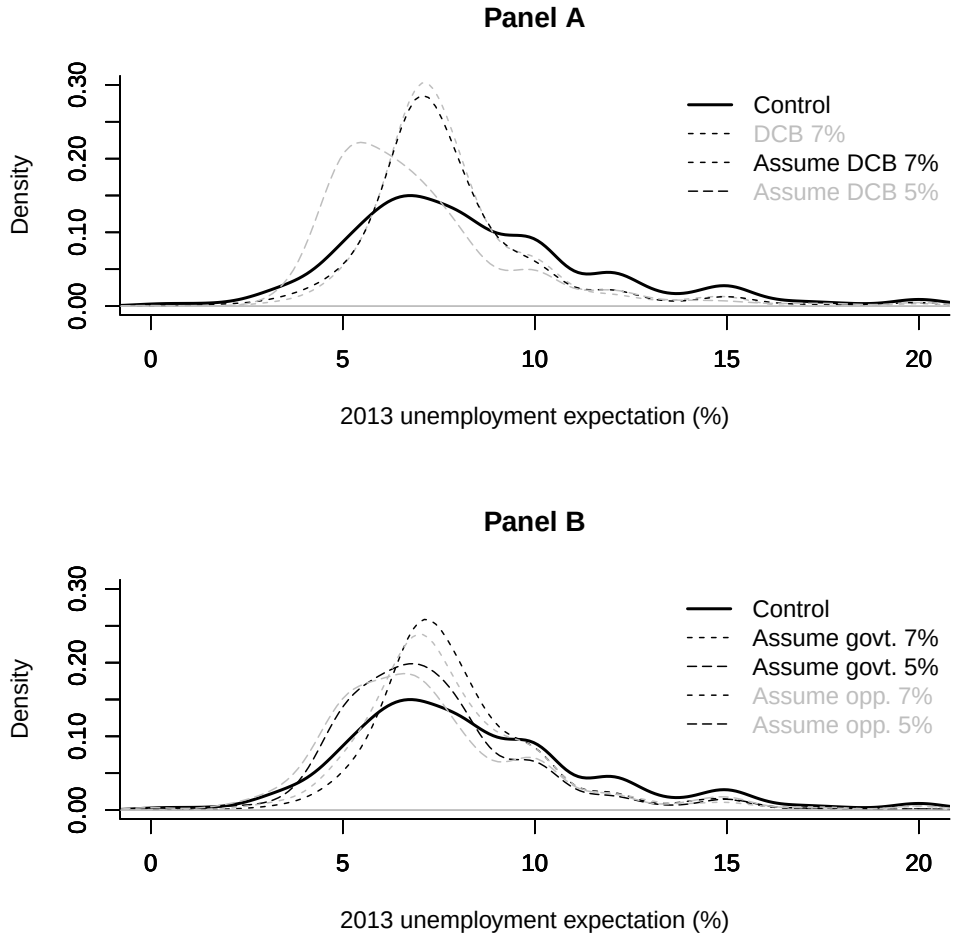

Figure 1: Unemployment expectations by DCB treatments

Note: For graphical exposition, the x-axis is truncated so that the 1% of the sample with expectations above 20% are not visible.

Table 1: Effect of information treatments on unemployment expectations (%)

	Outcome: unemployment expectations		
	(1) Full sample	(2) Unsophist. voters	(3) Sophist. voters
Control	9.012***	11.461***	7.070***
	(0.185)	(0.362)	(0.078)
DCB 7% treatment (combined)	-1.123***	-2.498***	-0.031
	(0.197)	(0.387)	(0.084)
Government 7% treatment	-0.848***	-1.876***	0.066
	(0.213)	(0.421)	(0.096)
Opposition 7% treatment	-0.923***	-1.801***	-0.226**
	(0.223)	(0.435)	(0.098)
DCB 5% treatment	-1.663***	-2.391***	-0.907***
	(0.230)	(0.470)	(0.098)
Government 5% treatment	-1.218***	-2.294***	-0.621***
	(0.233)	(0.446)	(0.098)
Opposition 5% treatment	-1.335***	-2.194***	-0.781***
	(0.236)	(0.458)	(0.104)
Coefficient equality F tests (p values)			
DCB 7% = Government 7%	0.08*	0.02**	0.13
DCB 7% = Opposition 7%	0.16	0.01**	0.00***
Government 7% = Opposition 7%	0.65	0.82	0.00***
DCB 5% = Government 5%	0.02**	0.81	0.00***
DCB 5% = Opposition 5%	0.10	0.63	0.16
Government 5% = Opposition 5%	0.57	0.79	0.08*
Observations	5,705	2,533	3,172
Outcome mean	7.98	9.51	6.76
Outcome std. dev.	3.55	4.72	1.24
Current unemployment estimate mean	8.58	10.97	6.67

Notes: The dependent variable is a respondent's unemployment expectation for the end of 2013 (%). All specifications are estimated using OLS, with robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$. The coefficient tests at the foot of the table report the p value from a two-sided F test of coefficient equality. Sophisticated voters are defined as respondents whose current unemployment estimate is within 1.5 percentage points of the true rate in January 2013 (see main text for further details).

a 7% treatment reduces unemployment expectations by around 1 percentage point, while a 5% treatment subtracts a further 0.5 percentage points. The tests of the differences between treatment effects at the foot of the table show that, at both forecast levels, the DCB's claim has a significantly larger impact on unemployment expectations than the government's, while the difference between the DCB and the opposition is almost statistically significant. There is no discernible difference between the government and opposition 5% or 7% treatments. However, we now demonstrate that averaging across all respondents hides important differences by voter sophistication.

3.2 Voter sophistication and differences in source credibility

Respondents saw credibility differences between the DCB and political parties on average. However, our main argument is that politically sophisticated and unsophisticated voters respond differently. We explore this possibility in columns (2) and (3) of Table 1 by respectively splitting the sample between unsophisticated and sophisticated voters. The results show that sophisticated and unsophisticated voters indeed respond very differently to unemployment forecasts.

Politically unsophisticated voters regard the DCB as more credible, but do not differentiate between political sources. Our estimates in column (2) show that the DCB treatments substantially reduced unemployment expectations among unsophisticated voters. The first and second coefficient equality tests at the foot of the column indicate that the DCB 7% treatment reduced expectations significantly more than the 7% treatment from either political party. The final three coefficient tests, however, indicate that the 5% treatments are statistically indistinguishable. Furthermore, the almost identical coefficients for different political parties—in the third and sixth coefficient tests—clearly demonstrate that unsophisticated voters do not detect differences in the incentives of political parties to send the messages that they receive.

In contrast to unsophisticated voters, sophisticated voters systematically perceive significant differences in source credibility. In particular, only sophisticated voters differentially update in accordance with the incentives for each party to claim that the economy is doing well. Relative to the

7.1% average forecast among sophisticated voters in the control group, sophisticated voters discount positive economic appraisals by the government and emphasize positive economic appraisals by the opposition. For the 7% treatments, the opposition claim that the government is performing slightly better than sophisticated voters previously believed causes voters to significantly reduce their unemployment expectations. The second and third tests at the foot of column (3) show that receiving this message from the opposition significantly reduces unemployment expectations relative to the DCB and government 7% treatments. This finding accords with our theoretical expectation that voters are more likely to regard the economy as doing better than previously-believed after receiving a claim from a source with incentives to claim otherwise. This finding suggests that voters may even overshoot the specific opposition forecast, potentially believing that the opposition is still under-stating economic performance, while high trust of the DCB does not cause voters to deviate from their prior when it is confirmed by the DCB. Among sophisticated voters with current unemployment estimates between 5% and 7%, and who are thus expected to increase their expectations in response to the 7% treatment, the converse relationship also holds: the Online Appendix shows that receiving a pessimistic forecast (relative to their current perspective) from the DCB or government is more credible than from the opposition. Turning to the 5% treatments, the fourth and sixth coefficient tests similarly show that the DCB and opposition sources cause significantly larger reductions in unemployment expectations than the government source. In this more positive outlook, we also cannot reject the possibility that voters regard the DCB's and opposition's forecasts as equally credible. These results support our claim that sophisticated voters are indeed particularly sensitive to the source of new economic information.

Although they fail to systematically discern differences in source credibility, unsophisticated voters still substantially alter their unemployment expectations. Since the average unsophisticated voter believes the current unemployment rate is 11.0% and expects the rate to reach 11.5% at the end of 2013 (in the control group), the large reductions after receiving either forecast are consistent with previous research emphasizing the malleability of the least informed (e.g. Converse 1962; Za-

ller 1992, 2004). However, contrary to such theories, we find that sophisticated voters also update their beliefs after receiving new economic information. Column (3) shows that all treatments except the DCB and government 7% projections significantly alter the unemployment expectations of sophisticated voters. The lack of effect for these two treatments reflects the prior of 7.1% (in the control group) hardly deviating from the 7% treatment. Furthermore, relative to the difference between their current unemployment estimate and the treatment projection, sophisticated voters proportionately change their beliefs as much as unsophisticated voters. The next section examines whether these changes in stated beliefs are sufficiently important to translate into vote intentions.

3.3 Alternative interpretations

A key concern that could potentially undermine the interpretation of our findings is that differences in sophistication actually reflect differences in partisanship. Although analyses in European contexts have generally found policy preferences to more strongly drive partisan choices (e.g. Adams 2012; Budge, Crewe and Farlie 1976), an influential literature—based primarily on studies from the U.S.—has found that differential updating is strongly moderated by partisanship (e.g. Boudreau and MacKenzie 2014; Bullock 2011; Healy and Malhotra 2013). However, contrary to the expectation that partisanship would reduce voter responses to the treatments, we find that sophisticated voters are more sensitive to source credibility. Furthermore, in addition to our measure of political sophistication being balanced across measures of previous vote choice, we also find no evidence of differential updating by political allegiance: the Online Appendix demonstrates that respondents who voted for a government (right) party at the 2011 election did not differentially update their beliefs when provided with information from the government (opposition). We similarly found no difference when defining left and right-wing supporters as respondents who voted left or right party in the 2007 election. Furthermore, we find no evidence of a more complex conditional relationship: the Online Appendix shows that even within sophisticated and unsophisticated groups of voters, there are no differential responses to treatments by past partisanship.

Although, as noted above, our measure of voter sophistication is highly correlated with measures used in previous studies, and serves as a sufficient statistic for such alternative measures in the context of updating economic expectations, we nevertheless examine the robustness of this measure. We compute a summative scale containing commonly used (standardized) indicators of sophistication—namely education, frequency of watching the news, frequency of discussing politics, self-identification as politically informed, and (log) wages—and divided respondents above and below the median scale score. In the Online Appendix, we show broadly similar differences by this measure of sophistication. For both the 5% and 7% treatments, sophisticated voters update significantly more when the information is provided by the DCB than political parties, while the opposition but not the government 5% claim is statistically significant. Conversely, unsophisticated voters do not differentially update across sources. Unsurprisingly, given the contextual relevance of our measure and its predictive power vis-á-vis standard measures, these estimates are less precise than our preferred measure.

4 Effects of unemployment expectations on economic voting

The preceding analysis has shown that information about aggregate unemployment projections affects the expectations of all Danes regarding the economy's prospects. While sophisticated respondents were sensitive to differences in source credibility, unsophisticated voters updated their beliefs more indiscriminately. We now examine the implications for economic voting. We first ask whether lower unemployment expectations increase the likelihood that respondents would vote for the government. Of particular importance for standard political economy models of vote choice, but also strategists determining a party's public statements, we then ask which types of voters vote according to their unemployment expectations. Finally, we examine mechanisms to check our economic voting interpretation of the evidence.

4.1 Evidence of economic voting on average

Table 2 reports estimates of equation (2), identifying the average effect of a percentage point increase in unemployment expectations on vote intention among individuals whose expectations are affected by the instruments. The outcomes in columns (1)-(6) are indicators for supporting a particular party or group of parties. The large F statistic confirms a strong first stage (see the Online Appendix for point estimates).

Providing strong evidence for economic voting, the exogenous provision of economic information causes voters to substantially alter their vote intention. Column (1) finds that a percentage point decrease in unemployment expectations causes the average complier to increase their support for the parties of government by 3.5 percentage points.[27] Increased government support is almost exactly mirrored by the decrease in support for right-wing parties in column (5), with the majority of votes coming from the main right-wing Liberal party shown in column (6). In the context of coalition politics, proportional representation, and especially the extremely close recent Danish elections, information about aggregate unemployment could easily have altered the composition of government. Even by the standards of countries with greater clarity of responsibility, the effect is very substantial—in spite of vote intention being asked 18 questions after the treatment.

While the allocation of credit and blame for the economy's progress is usually relatively clear when there is a single-party government, voter sanctioning is not obvious among coalition partners (Anderson 1995; Duch and Stevenson 2008). Columns (2)-(4) disaggregate the government vote share by the three parties in the governing coalition. The results clearly indicate that the two largest coalition partners—the Social Democrats and the Social Liberal Party, who had 44 and 17 seats and 10 and 6 cabinet positions respectively—are the sole beneficiaries, both gaining 1.6 percentage point increases in the probability of a respondent voting for them for each percentage point decrease in unemployment expectations. This represents a relatively larger gain for the

[27]The reduced form estimates show similar results in the Online Appendix.

Table 2: Effect of unemployment expectations on vote intention

	(1) Govt.	(2) Soc. Dem.	(3) Soc. Lib.	(4) Soc. Peop.	(5) Right	(6) Liberals
Unemployment expectations (%)	-0.035**	-0.016	-0.016*	-0.003	0.034**	0.024*
	(0.014)	(0.011)	(0.009)	(0.007)	(0.015)	(0.014)
First stage F statistic	32.6	32.6	32.6	32.6	32.6	32.6
Observations	5,705	5,705	5,705	5,705	5,705	5,705
Outcome mean	0.32	0.17	0.09	0.06	0.41	0.28
Unem. exp. mean	7.98	7.98	7.98	7.98	7.98	7.98
Unem. exp. std. dev.	3.55	3.55	3.55	3.55	3.55	3.55

Notes: The dependent variables are indicators for intending to vote for (1) a party in the governing coalition, (2) the Social Democratic Party, (3) the Social Liberal Party, (4) the Socialist People's Party, (5) any right-wing party, and (6) the Liberal Party. All specifications are estimated using 2SLS, and control for current unemployment expectations. Robust standard errors are provided in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

smaller Social Liberal party. In line with the findings of Anderson (1995) and Duch, Przepiorka and Stevenson (N.d.), responsibility is assigned to the parties with greatest control over economic policy: while the Social Democrats led the coalition and secured the Premiership, the leader of the Social Liberals—who campaigned on their centrist economic agenda—became Minister for the Economy and Interior. The intended vote share of the more extreme left-wing Socialist People's Party, which held 16 seats and 6 cabinet positions, is essentially unaffected.

4.2 Voter sophistication and the capacity to vote economically

While unemployment expectations significantly affect respondent vote intention on average, it is not clear that unemployment expectations equally affect the political preferences of all voters. We examine the heterogeneous effects—primarily by voter sophistication—of unemployment expectations on intending to vote for a party in the governing coalition in Table 3 by splitting our sample. Despite substantially altering unemployment expectations among unsophisticated voters, our estimates provide clear evidence that economic information only induces economic voting among

Table 3: Effect of unemployment expectations on vote intention, by voter sophistication

	Outcome: intend to vote for the government					
	(1) Unsophist. voters	(2) Sophist. voters	(3) Non-swing voters	(4) Swing voters	(5) Keep immig. benefits	(6) Lower immig. benefits
Unemployment expectations (%)	-0.016	-0.061***	-0.050**	-0.036	-0.028*	-0.035
	(0.013)	(0.024)	(0.023)	(0.023)	(0.016)	(0.027)
Observations	2,533	3,172	2,173	1,654	4,303	1,402
First stage F statistic	20.3	66.6	13.4	13.1	23.4	12.7
Outcome mean	0.30	0.34	0.37	0.26	0.27	0.47
Unem. exp. mean	9.51	6.76	7.91	7.91	8.01	7.90
Unem. exp. std. dev.	4.72	1.24	3.57	3.47	3.51	3.69

Notes: The dependent variable in all specifications is an indicator for voting for a party in the governing coalition. The head of each column defines the subset of respondents that each specification was estimated for. All specifications are estimated using 2SLS, and control for current unemployment expectations to increase efficiency. Robust standard errors are provided in parentheses. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

sophisticated voters.

The small and statistically insignificant coefficient in column (1) indicates that unemployment expectations do not substantially affect vote choice among unsophisticated voters. Thus, although those with the least accurate beliefs about current unemployment both receive and accept information about unemployment, this does not translate into vote intentions.

Conversely, column (2) reports a large effect among sophisticated voters: a percentage point decrease in unemployment expectations increases the probability that a respondent intends to vote for a government party by six percentage points. To demonstrate the robustness of these differences by sophistication, the Online Appendix also shows substantively similar results when using our alternative measure of sophistication combining commonly popular measures of political sophistication. Since, as noted above, sophistication is uncorrelated with measures of partisanship,

the differential voting responses by sophistication are not being driven by partisan affiliation.[28]

Together, these findings highlight an important limit on the provision of political information: only a subset of those who update their beliefs translate such beliefs into actions, and those who update the most are not necessarily most likely to vote economically. In conjunction with our earlier finding that only sophisticated voters are able to discern subtle differences in source credibility, the results suggest that politically unsophisticated voters lack the will or the capacity to vote according to their evaluations of government competence. To better differentiate a lack of will from a lack of capacity, we investigate whether unsophisticated voters are disproportionately partisan or simply care about other policy issues.

Some standard political economy models suggest an alternative interpretation of our results, that non-partisan "swing" voters are the most likely to transfer their votes to a party on the basis of competence (e.g. Lindbeck and Weibull 1987). If our sophisticated voters are swing voters, this would explain our results. We test this by exploiting the panel structure of the dataset to define an indicator for the 43% of respondents who reported voting for different parties at the 2007 and 2011 elections.[29] Columns (3)-(4) of Table 3 demonstrate that such swing voters are not driving changes in government support. Rather, the effect of unemployment expectations among swing voters, which is identical across swing voter definitions, is statistically indistinguishable from zero.[30] Despite the fact that they do not fail to update their expectations, the Online Appendix shows that Denmark's swing voters—who discuss politics less, are less educated and have lower math test scores—are characterized by low political sophistication. These differences are also

[28]The Online Appendix also demonstrates that voters that voted for a left party are not more likely to support the leftist governing coalition when their unemployment expectations are lower.

[29]The Online Appendix shows that the results are robust to instead using an indicator for the 23% whose 2011 vote differed from their 2012 vote intentions.

[30]Given the first stage for swing voters is especially strong, this result does not reflect swing voters failing to update their unemployment expectations. To ensure our definition of swing voters is not picking up shifts to parties offering similar platforms, we also calculated measures for left and right party groupings and examined swings to the left and swing to the right and in each case found similar results. The results are similarly robust to defining swing voters as individuals whose 2011 and 2012 survey vote intentions differed.

likely to be compounded by the complexity of assigning responsibility over economic policy to different parties in Denmark's PR electoral system defined by coalition governments, many parties and unstable alliances in the political center (Anderson 1995; Powell Jr. and Whitten 1993).

A second alternative explanation for swing and less sophisticated voters not engaging in economic voting is that economic competence is not a salient issue among these voters (e.g. Shayo 2009). Rising immigration in Denmark has become a second political cleavage in recent years, so it is possible that such voters are instead principally concerned with this issue. However, voter opinions and contextual data do not support this possibility. Columns (5) and (6) in Table 3 show that economic voting is similarly prevalent across those supporting and opposing the reinstatement of separate and lower state benefits for immigrants.[31] Furthermore, we show in the Online Appendix that there are no differences by parish (or municipality) immigrant share.

4.3 Mechanisms

The key theoretical claim underpinning economic voting is that, conditional on receiving credible information, unemployment expectations affect vote choice through voter perceptions of government competence. We test this mechanism in column (1) of Table 4 by examining the effect of unemployment expectations on respondent confidence in the government. The results show that lower unemployment expectations significantly increase confidence in the Danish government, and thus further support the occurrence of economic voting. Consistent with sophisticated voters regarding unemployment as a more important signal of government performance than unsophisticated voters, columns (2) and (3) show that the confidence of sophisticated voters is three times more responsive to a given change in unemployment expectations.

Nevertheless, a potentially confounding explanation of our results is that evaluations of government competence are not changing, but rather that lower unemployment expectations have shifted policy preferences toward those associated with left-wing parties (e.g. Meltzer and Richard 1981).

[31] We use 2012 survey responses here because the 2013 question is post-treatment.

Table 4: Economic voting mechanism tests

	(1) Conf. govt.	(2) Conf. govt. (unsophist.)	(3) Conf. govt. (sophist.)	(4) Redist.	(5) Unem. insurance	(6) Red-Green
Unemployment expectations (%)	-0.100*** (0.029)	-0.051* (0.029)	-0.141*** (0.048)	0.032 (0.030)	-0.011 (0.018)	0.004 (0.008)
First stage F statistic	33.4	21.2	66.7	32.6	33.5	32.6
Observations	5,688	2,524	3,164	5,705	5,614	5,705
Outcome mean	2.69	2.64	2.73	3.20	2.23	0.06
Outcome std. dev.	1.00	1.03	0.97	1.02	0.61	0.25

Notes: The dependent variables are (1) a five-point scale of confidence in the government, (2) a five-point scale measuring support for redistribution, (3) a three-point scale measuring support for increasing unemployment insurance, and (4) an indicator for intending to vote for the Red-Green Alliance. All specifications are estimated using 2SLS, and control for current unemployment expectations to increase efficiency. Robust standard errors are provided in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

Self-interested voters maximizing their expected income should decrease their support for redistribution and unemployment insurance to the extent that higher aggregate unemployment expectations are taken as a signal of economy-wide, rather than individual-specific, economic prospects. If aggregate unemployment expectations instead primarily update a voter's subjective probability of being unemployed, support for redistribution and unemployment insurance should increase. We show these predictions formally in the Online Appendix.

However, changes in policy preferences cannot account for our results. First, we examine five- and three-point scales that respectively increase with general support for redistribution and specific support for unemployment benefits. The precisely estimated null effects in columns (4) and (5) of Table 4 show no support for either claim.[32] Second, the existence of left-wing parties outside the government provide a further placebo test for our economic voting interpretation. The Red-Green Alliance—the most left-wing party represented in the Danish Parliament—might expect to pick up

[32]Unreported results show that the effect does not differ by income level. However, the Online Appendix shows that economic voting is confined to those individuals with relatively moderate redistributive preferences.

votes if the information treatments were inducing a change in preferences. Column (6) shows that changes in unemployment expectations do not affect the probability of voting for the Red-Green Alliance. Together, this evidence reinforces the conclusion that economic voting is the principal political manifestation of changes in aggregate unemployment expectations.

5 Conclusion

A key question for democratic accountability is when information causes voters to re-evaluate the government's competence and act politically on their beliefs by voting economically. We move beyond existing work by focusing on the interaction between source credibility and voter political sophistication. We show that although unemployment forecasts cause all types of voters to update their unemployment expectations, only sophisticated voters are able to discern both institutional and political differences in source credibility. Despite the fact that poorly informed voters at least minimally engage with new information, only among sophisticated voters—which are neither extreme partisans nor voters that regularly vote for different parties—does information impact economic voting. We conclude that it is the interaction of credible information and political sophistication that explains when new economic information will affect political behavior.

We acknowledge several limits on the external validity of this experiment. First, we only examine Denmark. However, finding effects in Denmark's complex institutional environment and open economy may represent a lower bound on information's effects. Also, in few other countries could we so easily draw the panel and registry data that we use. Second, we cannot study the likelihood that the least politically engaged voters do not translate information into political action because they do not consume the information in the first place. Rather, we force respondents to receive new economic information, but—contrary to Zaller (1992, 2004)—find that the political behavior of sophisticated voters is more responsive to such information than the behavior of unsophisticated voters. Finally, we do not model or analyze the "real" process by which voters filter information

out of conflicting signals. That would be another valuable project.

The democratic implications of our findings are somewhat mixed. While economic voting is generally regarded as positive for democracy (Anderson 2007), our results show that information about aggregate unemployment is insufficient to induce politically unsophisticated voters to link their unemployment expectations to government performance—and this is not because such voters care about other issues. Nevertheless, finding any effect in Denmark's complex institutional environment and open economy is an important result because it may represent a lower bound on information's effects. Furthermore, since unsophisticated voters cannot differentiate credible from incredible information, the fact that such voters do not act on their information ensures the process cannot be strategically manipulated by political parties supplying incredible information.

Our results also illuminate political party communication strategies. That the least politically engaged voters do not translate their information into political action may explain why political parties in developed democracies target their platforms toward prominent and well-informed voters (Adams and Ezrow 2009; Gilens 2005). Furthermore, our results suggest that parties can benefit electorally from providing specific macroeconomic information, and this is of course prevalent among successful governments. However, since less credible information still affects voter beliefs, our results question why parties do not distort the facts more often. While this may entail losing credibility in some instances (see Druckman 2001), the line between proclaiming truths and falsehoods is often unclear if multiple numbers are available. An important challenge for future research is to understand when political parties choose to send more or less credible signals.

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Appendix

Distribution of seats in parliament

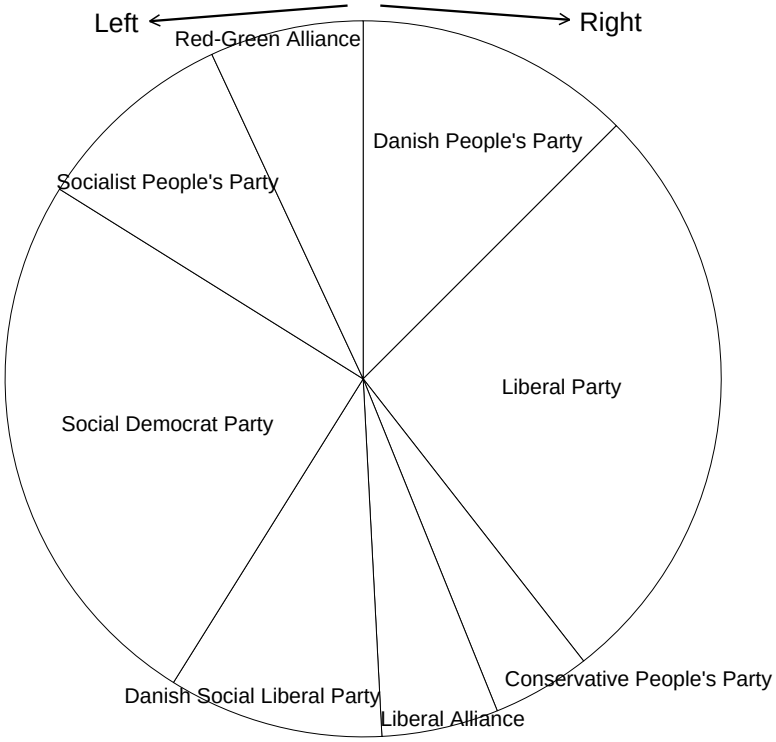

Figure A1: Folketing seat distribution after 2011 election

Notes: Left-right ordering reflects strength of ideology according to the 2011 Danish Election Study.

Danish Central Bank unemployment projections

The most salient definition of unemployment in Denmark is gross unemployment. Unlike net unemployment, gross unemployment includes people in active labor market programs (broadly defined). This is the official measure of the government, and is based on Statistics Denmark calculations—using the national administrative register—of the total number of unemployed persons. Gross unemployment is also used for measurement, projection and policy purposes. The

current (not seasonally corrected) gross unemployment rate as announced in January 2013 was 6.4%, although later revisions put it at 6.5%.

The projection that we cite for the DCB was derived as follows. In 2012Q3, the DCB projected the (not seasonally adjusted) annual change in the number of unemployed persons using both a base projection and multiple alternative scenarios.[33] We estimate the DCB's projection by adding this projected change to the current gross unemployment rate.[34] We thus added the projected change for late 2013 over late 2012 to the gross unemployment rate in late 2012 (6.0%). This yields a projection of 6.8% for the bad state and 6.5% for the baseline projection; we rounded this up to formulate our "almost 7%" treatment condition.

Bayesian interpretation of information updating

Our approach can be clearly shown in a Bayesian updating framework. Specifically, we write individual i's conditional posterior belief about future unemployment level, U, as:

$$P(U = u|X_i, Z_i) = P(U = u|X_i)\frac{P(Z_i|U = u, X_i)}{P(Z_i|X_i)},$$

where Z_i is an information shock received by i, and X_i captures i's characteristics (e.g. ideology and sophistication). The location and specificity of i's prior belief, $P(U = u|X_i)$, depends upon X_i. The likelihood $P(Z_i|U = u, X_i)$ represents i's interpretation of the informativeness of the signal they received: $P(Z_i|U = u, X_i)/P(Z_i|X_i) = 1$ or $P(Z_i|U = u, X_i) = P(Z_i|X_i)$ captures i not believing that receiving signal Z_i is related to the likelihood that the state of the world is $U = u$. H1 and H2 hypothesize that $P(Z_i|U = u, X_i)$ (or more simply $P(Z_i|U = u)$ because individual characteristics

[33] See Danmarks Nationalbank. Monetary Review, 3rd Quarter, 2012. Net unemployment is still computed by macroeconomic models accumulated over many years, despite gross unemployment being the main measure that is ultimately reported. This is because the DCB only projects the total number of unemployed persons. The central bank does not provide monthly projections, only annual.

[34] We thus rely on the very reasonable assumption that net and gross unemployment changes are highly correlated.

play a weak role in objective credibility) is large where Z_i comes from an expert or surprising source. H4 instead hypothesizes that updating depends upon the interaction of the source of Z_i and X_i. H3 allows for $P(U = u|X_i)$ to be large, but also implies that Z_i does not add much new information.

Formal model of information and policy preferences

We extend the Romer (1975) and Meltzer and Richard (1981) framework to include uncertainty over income in a simple way.

Take a continuum of voters of unit mass, differentiated by their income prospects. Voter i's realized income is $y_i \in \mathscr{Y} \subseteq \mathbb{R}^+$. We build in uncertainty in a simple fashion. Individual i's uncertainty at the time of determining their policy preferences is operationalized as follows: with probability $p_i(z_i) \in (0,1)$ their income is y_i^L, and with probability $1 - p_i(z_i)$ their income is y_i^H, where $y_i^H > y_i^L$ and z_i denotes the amount of information i possess about the economy (increases in z_i represent more information). Assume $p_i(z_i)$ is differentiable and monotonic in z_i. To save space, define i's expected income as $Y_i(z_i) \equiv p_i(z_i)y_i^L + (1 - p_i(z_i))y_i^H$.

Voters are also uncertain about the aggregate distribution of income. In particular, with voter i assigns probability $q(z_i) \in (0,1)$ to economy-wide average income being $\bar{y}^L$, and probability $1 - q(z_i)$ to economy-wide average income being $\bar{y}^H$, where $\bar{y}^H > \bar{y}^L$. Assume q is differentiable and monotonic in z_i. Define i's expected average income in the economy as $\bar{Y}(z_i) \equiv q(z_i)\bar{y}^L + (1 - q(z_i))\bar{y}$

The government must choose a tax and benefit policy pair (τ, T) to be implemented after income is realized, where $\tau \in [0,1]$ is a proportional tax rate levied on y and $T \geq 0$ is a lump-sum transfer made to all citizens. There is a cost $\phi(\tau)\bar{y}$ to increasing τ, where $\int_{y \in \mathscr{Y}} y dF(y) = \bar{y}$ is the realized mean income and $\phi : [0,1] \mapsto \mathbb{R}^+$ is a convex-increasing function such that $\phi'(\tau) > 0, \phi''(\tau) > 0$ and $\phi(0) = 0$. This cost could be labor supply disincentives, capital misallocation or the inefficiency of revenue collection. We assume that the tax rate cannot depend upon the realized

state of the world.

We now derive individual i's preferences over policies before income is realized. From the perspective of voter i, the *ex ante* government budget constraint is:

$$[\tau - \phi(\tau)]\,\bar{Y}(z_i) \leq T. \tag{3}$$

Since the budget constraint will bind in equilibrium, the problem is reduced to a single dimensional problem in τ.

A voter with income y has the following policy utility function, receiving utility from post-tax income and the lump-sum transfer:

$$\begin{aligned} u\Big((1-\tau)Y_i(z_i) + [\tau - \phi(\tau)]\,\bar{Y}(z_i) + (1-\tau)\bar{Y}(z_i) + [\tau - \phi(\tau)]\,\bar{Y}(z_i)\Big) \\ = u\Big((1-\tau)Y_i(z_i) + [\tau - \phi(\tau)]\,\bar{Y}(z_i)\Big), \end{aligned} \tag{4}$$

where $u : \mathbb{R} \mapsto \mathbb{R}$ is a concave-increasing function: $u'(\cdot) > 0, u''(\cdot) < 0$ and $u(0) = 0$. Tax rates have two effects on voter utility: redistribution of income and a (disincentive) cost to increasing taxation.

Given preferences are strictly concave in τ, they are single-peaked. We can identify the ideal policy of voter i as:

$$\tau_i^* = \max\left\{(\phi')^{-1}\left(1 - \frac{Y_i(z_i)}{\bar{Y}(z_i)}\right), 0\right\}. \tag{5}$$

This reiterates the Romer-Meltzer-Richard logic that i's preference for taxation is increasing as their *expected* income relative to the *expected* average income falls. Note that all $Y_i(z_i) > \bar{Y}(z_i)$ prefer $\tau_i^* = 0$.

In the space where $\tau_i^* > 0$, or for voters with expected incomes exceeding the average expected

income, the comparative static with respect to new information is:

$$\frac{d\tau_i^*}{dz_i} = \frac{p_i'(z_i)(y_i^H - y_i^L)\bar{Y}(z_i) - q'(z_i)(\bar{y}^H - \bar{y}^L)Y_i(z_i)}{\phi''(\tau)[\bar{Y}(z_i)]^2}. \tag{6}$$

Given $y_i^H - y_i^L > 0$ and $\bar{y}^H - \bar{y}^L > 0$, and expected individual and aggregate income is positive, it is clear that we have opposing effects when $sgn(p_i'(z_i)) = sgn(q'(z_i))$—this is the obvious case for this paper as it is very unlikely that aggregate information would cause voters to differentially update. Intuitively, the first term in the numerator captures how information affects i's taxation preferences associated with their own expected income, while the second term captures how information affects taxation preferences in the rest of the economy. These are easiest to see by setting $q'(z_i) = 0$ and $p_i'(z_i) = 0$ respectively.

Without loss of generality (the results will just be the opposite), let us focus on the case where $sgn(p_i'(z_i)) = sgn(q'(z_i)) \leq 0$; this turns out to be the most appropriate case for our analysis because our information treatments cause voters to become more positive about the economy. It is now clear that new information causing voters to reduce their belief of being unemployed and reduce their belief of others being unemployed has the expected effects: the fall in the likelihood of i being unemployed reduces their preference for taxation (first term in (6)), while the fall in the likelihood of others being unemployed increases their preference for taxation (second term in (6)). These effects clearly conflict, with the individual income incentive overpowering the general economy incentive when:

$$\frac{p_i'(z_i)}{q'(z_i)} > \frac{(\bar{y}^H - \bar{y}^L)Y_i(z_i)}{(y_i^H - y_i^L)\bar{Y}(z_i)}, \tag{7}$$

or when the change in $p_i(z_i)$ is sufficiently large relative to the change in $q(z_i)$.

These insights can be easily extended to voter in Denmark, where politics is primarily based on a left-right axis. Simplifying the analysis such that voters choose between left and right parties, it is clear that any increase in preferred tax rate should correspond to increased support for the

left-wing government.

Data

Variable definitions and summary statistics

The variable definitions below describe all the variables used in the main article and in the Online Appendix. Summary statistics for all variables are provided in Table A1.

Information source treatments. Respondents were randomly assigned to a control group receiving no information, one of six groups receiving the prime "Assume that that the [DCB/government/Liberals] estimates unemployment in 2013 will be [almost 7%/around 5%]" or "The DCB estimates unemployment in 2013 to be almost 7%." Respectively, these statements translate as: "*Antag at [Nationalbanken/Regeringen/Venstre] vurderer at arbejdsløsheden i 2013 vil være [knap 7%/ca. 5%]*" and This translates from: "*Nationalbanken vurderer at arbejdsløsheden i 2013 vil være knap 7%*." In the main text, treatments are denoted, for example, by "DCB 7% treatment".

Unemployment expectations. The percentage (not restricted to integers) reported by the respondent in response 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." This translates from: "*Hvad er dit bedste bud på hvad arbejdsløsheden vil blive i 2013? Vi vil gerne have dit bedste bud, også selvom du ikke er helt sikker*." This question immediately followed the treatment.

Current unemployment estimate. The percentage (not restricted to integers) reported by the respondent in response 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." This question immediately preceded the treatment.

Sophisticated voters. Indicator coded 1 for respondents that correctly identified the current

unemployment rate as between 5% and 8%, or within 1.5 percentage points of the true level (6.5%) in January 2013.

Vote intention. Respondent stated the party that would vote for in response to the question "How would you vote tomorrow?" Respondents choose one of the following: Social Democrat Party, Social Liberal Party, Conservative People's Party, Socialist People's Party, Danish People's Party, Liberal Party, Liberal Alliance, or Red-Green Alliance. Answers not stating a party were: blank, no answer, other, would not vote, and don't know. We counted the Conservative People's Party, Danish People's Party, Liberal Party, and Liberal Alliance as right-wing parties. We counted the Social Democratic Party, Social Liberal Party and Socialist People's Party as parties in government.

Confidence in the government. Responded were asked, on a scale from great mistrust (1) to great trust (5), how much they trust the government. This question was asked 11 questions after the treatment was administered.

Redistribution. This variable measures support for redistribution on a five-point scale ranging from "every man for himself" (1) to the government "should help the poor a lot" (5). This was in response to the prime "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." This question was 19 questions after the treatment.

Unemployment insurance. Three-point "less-same-more" response to the question "The economic crisis has meant that many people have lost their job. Do you think that the government should support the unemployed?" this question was asked immediately after the question eliciting the respondent's unemployment expectations.

Lower immigrant benefits. Indicator coded 1 for respondents who responded that separate and lower benefits for immigrants should be reinstated in 2012.

Denmark economic prospects. Five point scale, from "worsen considerably" to "improve considerably, response to the question of how the Danish economy overall will do in 2013.

Improving economic prospects. Indicator coded 1 for individuals responding that the Danish economy for 2013 will be "much better" or "better" than 2012. This variable is re-coded from Denmark economic prospects.

Frequently discuss politics. The sum of the set of indicators coded 1 for respondents who answered that they talk to family, friends, neighbors, work colleagues or others about politics.

News every day. Indicator coded 1 for respondents who state that they watch or read about politics and economics in the news "every day".

Wage income 2012 (log). Reported total wage income before taxation in 2012, as a natural logarithm. Respondents with zero income were coded as 0.

Expected income 2013 (log). Reported total expected income before taxation for 2013, as a natural logarithm. Respondents with zero income were coded as 0.

Tenured. Indicator coded 1 for respondents with tenured jobs.

Own job risk. The probability, as a percentage, assigned by the respondent to the possibility that they will experience a period of unemployment in the forthcoming year.

Risk aversion. Risk aversion scale, from risk-loving (1) to risk-averse (10).

Education. Three-point scale indicating the level of education achieved by the respondent: 1 ("basic") is less than university education; 2 ("medium") is some or complete undergraduate university; 3 ("long") is further academic study. This variable was computed using the Danish register data for the entire population, and is therefore not available in the replication dataset.

Woman. Indicator coded 1 for women.

Year of birth. Year of birth.

Voted government at last election. Indicator coded 1 for respondents who voted for one of the Social Democrats, Social Liberals, or Socialist People's parties in the 2011 election.

Voted left at last election. Indicator coded 1 for respondents who voted for one of the Social Democrats, Social Liberals, Socialist People's, or Red-Green Alliance parties in the 2011 election.

Voted right at last election. Indicator coded 1 for respondents who did not vote for a left party

at the 2011 election.

Voted left at 2007 election. Indicator coded 1 for respondents who voted for one of the Social Democrats, Social Liberals, Socialist People's, or Red-Green Alliance parties in the 2007 election.

Voted right at 2007 election. Indicator coded 1 for respondents who did not vote for a left party at the 2007 election.

Swing voter (previous votes). Indicator coded 1 for respondents who provided different responses to the question asked respondents to recall who they voted for in the 2007 and 2011 elections.

Swing voter (previous intentions). Indicator coded 1 for respondents who provided different responses to the vote intention questions asked in 2011 and 2012.

Extreme voter. Indicator coded 1 for respondents who answered either "should help the poor a lot" or "every man for himself" to the redistribution question in 2012.

Municipal immigration share. The share of immigrants in the municipality that the respondent resides in. Denmark contains 98 municipalities. This variable was computed using the Danish register data for the entire population, and is therefore not available in the replication dataset.

Parish immigration share. The share of immigrants in the parish that the respondent resides in. The average parish contains around 2,500 residents. Parishes are the lowest administrative units in Denmark. This variable was computed using the Danish register data for the entire population, and is therefore not available in the replication dataset.

Replication and register data

With the exception of education and parish and municipal aggregates, all variables are available in the replication dataset. This data was constructed from the 2010-2014 panel surveys (Kreiner, Lassen and Leth-Peterson 2013). However, our education measure comes from the Danish register database, and thus requires special permission for usage (see below). Accordingly, education variables cannot be provided in the replication data. Similarly, parish and municipal immigration rates

Table A1: Summary statistics

	Obs.	Mean	Std. dev.	Min.	Max.
Dependent variables					
Social Democrat Party	5,705	0.17	0.37	0	1
Social Liberal Party	5,705	0.09	0.29	0	1
Socialist People's Party	5,705	0.06	0.24	0	1
Liberal Party	5,705	0.28	0.45	0	1
Red-Green Alliance	5,705	0.06	0.25	0	1
Confidence in government	5,688	2.69	1.00	1	5
Redistribution	5,705	3.20	1.02	1	5
Unemployment insurance	5,614	2.23	0.61	1	3
Dependent/endogenous variable					
Unemployment expectations	5,705	7.98	3.55	0	45
Treatment variables					
Control	5,705	0.13	0.33	0	1
DCB 7% treatment (combined)	5,705	0.25	0.44	0	1
DCB 5% treatment	5,705	0.13	0.33	0	1
Government 7% treatment	5,705	0.12	0.33	0	1
Government 5% treatment	5,705	0.12	0.33	0	1
Opposition 7% treatment	5,705	0.12	0.33	0	1
Opposition 5% treatment	5,705	0.12	0.33	0	1
Covariates					
Current unemployment estimate	5,705	8.58	4.31	0	45
Sophisticated voter	5,705	0.56	0.50	0	1
Swing voter (previous votes)	3,827	0.43	0.50	0	1
Swing voter (previous intentions)	4,566	0.23	0.42	0	1
News every day	5,705	0.72	0.45	0	1
Improving economic prospects	5,675	0.34	0.47	0	1
Municipal immigrant share	5,704	9.74	5.53	3.67	32.75
Parish immigrant share	5,704	8.76	7.09	0	69.72
Medium education	5,642	0.67	0.47	0	1
High education	5,642	0.11	0.31	0	1
Woman	5,705	0.49	0.50	0	1
Year of birth	5,692	1961.85	11.41	1930	1991
Frequently discuss politics	5,705	2.30	1.14	1	4
Voted government at last election	5,705	0.45	0.50	0	1
Voted left at last election	5,705	0.50	0.50	0	1
Voted right at last election	5,705	0.41	0.49	0	1
Voted left at 2007 election	3,827	0.46	0.50	0	1
Voted right at 2007 election	3,827	0.43	0.50	0	1
Extreme voter	4,566	0.17	0.38	0	1
Wage income 2012 (log)	5,532	10.19	5.15	0	19.84
Expected income 2013 (log)	5,554	12.70	0.63	5.01	20.50
Tenured	4,540	0.48	0.50	0	1
Job risk	4,540	16.36	29.09	0	100
Risk aversion	5,580	6.78	2.43	1	10
Lower immigrant benefits	5,705	0.25	0.43	0	1

were computed using the Danish register data and cannot be provided in the replication dataset.

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

Once the necessary permissions have been obtained, the replication code will then replicate all results in the Online Appendix (non-replicable results are currently "asterisked" out in the code). All the results in the main article can be replicated exactly because they do not require any register variables.

Voter sophistication measure

We measure voter sophistication according to the accuracy of their current unemployment estimate. As noted in the main text and above, a respondent is defined as sophisticated if their estimate is within 1.5 percentage points of the true 6.5% unemployment rate in January 2013. Although this is not a standard definition of political sophistication, it is particularly relevant for the case of economic voting. Moreover, it turns out that knowledge of the economy effectively serves as a sufficient statistic for more standard measures of political sophistication.

Table A2: Correlation between absolute deviation from current unemployment rate and standard indicators of political sophistication

	Outcome: Absolute deviation from current unemployment rate		
	(1)	(2)	(3)
Constant	6.232***	7.336***	9.077***
	(0.359)	(0.497)	(0.688)
Follow the news	-0.369***	-0.275***	-0.313***
	(0.056)	(0.071)	(0.085)
Wage income 2012 (log)	-0.038***	-0.037***	-0.038**
	(0.011)	(0.013)	(0.017)
Medium education	-0.618***	-0.794***	-0.726***
	(0.145)	(0.183)	(0.210)
High education	-1.239***	-1.285***	-0.897***
	(0.170)	(0.204)	(0.239)
Frequently discuss politics	-0.186***	-0.124**	-0.069
	(0.048)	(0.060)	(0.069)
Self-reported political knowledge		-0.433***	-0.359***
		(0.073)	(0.075)
Math test score (scale)			-2.639***
			(0.424)
Observations	5,471	3,682	2,662

Notes: The dependent variable is the absolute deviation between the respondent's current unemployment estimate and the true rate of 6.5%. The omitted education category is low education. All specifications are estimated using OLS, with robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

First, Table A2 shows that the absolute deviation of a respondent's unemployment estimate from the true rate is highly correlated with more standard measures of political sophistication. In particular, columns (1)-(3) show that the absolute deviation is significantly negatively correlated with frequency of watching the news, regular discussion of politics, wages (log), undergraduate and postgraduate education, the number of correct answers on a math test, and a respondent's self-reported level of political information. This indicates that our definition of sophistication is strongly positively correlated with this standard measures (since a low absolute deviation indicates greater sophistication). Table A3 shows that these correlations equally hold when using the indicator sophistication variable—an indicator for respondents with an absolute deviation that falls within 1.5 percentage points of the true current unemployment rate—used in the main paper. In this case, a high value of the dependent variable represents greater sophistication.

Second, Tables A4 and A5 interact our treatments with various measures of political sophistication. In both tables, all coefficients are estimated from a single regression controlling for all interactions simultaneously. The first row indicates the baseline effect in the control and treatment conditions, while the six rows below show the interaction between each treatment and the specified variable (this is the lower-order term in the case of the control condition). Table A4 shows that standard measures of political sophistication indicate that less sophisticated voters update significantly less on average. It is important to note that when pooling the full sample here, we are not able to capture the nuances of how voters update. However, Table A5 shows that the inclusion of the interaction between the absolute deviation from the true current unemployment rate and the treatments breaks the correlation for all other political sophistication measures—including, as shown in the first column, in the control group. This forms the basis of our claim that the current unemployment estimate is a sufficient statistic for measuring political sophistication in our context of economic voting.

Third, Table A6 shows that our indicator of political sophistication is not correlated with previous vote choices. Specifically, columns (1)-(8) show that political sophistication is not significantly

Table A3: Correlation between our voter sophistication indicator and standard indicators of political sophistication

	Outcome: Sophisticated voter		
	(1)	(2)	(3)
Constant	0.204***	0.119**	-0.038
	(0.038)	(0.050)	(0.065)
Follow the news	0.041***	0.026***	0.022**
	(0.006)	(0.008)	(0.009)
Wage income 2012 (log)	0.002*	0.002	0.000
	(0.001)	(0.002)	(0.002)
Medium education	0.044***	0.053***	0.056**
	(0.016)	(0.020)	(0.024)
High education	0.139***	0.135***	0.107***
	(0.024)	(0.029)	(0.034)
Frequently discuss politics	0.027***	0.023***	0.019**
	(0.006)	(0.007)	(0.009)
Self-reported political knowledge		0.050***	0.051***
		(0.008)	(0.010)
Math test score (scale)			0.262***
			(0.046)
Observations	5,471	3,682	2,662

Notes: The dependent variable is our indicator of voter sophistication. The omitted education category is low education. All specifications are estimated using OLS, with robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

Table A4: Heterogeneous effects of information treatments on unemployment expectations (%), by conditional marginal effect of standard political sophistication measures

	Outcome: Unemployment expectations							
	Control	DCB 7% (true)	DCB 7%	DCB 5%	Govt. 7%	Govt. 5%	Opp. 7%	Opp. 5%
Baseline (treatment or control) effect	15.064***	-6.292***	-5.928***	-5.254***	-4.287**	-5.641***	-5.140***	-4.859***
	(1.391)	(1.488)	(1.518)	(1.77)	(1.702)	(1.648)	(1.644)	(1.751)
× Follow the news	-0.531***	0.523**	0.459**	0.264	0.226	0.346	0.360	0.157
	(0.205)	(0.224)	(0.232)	(0.272)	(0.248)	(0.248)	(0.232)	(0.27)
× Frequently discuss politics	-0.264	0.020	0.062	0.174	0.219	0.062	0.161	0.166
	(0.170)	(0.187)	(0.197)	(0.207)	(0.191)	(0.219)	(0.207)	(0.212)
× Wage income 2012 (log)	-0.124***	0.110**	0.100**	0.033	0.089*	0.162***	0.065**	0.115**
	(0.045)	(0.049)	(0.050)	(0.056)	(0.049)	(0.052)	(0.055)	(0.054)
× Medium education	-1.614***	1.445***	1.517***	1.803***	1.022	1.006	1.518**	1.266*
	(0.552)	(0.591)	(0.604)	(0.665)	(0.644)	(0.669)	(0.637)	(0.671)
× High education	-1.977***	1.585**	1.551**	1.505**	0.793	0.599	1.692**	1.729**
	(0.640)	(0.699)	(0.719)	(0.755)	(0.725)	(0.768)	(0.747)	(0.868)

Notes: All coefficients are estimated from a single OLS equation interacting all treatments conditions with the variables on the left hand side of the table (see Online Appendix for their definitions). The first column contains the lower-order effects (in the control group), including the constant in the first cell. The sample size is 5,471. Robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

Table A5: Heterogeneous effects of information treatments on unemployment expectations (%), by conditional marginal effect of standard political sophistication measures—controlling for interaction with the absolute deviation

	Outcome: Unemployment expectations							
	Control	DCB 7% (true)	DCB 7%	DCB 5%	Govt. 7%	Govt. 5%	Opp. 7%	Opp. 5%
Baseline (treatment or control) effect	6.218***	0.371	0.096	-1.363	1.511*	-0.385	0.774	-0.264
	(0.621)	(0.758)	(0.913)	(0.972)	(0.887)	(0.957)	(0.889)	(1.114)
× Absolute deviation	1.013***	-0.548***	-0.513***	-0.242**	-0.549***	-0.370***	-0.400***	-0.418***
	(0.036)	(0.071)	(0.101)	(0.099)	(0.092)	(0.086)	(0.098)	(0.1)
× Follow the news	-0.032	0.118	0.136	0.112	-0.070	-0.002	-0.014	-0.020
	(0.104)	(0.126)	(0.139)	(0.146)	(0.141)	(0.142)	(0.13)	(0.172)
× Frequently discuss politics	-0.016	-0.119	-0.082	0.107	0.085	-0.022	-0.122	-0.004
	(0.080)	(0.099)	(0.112)	(0.115)	(0.105)	(0.122)	(0.119)	(0.126)
× Wage income 2012 (log)	-0.011	-0.001	0.005	-0.042	-0.025	0.036	0.007	0.019
	(0.020)	(0.025)	(0.026)	(0.026)	(0.025)	(0.028)	(0.029)	(0.032)
× Medium education	0.123	-0.093	-0.210	0.195	-0.249	-0.281	-0.217	0.000
	(0.249)	(0.304)	(0.314)	(0.347)	(0.335)	(0.359)	(0.337)	(0.415)
× High education	-0.120	0.326	0.099	0.534	-0.425	-0.060	0.297	0.433
	(0.346)	(0.42)	(0.428)	(0.442)	(0.425)	(0.468)	(0.443)	(0.542)

Notes: All coefficients are estimated from a single OLS equation interacting all treatments conditions with the variables on the left hand side of the table (see Online Appendix for their definitions). The first column contains the lower-order effects (in the control group), including the constant in the first cell. The sample size is 5,471. Robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

correlated with voting for a left or right party in either 2007 or 2011 in any specification. We omit the effectively identical results for the absolute deviation measure.

Support for the identification assumptions

Balance across treatments

Tables A7 and A8 look at balance over pre-treatment covariates from both the Register and the survey. *F* tests of all treatment coefficients being equal are rejected with regularity consistent with chance: only in one of 16 tests was the joint test statistically different from zero at the 5% level (and also once at the 10% level). Even in those cases, the differences between treatment conditions are small. Accordingly, we do not include controls, although the results are robust to including such variables.

IV assumptions

Consistent instrumental variables estimation requires two additional assumptions beyond random assignment: monotonicity and an exclusion restriction. Monotonicity entails that each individual would update their unemployment expectations in the same direction after receiving the treatment. However, the 27% of respondents with lower current unemployment estimates than the treatment projection they receive may increase their unemployment expectations. Figures A2 and A3 show the cumulative density functions plotting the proportion of individuals for each instrument expecting unemployment below a certain level. The key point to note is that while the 7% treatments lie almost entirely to the left of the control group, the 5% treatments do not. This implies that the monotonicity assumption underpinning the instrumental variable does not hold in such cases.

Fortunately, IV estimation remains consistent when "few subjects are defiers, or if defiers and compliers have reasonably similar distributions of potential outcomes" (de Chaisemartin 2014:

Table A6: Correlation between our indicator of sophisticated voters and measure of partisanship

	Outcome: Sophisticated voter							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	2.971***	0.546***	2.892***	0.555***	2.900***	0.565***	2.848***	0.571***
	(0.073)	(0.009)	(0.066)	(0.009)	(0.086)	(0.011)	(0.082)	(0.011)
Voted left at last election	-0.153	0.019						
	(0.101)	(0.013)						
Voted right at last election			0.004	0.001				
			(0.102)	(0.013)				
Voted left in 2007 election					-0.157	0.013		
					(0.123)	(0.016)		
Voted right in 2007 election							-0.049	0.002
							(0.124)	(0.016)
Observations	5,705	5,705	5,705	5,705	3,827	3,827	3,827	3,827

Notes: The dependent variable is our indicator of voter sophistication. All specifications are estimated using OLS, with robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

Table A7: Balance tests 1

	(1) Current U Est.	(2) Woman	(3) Year of birth	(4) Basic Edu.	(5) Med. Edu	(6) Long Edu.	(7) Voted Govt.	(8) Voted Left
Control	8.704***	0.493***	1961.541***	0.243***	0.660***	0.097***	0.432***	0.492***
	(0.177)	(0.019)	(0.434)	(0.016)	(0.018)	(0.011)	(0.019)	(0.019)
DCB 7% treatment	-0.264	-0.002	0.869	0.003	-0.016	0.013	0.021	0.015
	(0.230)	(0.026)	(0.608)	(0.023)	(0.025)	(0.016)	(0.026)	(0.026)
DCB 7% treatment (true)	-0.364	-0.009	0.171	0.002	0.012	-0.014	0.012	-0.000
	(0.226)	(0.027)	(0.617)	(0.023)	(0.025)	(0.015)	(0.027)	(0.027)
DCB 5% treatment	-0.249	0.025	0.374	-0.047**	0.026	0.021	0.049*	0.042
	(0.229)	(0.026)	(0.601)	(0.022)	(0.025)	(0.016)	(0.026)	(0.026)
Government 7% treatment	-0.086	0.009	0.234	-0.033	0.010	0.024	0.004	-0.002
	(0.240)	(0.027)	(0.601)	(0.022)	(0.025)	(0.017)	(0.026)	(0.027)
Government 5% treatment	0.224	-0.004	-0.190	-0.030	0.012	0.017	0.031	0.017
	(0.252)	(0.027)	(0.626)	(0.022)	(0.025)	(0.016)	(0.026)	(0.027)
Opposition 7% treatment	-0.265	-0.024	0.443	-0.009	0.013	-0.004	-0.000	-0.007
	(0.239)	(0.027)	(0.607)	(0.023)	(0.025)	(0.016)	(0.026)	(0.027)
Opposition 5% treatment	0.011	-0.012	0.529	-0.021	-0.010	0.031*	0.050*	0.036
	(0.249)	(0.027)	(0.602)	(0.023)	(0.025)	(0.017)	(0.026)	(0.027)
Observations	5,705	5,705	5,692	5,642	5,642	5,642	5,705	5,705
F test of equality over treatments	$p = 0.19$	$p = 0.75$	$p = 0.78$	$p = 0.18$	$p = 0.74$	$p = 0.07$	$p = 0.29$	$p = 0.47$

Notes: The dependent variable is listed at the top of each column. All specifications estimated using OLS. Robust standard errors in parentheses. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$.

Table A8: Balance tests 2

	(1) Voted right	(2) Wages 2012 (log)	(3) Exp. Income 2013 (log)	(4) Homeowner	(5) Denmark econ. prospects	(6) Tenured	(7) Job risk	(8) Risk aversion
Constant (control)	0.442***	10.160***	12.681***	0.801***	3.091***	0.499***	15.583***	6.750***
	(0.019)	(0.197)	(0.025)	(0.015)	(0.031)	(0.021)	(1.186)	(0.095)
DCB 7% treatment	-0.034	-0.068	0.033	0.017	0.041	-0.036	0.660	-0.062
	(0.026)	(0.275)	(0.034)	(0.021)	(0.042)	(0.029)	(1.676)	(0.131)
DCB 5% treatment	-0.062**	-0.109	0.028	0.034*	0.046	-0.029	1.120	0.153
	(0.229)	(0.280)	(0.033)	(0.020)	(0.042)	(0.030)	(1.709)	(0.131)
Government 7% treatment	-0.010	-0.005	0.044	0.034	0.044	-0.009	-0.419	0.067
	(0.026)	(0.279)	(0.037)	(0.020)	(0.042)	(0.030)	(1.659)	(0.131)
Government 5% treatment	-0.039	-0.004	0.015	0.010	0.036	-0.010	-0.638	0.220*
	(0.252)	(0.279)	(0.034)	(0.021)	(0.042)	(0.030)	(1.677)	(0.132)
Opposition 7% treatment	-0.003	0.156	0.022	0.018	0.032	-0.049	3.737**	-0.015
	(0.027)	(0.277)	(0.034)	(0.021)	(0.042)	(0.030)	(1.788)	(0.133)
Opposition 5% treatment	-0.045*	0.139	0.011	0.034	-0.010	-0.029	1.741	0.101
	(0.026)	(0.276)	(0.035)	(0.020)	(0.043)	(0.030)	(1.721)	(0.135)
DCB 7% treatment (true)	-0.022	0.175	0.033	0.000	-0.015	-0.004	-0.029	-0.215
	(0.027)	(0.278)	(0.034)	(0.021)	(0.043)	(0.030)	(1.704)	(0.133)
Observation	5,705	5,532	5,544	5,705	5,675	4,540	4,540	5,580
F test of equality over treatments	$p = 0.20$	$p = 0.95$	$p = 0.95$	$p = 0.44$	$p = 0.38$	$p = 0.68$	$p = 0.26$	$p = 0.03$

Notes: See Table A7.

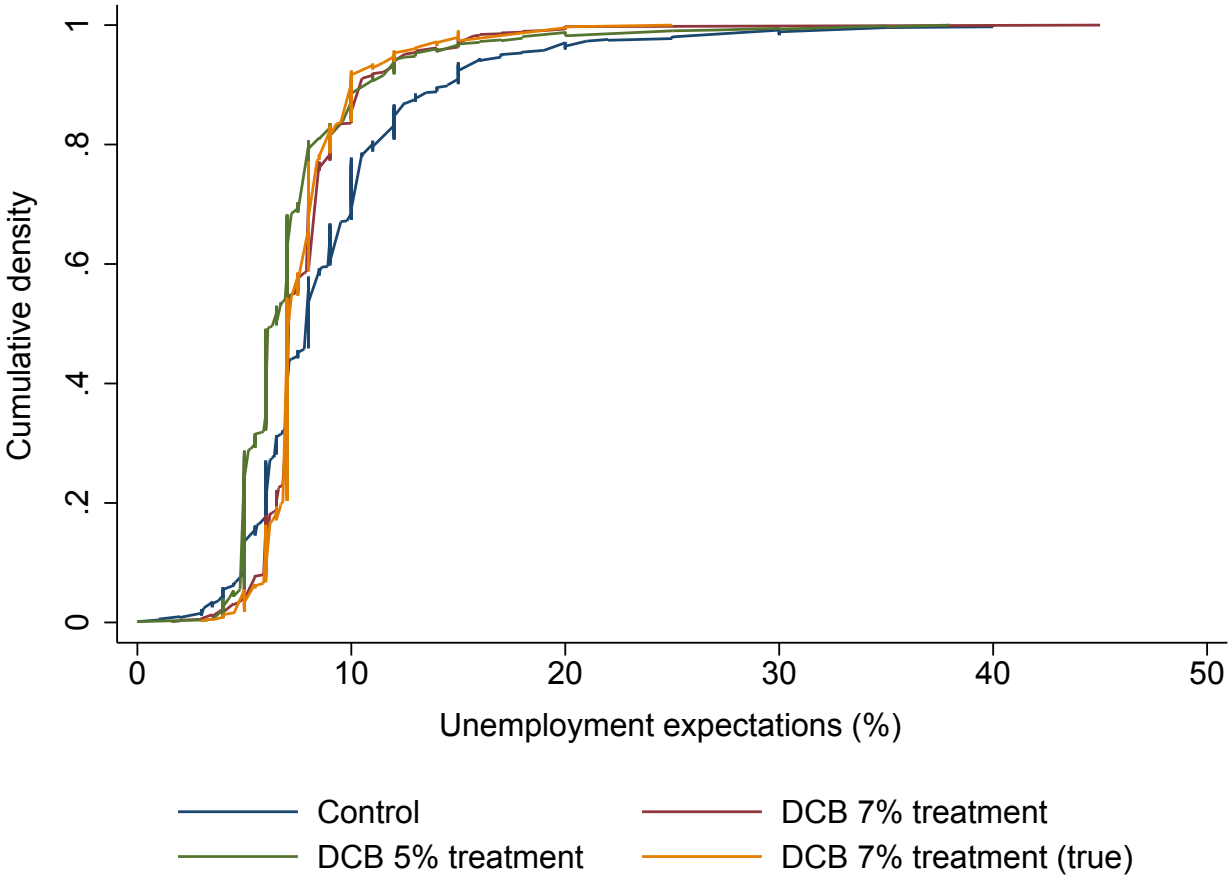

Figure A2: Cumulative density plots of unemployment expectations by information treatment 1

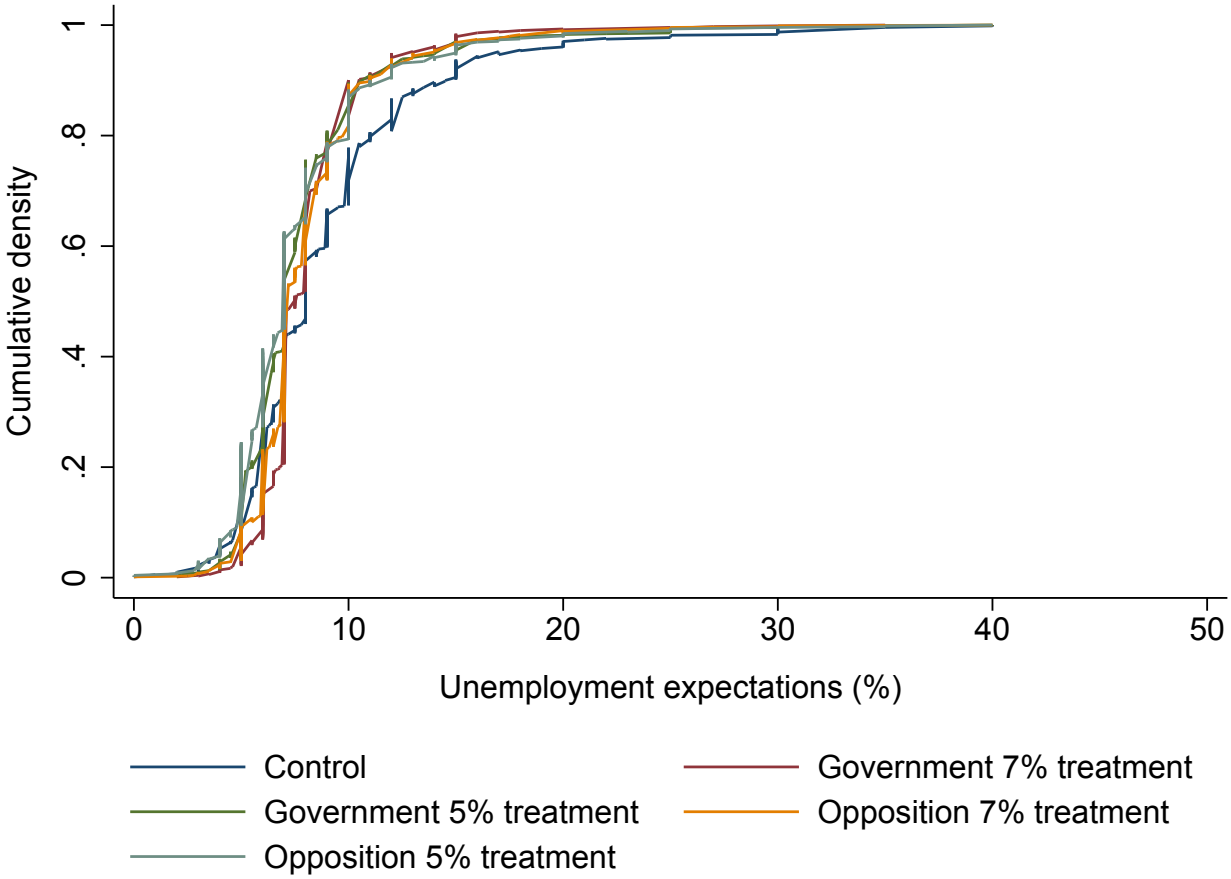

Figure A3: Cumulative density plots of unemployment expectations by information treatment 2

Table A9: Effects of treatments on belief that political information is important

	Outcome: Information is important	
	(1)	(2)
Control	0.662***	0.662***
	(0.018)	(0.018)
DCB 7% treatment (true)	0.006	
	(0.025)	
DCB 7% treatment (assume)	-0.001	
	(0.025)	
DCB 5% treatment	0.003	
	(0.025)	
Government 7% treatment	0.023	
	(0.025)	
Government 5% treatment	-0.021	
	(0.025)	
Opposition 7% treatment	-0.001	
	(0.025)	
Opposition 5% treatment	-0.012	
	(0.025)	
Any treatment		-0.000
		(0.019)
Observations	5,705	5,705

Notes: Dependent variable is a dummy for whether the respondent believes political information is important for either private economic decisions or as part of the respondent's job. Both specifications are estimated using OLS and include 5,705 observations. Robust standard errors in parentheses. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

7).[35] Although updating upward and downward may have similar effects, we also separately examine voters whose current unemployment estimates are above, below and between the information provided by the treatments as a further robustness check below.

The exclusion restriction is discussed in the main paper. However, as noted in the main text, Table A9 shows no treatment affects the respondent's belief that political information is important. This serves as an important robustness check for the exclusion restriction concern that simply

[35]More precisely, the Wald estimator recovers the average treatment effect for the set of compliers not canceled out by the defiers (de Chaisemartin 2014).

Table A10: Effect of information treatments on confidence in sources

	(1) Trust DCB	(2) Trust government	(3) Trust opposition
Control	0.682***	0.175***	0.290***
	(0.018)	(0.014)	(0.017)
DCB 7% treatment (combined)	0.063***		
	(0.021)		
DCB 5% treatment	0.063***		
	(0.024)		
Government 7% treatment		0.004	
		(0.020)	
Government 5% treatment		0.023	
		(0.021)	
Opposition 7% treatment			0.012
			(0.024)
Opposition 5% treatment			0.004
			(0.024)
Observations	2,880	2,123	2,118

Notes: The dependent variable is given at the top of each column. All specifications estimated using OLS. Robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

receiving the treatment inducing respondents to think about politics differentially without being affected by the particular unemployment information provided.

Confidence in sources

Table A10 shows that of the treatment sources, only the DCB treatment significantly increases trust in the source of the information. Trust is a dummy variable for trusting or greatly trusting the institution. This test was designed to ameliorate the concern that simply hearing the source's name, independently of the information, is driving the results. Although this is not quite possible for the DCB, its large effects combined with the high level of initial trust, indicate that this should not be a problem.

Additional results

Effects of information source on unemployment expectations

We split the sample to allow for differential effects between voters updating in different directions. In particular, we subset our results by comparing respondents whose current unemployment estimate is above, below and between the 5% and 7% treatment projections. Given that current estimates are highly correlated with expectations in the control group, they represent a good proxy for prior beliefs over the unemployment expectation. Accordingly, we thus expect that all treatments, but especially the 7% treatment, will increase the expectations of those with a current estimate below 5%. Similarly, we expect that all treatments, but especially the 5% treatment, will decrease the expectations of those with a current estimate above 7%. For those with an estimate between 5% and 7% we expect that the 5% treatments will decrease expectations and the 7% treatments will increase expectations; since this group is also comprised entirely by sophisticated voters, we expect to observe that such voters will be best able to differentiate source credibility.

The results in Table A11 broadly reflect our findings regarding political sophistication. Column (2) shows that voters with current unemployment estimates between the two treatments behave like sophisticated voters, recognizing both institutional and political incentive bases for credibility. As expected, for this group, the 5% and 7% treatment levels have opposite effects. As noted in the main text, a larger-than-prior opposition forecast is significantly less credible than a similar government or DCB forecast, while a smaller-than-prior forecast is most credible from the DCB, although the opposition claim causes a larger reduction than the government claim. Similarly, in column (3), those with low current estimates—who have perhaps paid attention in the past but failed to adjust their beliefs during the financial crisis—find both the DCB and especially the government's relatively negative economic outlook to be most convincing. However, like unsophisticated voters, those in column (1) with high initial estimates, if anything, only discern differences in institutional credibility regarding the DCB. The Online Appendix confirms that the high current estimate sam-

Table A11: Effect of information treatments on unemployment expectations (%), by direction of expected updating

	Outcome: Unemployment expectations		
	(1) High current est.	(2) Current est. $\in [5,7]$	(3) Low current est.
Control	11.691***	6.505***	4.027***
	(0.281)	(0.071)	(0.181)
DCB 7% treatment (combined)	-2.572***	0.295***	1.560***
	(0.302)	(0.079)	(0.231)
Government 7% treatment	-2.177***	0.340***	1.882***
	(0.329)	(0.090)	(0.315)
Opposition 7% treatment	-1.958***	0.076	1.373***
	(0.346)	(0.095)	(0.277)
DCB 5% treatment	-2.743***	-0.623***	0.576**
	(0.371)	(0.091)	(0.235)
Government 5% treatment	-2.416***	-0.426***	0.631***
	(0.354)	(0.093)	(0.237)
Opposition 5% treatment	-2.132***	-0.496***	0.205
	(0.371)	(0.098)	(0.230)
Coefficient equality F tests (p values)			
DCB 7% = Government 7%	0.31	0.64	0.07*
DCB 7% = Opposition 7%	0.08*	0.00***	0.79
Government 7% = Opposition 7%	0.41	0.00***	0.13
DCB 5% = Government 5%	0.31	0.02**	0.80
DCB 5% = Opposition 5%	0.07*	0.15	0.07*
Government 5% = Opposition 5%	0.38	0.44	0.04**
Observations	2,961	2,317	427
Outcome mean	9.62	6.44	4.99
Outcome std. dev.	4.16	1.07	1.53
Current unemployment estimate mean	11.15	6.21	3.57

Notes: The dependent variable is a respondent's unemployment expectation for the end of 2013 (%). All specifications are estimated using OLS, with robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$. The coefficient tests at the foot of the table report the p value from a two-sided F test of coefficient equality. "High current est." ("Low current est.") voters whose current unemployment estimate is greater (lower) than all treatments levels.

ple disproportionately includes voters with less education and who discuss politics and watch the news rarely.

We find no evidence that treatments differ across individuals with different prior measures of partisanship. First, the interaction coefficients in columns (1) and (2) of Table A12 respectively show that there is no significant difference in response to any treatment across voters that voted for a left party and voters that voted for a right party at the previous (2011) election. Second, columns (3) and (4) of Table A12 similarly show no significant difference when using left and right vote choice in the 2007 election. Together, these results show that there are no differences in response to treatments between left and right partisans.

Even though the *average* partisanship does not differentially respond to different treatments, it remains possible that only sophisticated (or unsophisticated) voters respond differentially to treatment sources. However, Table A13 further shows that there is no significant triple interaction between our treatments, voting for a left party at the previous election, and our voter sophistication indicator. To save space, we omit similar results using our other measures of partisanship.

We also show that we obtain similar, albeit less precise, results when using more standard measures of political sophistication. In particular, we computed a summative rating scale combining standardized measures of education, frequency of watching the news, frequency of discussing politics, self-identification as politically informed, and (log) wages in 2012. ($\alpha = 0.39$). As expected, this index is highly (negatively) correlated with the absolute deviation between the current unemployment estimate and the true rate; regressing the deviation on the index returns a highly statistically coefficient of -1.279 (0.101). Following the analysis in the main paper, we split the sample between low and high sophistication types—as defined, by above and below median scores by our political sophistication scale. The results in Table A14 provide broad support for the findings in the main paper. In particular, for both the 5% and 7% treatments, column (2) shows that sophisticated voters update significantly more when the information is provided by the DCB than political parties. Although the difference is not statistically significant, only the opposition—but

Table A12: Effect of information treatments on unemployment expectations (%), by partisanship

	Outcome: Unemployment expectations			
	(1) Voted left at last election	(2) Voted right at last election	(3) Voted left in 2007 election	(4) Voted right in 2007 election
Control	8.851***	9.130***	9.122***	9.101***
	(0.252)	(0.244)	(0.319)	(0.298)
Partisanship measure	0.326	-0.268	-0.260	-0.213
	(0.370)	(0.373)	(0.439)	(0.445)
DCB 7% treatment (combined)	-0.924***	-1.253***	-1.180***	-1.278***
	(0.268)	(0.261)	(0.338)	(0.314)
× Partisanship measure	-0.404	0.294	0.024	0.234
	(0.394)	(0.397)	(0.466)	(0.473)
Government 7% treatment	-0.550*	-1.007***	-0.917**	-1.025***
	(0.298)	(0.284)	(0.372)	(0.357)
× Partisanship measure	-0.608	0.362	-0.011	0.231
	(0.426)	(0.430)	(0.516)	(0.521)
Opposition 7% treatment	-0.646**	-1.076***	-0.998***	-1.018***
	(0.314)	(0.290)	(0.384)	(0.356)
× Partisanship measure	-0.567	0.346	0.058	0.108
	(0.445)	(0.451)	(0.533)	(0.542)
DCB 5% treatment	-1.420***	-1.759***	-1.932***	-1.782***
	(0.326)	(0.299)	(0.371)	(0.377)
× Partisanship measure	-0.482	0.207	0.348	0.005
	(0.462)	(0.470)	(0.546)	(0.544)
Government 5% treatment	-2.743***	-1.411***	-1.043**	-1.446***
	(0.330)	(0.307)	(0.433)	(0.389)
× Partisanship measure	-0.540	0.453	-0.340	0.533
	(0.467)	(0.472)	(0.580)	(0.591)
Opposition 5% treatment	-1.110***	-1.384***	-1.580***	-1.546***
	(0.324)	(0.320)	(0.394)	(0.370)
× Partisanship measure	-0.449	0.091	0.160	0.075
	(0.472)	(0.469)	(0.556)	(0.564)
Observations	5,705	5,705	3,827	3,827

Notes: The dependent variable is a respondent's unemployment expectation for the end of 2013 (%). The partisanship measure is given at the top of each column. All specifications are estimated using OLS, with robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

Table A13: Effect of information treatments on unemployment expectations (%), by partisanship and voter sophistication

	Outcome: Unemployment expectations
DCB 7% treatment (combined)	-0.577
× Voted left at last election	(0.775)
DCB 7% treatment (combined)	0.617
× Voted left at last election × Sophisticated voter	(0.793)
Government 7% treatment	-1.079
× Voted left at last election	(0.842)
Government 7% treatment	1.267
× Voted left at last election × Sophisticated voter	(0.863)
Opposition 7% treatment	-0.796
× Voted left at last election	(0.869)
Opposition 7% treatment	0.753
× Voted left at last election × Sophisticated voter	(0.891)
DCB 5% treatment	-0.809
× Voted left at last election	(0.941)
DCB 5% treatment	0.915
× Voted left at last election × Sophisticated voter	(0.961)
Government 5% treatment	-0.670
× Voted left at last election	(0.892)
Government 5% treatment	0.796
× Voted left at last election × Sophisticated voter	(0.913)
Opposition 5% treatment	-0.318
× Voted left at last election	(0.917)
Opposition 5% treatment	0.271
× Voted left at last election × Sophisticated voter	(0.940)
Observations	5,705

Notes: The dependent variable is a respondent's unemployment expectation for the end of 2013 (%). Lower order terms are omitted. All specifications are estimated using OLS, with robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

Table A14: Effect of information treatments on unemployment expectations (%), by alternative measure of political sophistication

	Outcome: Unemployment expectations	
	(1)	(2)
	Low pol. sophist.	High pol. sophist.
Control	9.985***	8.081***
	(0.322)	(0.170)
DCB 7% treatment (combined)	-1.798***	-0.507***
	(0.340)	(0.193)
Government 7% treatment	-1.422***	-0.314
	(0.368)	(0.209)
Opposition 7% treatment	-1.694***	-0.214
	(0.370)	(0.243)
DCB 5% treatment	-2.149***	-1.135***
	(0.395)	(0.241)
Government 5% treatment	-2.049***	-0.429
	(0.374)	(0.274)
Opposition 5% treatment	-2.060***	-0.671**
	(0.387)	(0.265)
Coefficient equality F tests (p values)		
DCB 7% = Government 7%	0.07	0.16
DCB 7% = Opposition 7%	0.62	0.11
Government 7% = Opposition 7%	0.29	0.62
DCB 5% = Government 5%	0.74	0.01
DCB 5% = Opposition 5%	0.78	0.07
Government 5% = Opposition 5%	0.97	0.40
Observations	2,852	2,853
Outcome mean	8.36	7.60
Outcome std. dev.	3.97	3.04
Current unemployment estimate mean	9.22	7.94

Notes: The dependent variable is a respondent's unemployment expectation for the end of 2013 (%). All specifications are estimated using OLS, with robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$. The coefficient tests at the foot of the table report the p value from a two-sided F test of coefficient equality. "High pol. sophist." ("Low pol. sophist.") indicate voters whose political sophistication scale scores are greater (lower) than the median of our sample.

not the government—5% claim is significantly negative, and is more than twice the size of the government 5% treatment. Column (1) shows that unsophisticated voters do not significantly differentially update across sources, except marginally in the case of comparing the DCB and government 7% treatments. Unsurprisingly, given the contextual relevance of our measure and its predictive power (see Table A2), these estimates are less precise than our preferred measure.

Comparison of sub-sample characteristics

Table A15 shows the summary statistics in terms of political disposition for the main subsamples that we analyze in the main paper.

Effects of information source on political preferences

Table A16 provides our first stage estimates for vote intention regressions. The results are very similar to the coefficients provided in Table 1 of the main paper, but gain precision due to the inclusion of the current unemployment estimate.

Table A17 shows the reduced form estimates. Panel A, which fully separates treatments, generally shows that the more powerful treatment has a larger effect on support for a political party. That is to say the treatment effects look to be fairly monotonic given the fact that most individuals over-estimated the future unemployment rate relative to the true projection. Although most relationships are not statistically significant, this is due to three reasons. First, as noted in the text, the 7% treatments cause updating from both directions and thus average over countervailing effects. Second, the reduced form averages give greater weight to those with a large first stage, which are generally the individuals who seem to be those least capable of mapping information to political preferences. And finally, we use many treatments and thus relatively small sample sizes for each separate treatment, whereas the 2SLS estimates pool information across treatments. In fact, our 2SLS estimates are highly consistent with these reduced form estimates—it is easy to see this by noting the monontonic relationship between the treatments. This is particularly the case once we

Table A15: Comparison of sub-sample characteristics

	Unsophist. voters		Sophist. voters		Swing voters (previous vote)		Non-swing voters (previous vote)		Swing voters (previous intentions)		Non-swing voters (previous intentions)	
	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.
Follow the news	5.268	1.213	5.509	1.004	5.333	1.162	5.458	1.037	5.351	1.128	5.413	1.100
Wage income 2012 (log)	9.960	5.253	10.379	5.060	10.678	4.756	10.102	5.270	10.427	4.957	10.244	5.140
Frequently discuss politics	2.192	1.132	2.387	1.137	2.268	1.138	2.330	1.139	2.254	1.134	2.318	1.146
Medium education	0.666	0.472	0.665	0.472	0.662	0.473	0.669	0.471	0.663	0.473	0.671	0.470
High education	0.080	0.271	0.131	0.338	0.103	0.304	0.118	0.322	0.105	0.307	0.108	0.310
Math test score (scale)	0.748	0.221	0.807	0.197	0.781	0.209	0.783	0.210	0.782	0.211	0.782	0.209
Voted left at last election	0.493	0.500	0.513	0.500	0.482	0.500	0.526	0.499	0.581	0.494	0.482	0.500
Voted right at last election	0.414	0.493	0.416	0.493	0.351	0.477	0.465	0.499	0.258	0.438	0.461	0.499

Table A16: Effect of information treatments on unemployment expectations (%), controlling for current unemployment estimate (first stage)

	Outcome: unemployment expectations (%)
Control	3.523***
	(0.216)
DCB 7% treatment (combined)	-0.927***
	(0.104)
DCB 5% treatment	-1.506***
	(0.127)
Government 7% treatment	-0.794***
	(0.122)
Government 5% treatment	-1.360***
	(0.126)
Opposition 7% treatment	-0.756***
	(0.120)
Opposition 5% treatment	-1.342***
	(0.137)
Current unemployment estimate	0.631***
	(0.025)
Observations	5,705
First stage F statistic	32.6

Notes: Estimated using OLS, and controlling for current unemployment estimate. Robust standard errors in parentheses. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

group together treatment levels: Panel B shows that grouping together the 5% and 7% treatments across source produces clearly statistically significant results.

Table A18 provides the 2SLS estimates when we do not control for current unemployment expectations. Consistent with our claim in the main text, the results are almost identical, but the first stage is weaker.

Table A19 shows that economic voting also differs by our alternative measure of political sophistication. Columns (1) and (2) compare respondents below and above the political sophistication scale median (defined above), and shows that only the more educated voters vote economically after receiving new economic information. These results provide further support for our finding that sophisticated voters drive economic voting.

Columns (3) and (4) of Table A19 similarly show relatively similar effects by previous vote choice. Given support for the government in the right vote choice sample is very small, such tests are not especially informative. However, a specification instrumenting for the interaction of unemployment expectations with previous vote choice (not shown here) provides clearer results, demonstrating that there is no significant difference.

Furthermore, as noted in the main paper, columns (5) and (6) show that the swing voter results from the main paper are robust to using previous vote intentions to define swing voters.

Although the respondents whose vote intention was affected were not swing voters, they are not ideological extremists. Coding the 17% of the sample who provided the most extreme responses (from either end) to our redistribution question in the 2012 survey, columns (7) and (8) of Table A19 shows that the response of such voters is statistically insignificant and substantially smaller than among non-extreme voters. This result indicates while providing economic information cannot move the most extreme voters, more moderate partisans can be swayed by credible information.

Table A20 show how the effect of unemployment expectations varies by local immigration experiences and respondent views on immigration policy. The results clearly show that there is no

Table A17: Reduced form effect of treatments on political preferences

Panel A: all treatments	(1) Govt.	(2) Soc. Dem.	(3) Soc. Lib.	(4) Soc. Peop.	(5) Conf. govt.	(6) Red-Green	(7) Right	(8) Liberals	(9) Redist.	(10) U. insurance
Control	0.351***	0.150***	0.138***	0.063***	2.732***	0.054***	0.426***	0.308***	3.188***	2.151***
	(0.021)	(0.017)	(0.013)	(0.011)	(0.046)	(0.012)	(0.023)	(0.021)	(0.047)	(0.029)
DCB 7% treatment (combined)	0.019	0.029*	-0.007	-0.003	0.085*	-0.016	-0.016	-0.018	-0.096**	-0.020
	(0.021)	(0.017)	(0.013)	(0.011)	(0.046)	(0.012)	(0.023)	(0.021)	(0.046)	(0.028)
DCB 5% treatment	0.048*	0.014	0.025	0.008	0.102*	-0.010	-0.049*	-0.042*	-0.081	0.009
	(0.025)	(0.019)	(0.016)	(0.013)	(0.052)	(0.013)	(0.026)	(0.024)	(0.053)	(0.032)
Government 7% treatment	0.006	0.010	-0.011	0.007	0.049	-0.006	0.003	0.001	-0.081	-0.009
	(0.024)	(0.019)	(0.015)	(0.013)	(0.052)	(0.014)	(0.026)	(0.024)	(0.053)	(0.032)
Government 5% treatment	0.028	0.008	0.020	-0.001	0.075	-0.006	-0.032	-0.014	-0.033	0.024
	(0.025)	(0.019)	(0.016)	(0.013)	(0.053)	(0.014)	(0.026)	(0.024)	(0.054)	(0.032)
Opposition 7% treatment	-0.012	-0.003	-0.004	-0.005	-0.026	-0.011	0.018	-0.011	-0.072	0.000
	(0.024)	(0.019)	(0.015)	(0.012)	(0.052)	(0.013)	(0.026)	(0.024)	(0.053)	(0.033)
Opposition 5% treatment	0.043*	0.041**	0.002	-0.000	0.206***	-0.008	-0.027	-0.030	-0.061	0.001
	(0.025)	(0.020)	(0.015)	(0.013)	(0.053)	(0.013)	(0.026)	(0.024)	(0.053)	(0.033)
Current unemployment estimate	-0.006***	0.000	-0.005***	-0.000	-0.013***	0.002**	-0.000	-0.001	0.010***	0.009***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)
Observations	5,705	5,705	5,705	5,705	5,688	5,705	5,705	5,705	5,705	5,614
Panel B: combined treatment levels	(1) Govt.	(2) Soc. Dem.	(3) Soc. Lib.	(4) Soc. Peop.	(5) Conf. govt.	(6) Red-Green	(7) Right	(8) Liberals	(9) Redist.	(10) U. insurance
Control	0.351***	0.150***	0.138***	0.063***	2.732***	0.054***	0.426***	0.308***	3.187***	2.151***
	(0.021)	(0.017)	(0.013)	(0.011)	(0.046)	(0.012)	(0.023)	(0.021)	(0.047)	(0.029)
All 7% treatments	0.008	0.017	-0.007	-0.001	0.049	-0.012	-0.003	-0.012	-0.086**	-0.012
	(0.019)	(0.015)	(0.012)	(0.010)	(0.041)	(0.011)	(0.021)	(0.019)	(0.041)	(0.025)
All 5% treatments	0.040**	0.021	0.016	0.002	0.128***	-0.008	-0.036*	-0.029	-0.059	0.011
	(0.020)	(0.016)	(0.013)	(0.010)	(0.043)	(0.011)	(0.021)	(0.020)	(0.043)	(0.026)
Current unemployment estimate	-0.006***	0.000	-0.005***	-0.000	-0.013***	0.002**	-0.000	-0.001	0.010***	0.009***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)
Observations	5,705	5,705	5,705	5,705	5,688	5,705	5,705	5,705	5,705	5,614

Notes: All specifications estimated using OLS, and control for current unemployment expectations. Robust standard errors in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

Table A18: Effect of unemployment expectations on vote intention, without controlling for current unemployment estimate

	(1) Govt.	(2) Soc. Dem.	(3) Soc. Lib.	(4) Soc. Peop.	(5) Right	(6) Liberals
Unemployment expectations (%)	-0.032**	-0.016	-0.014	-0.002	0.031**	0.025*
	(0.014)	(0.011)	(0.009)	(0.007)	(0.015)	(0.014)
Observations	5,705	5,705	5,705	5,705	5,705	5,705
First stage F statistic	10.47	10.47	10.47	10.47	10.47	10.47
Outcome mean	0.32	0.17	0.09	0.06	0.41	0.28
Unem. exp. mean	7.98	7.98	7.98	7.98	7.98	7.98
Unem. exp. std. dev.	3.55	3.55	3.55	3.55	3.55	3.55

Notes: The dependent variables are indicators for intending to vote for (1) a party in the governing coalition, (2) the Social Democratic Party, (3) the Social Liberal Party, (4) the Socialist People's Party, (5) any right-wing party, and (6) the Liberal Party. All specifications are estimated using 2SLS. Robust standard errors are provided in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

significant difference in economic voting by either measure of immigration.

Table A19: Additional heterogeneous effects of unemployment expectations on intending to vote for the government

	Outcome: intend to vote for the government							
	(1) Low pol. sophist.	(2) High pol. sophist.	(3) Voted left at last election	(4) Voted right at last election	(5) Non-swing voter (previous intentions)	(6) Swing voter (previous intentions)	(7) Non-extreme voters	(8) Extreme voters
Unemployment expectations (%)	-0.007	-0.070***	-0.028	-0.001	-0.050***	0.036	-0.054***	-0.002
	(0.015)	(0.025)	(0.020)	(0.007)	(0.019)	(0.027)	(0.019)	(0.028)
Observations	2,852	2,853	2,877	2,367	3,533	1,033	3,782	784
First stage F statistic	18.63	16.04	24.99	9.89	21.78	8.43	23.16	5.18
Outcome mean	0.29	0.36	0.60	0.03	0.33	0.30	0.33	0.30
Unemployment expectations means	8.36	7.60	7.92	7.99	7.83	8.49	7.83	8.49
Unemployment expectations std. dev.	3.97	3.04	3.56	3.48	3.35	4.09	3.35	4.09

Notes: The dependent variable in all specifications is an indicator for voting for a party in the governing coalition. The head of each column defines the subset of respondents that each specification was estimated for. All specifications are estimated using 2SLS, and control for current unemployment expectations. Robust standard errors are provided in parentheses. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

Table A20: Heterogeneous effect of unemployment expectations on incumbent vote intention by immigration exposure

	(1) Govt.	(2) Govt.
Unemployment expectations (%)	0.046	0.004
	(0.103)	(0.096)
× parish immigrant share	-0.008	
	(0.010)	
× municipal immigrant share		-0.004
		(0.009)
Observations	5,704	5,705

Notes: The dependent variable in all specifications is an indicator for voting for a party in the governing coalition. All specifications are estimated using 2SLS, and control for current unemployment expectations. Robust standard errors are provided in parentheses. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$..