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The Effect of Income and Unemployment Shocks on Political Preferences*

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Abstract: Individuals' political preferences are the result of a combination of self-interest and beliefs about how the world works. While it is broadly accepted that expectations about social mobility in the long-run can affect political preferences today, much less is known about how voters update their preferences when reality does not match expectations. The goal of this paper is to understand the dynamics of political preferences over redistribution as new information about individual voters' economic circumstances and experiences arrives in the form of unanticipated shocks to two key determinants of individual welfare: employment and income. We elicit and validate subjective probabilistic expectations over future employment and incomes in the short term and construct measures of anticipated and unanticipated shocks comparing expectations with realized outcomes, measured from third party reports, in a large panel. Our main result, based on fixed effects specifications allowing for interpersonal differences in expectations formation and ability, is that unanticipated economic shocks affect preferences, while anticipated shocks do not. In particular, unanticipated unemployment shocks increase support for unemployment benefits, while realizing expected unemployment has no effects; unanticipated negative income shocks cause voters to diverge, with pro-market voters moving further to the right as a consequence of negative shocks, while anticipated changes have no effect; unanticipated negative economic shocks affect vote intentions, while anticipated shocks do not.

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Introduction

How much redistribution do voters want? The scale and scope of government is a fundamental dimension of politics in advanced industrial societies. People have beliefs about how society and government work, and they have preferences over what government should do, and what they want from it. We ask: what happens to these beliefs and preferences when new information arrives? Our goal is to examine the dynamics of political preferences over redistribution and social insurance as individual voters receive new information about their economic circumstances and experiences in the form of unanticipated shocks to two key determinants of individual welfare: employment and income.

In modern political economy, individual preferences over redistribution reflect people's contemporaneous economic conditions like income (Meltzer and Richard 1981), employment status or labor market risk (Iversen and Soskice 2001), and wealth (Ansell 2014), as well as their beliefs about fairness and the extent to which success reflects effort (Alesina, Glaeser, and Sacerdote 2001).[1] A recent, focused literature shows how beliefs about income mobility across life (Alesina and La Ferrara 2005; Benabou and Ok 2001; Piketty 1995) or across generations (Alesina, Stantcheva, and Teso 2017) affect political preferences. The key insight of this perspective is that people expecting to move up the income distribution prefer less redistribution, even if their current economic situation would predict support for more redistribution. In both cases, individual preferences can be fully characterized by cross-sectional differences, even if one set of such differences – beliefs over future income mobility – have a forward-looking component.

In contrast, little work speaks to whether and how voters *change* their views on redistribution and social insurance over time and what drives such (lack of) change. What happens if expectations over income and future employment are not met – or, in contrast, are borne out entirely? What are the consequences if beliefs about the link between effort and success are challenged by adverse experiences?

[1] See Alesina and Giuliano (2011) for a comprehensive survey of the literature on preferences for redistribution.

In this paper, we adopt the political economy perspective that current and future economic circumstances – in combination with beliefs about fairness and the functioning of the economy – affect preferences over redistribution and combine this with a dynamic perspective of the permanent income/life cycle model from the economics of savings and consumption. We introduce novel measures of unanticipated and anticipated changes in employment and income at the individual level and we show that unanticipated shocks to employment and income affect preferences for redistribution and social insurance, while anticipated changes have no effect, throughout controlling for individual fixed effects. In particular, experiencing unemployment leads voters to support unemployment insurance more, but only if their unemployment spell was unexpected; in contrast, voters who anticipate experiencing unemployment report a higher support for UI *ex ante*, but actually experiencing the expected unemployment does not change their preferences. Negative, unanticipated income shocks cause voters who see effort as being relatively more important for success in life to prefer less redistribution, blaming government involvement for their economic situation. At the same time, such negative shocks leave voters who see luck as being equally or more important than effort for success unaffected, with political divergence (Bullock 2009) resulting.

Our empirical approach is based on combining a probabilistic expectations elicitation approach (Dominitz and Manski 1997; Manski 2004) with administrative data on actual subsequent outcomes. For economic expectations over risk of unemployment and income, political preferences and political beliefs we employ a large-scale Danish panel survey collected 2010-2013. We combine this with third party reported information from Danish administrative registry data on realized individual-level economic outcomes and socio-economic background variables to construct novel measures of unanticipated and anticipated employment and income shocks. From this, we define *unanticipated shocks* as changes in employment status or income that are inconsistent with voters' previously formed expectations, while *anticipated shocks* are consistent with prior expectations. The

panel structure of the data allows us to follow the development of individuals' preferences over time and to estimate fixed effects specifications to handle persistent individual differences in modes of expectations formation (Dominitz and Manski 2011), predictive ability (Alt, Marshall, and Lassen 2016), and optimism (Puri and Robinson 2007) across voters, resulting in observed effects reflecting within-individual variation. Elicitation of subjective probabilistic expectations place strong demands on respondents: we validate the survey expectations measures by showing that subjective expectations over both income and employment have predictive power for actual, realized income and employment, controlling for past experiences and fixed effects. At the same time, we show that expectations measures are internally consistent: People expecting to experience unemployment also expect lower income, controlling for individual and year fixed effects, and unexpected unemployment shocks translate into unexpected income shocks.

We assume these measures of unexpected shocks provide new information to individuals about how the economy works. We show that our results are not due to individual differences in modes of expectation formation or ability, nor are they the result of endogeneity in expectations formation arising from motivated reasoning or perceptual screens. In particular, partisanship, measured as past support for the current incumbent, is not predictive of economic optimism and, in turn, of experiencing an unanticipated shock.[2] As a result, the new information induced by unanticipated shocks has a causal effect on political preference formation.

Our main insights provide new perspectives on economic and partisan voting. Employing a change from centre-right to centre-left government in the middle of the panel, we show that unanticipated shocks affect voting intentions while anticipated shocks do not. In particular, unanticipated unemployment shocks generate economic voting; they negatively affect the propensity to vote for the incumbent, regardless of political color. In contrast, negative income shocks cause voters to engage in partisan voting, supporting center-right political parties, but

[2] Similarly, Alt, Marshall, and Lassen (2016) find no evidence of motivated reasoning among Danish voters processing new information about the working of the macroeconomy.

do not affect economic voting. In sum, accountability for different economic conditions appears to work through different channels.

In additional analyses, we show that results are robust to changes in the assumptions and definitions used to construct unanticipated and anticipated shock measures and we explore the persistence of unanticipated shocks. We also show that the underlying belief about how far success depends on effort or luck is more resistant to shocks than the attitudes it conditions. Furthermore, we show that unanticipated income shocks have a larger effect on preferences over redistribution than on preferences for unemployment insurance, while UI preferences respond more to unanticipated unemployment shocks, not significantly to income shocks, and still less to those income shocks not accompanied by an unemployment shock. Finally, we use the fact the UI membership in Denmark is voluntary as a revealed preference expression of known labor market risks and aversion against such risk, and show that UI members – who by their membership acknowledges some labor market risk – are less affected by an unexpected income shock than non-members, for whom an unanticipated shock may have a qualitative surprise component to it as well; for both groups, anticipated shocks continue to leave them unaffected.

We proceed as follows. The next section provides connections to relevant economic and political economy literature, section three details how we define and elicit shocks using a combination of survey and administrative data, validates these measures, and presents additional data. Section four presents our empirical specifications and discusses identification issues. Results and robustness, and concluding remarks, follow.

Unanticipated shocks, economic conditions, and political preferences

A large literature on the economics of consumption and savings argues that unanticipated income and wealth shocks have causal consequences for consumption and individual welfare (e.g., Campbell and Mankiw 1989; Mian, Rao, and Sufi 2013).

We argue that such unanticipated changes in voters' individual economic situation, in addition to having economic effects, constitute new information to voters about their position and potential vulnerability in the economy, leading voters to update both their political preferences over redistribution and, potentially, to reconsider for whom to vote. These economic surprises are exogenous in the sense that they were unexpected and as such not 'capitalized' into current economic preferences and, therefore, provide a causal estimate of the effect of unanticipated economic shocks on political preferences.

Life cycle, shocks and consumption in economics

The life cycle/permanent income hypothesis is a cornerstone of economic thinking on individual consumption and savings decisions. The basic theoretical setting relates – in a dynamic setting – income streams, wealth and consumption across the life cycle, and extensions allowing for borrowing constraints (Zeldes 1989) and uncertainty (Carroll 1997; Deaton 1991) have provided an organizing workhorse framework for understanding households' economic decisions.

This framework has provided a setting in which to study how economic behavior is affected by new information about the life-cycle paths of income or wealth. In these models, known forecasts of both the consumer's individual economic situation (and of the macro economy) are incorporated into the formation of expectations and affect, through this, current economic decision of consumption and savings. As such, consumers revise their plans and actions only when new information arrives, while "everything known about future changes in policy is already incorporated in present consumption" (Hall 1978, 973). An extensive literature, beginning with Hall, has sought to clarify and be precise about the conditions under which consumers react to new information, including when they are prevented from doing so by borrowing constraints (e.g. Zeldes 1989), and when different decision-making processes are in play (Campbell and Mankiw 1989; Laibson 1997). Our paper contributes to, if from a different angle, recent work to this literature, where the focus is on the effect of income and wealth shocks

on decision-making. In the economics literature, the Great Recession sparked a great deal of interest in explaining consumption and savings responses to shocks to income and wealth (Andersen and Leth-Petersen 2016; Mian, Rao, and Sufi 2013; Paiella and Pistaferri (forthcoming)); in this paper, we apply a similar kind of reasoning based on unanticipated shocks, but focus on political preferences, rather than decisions about consumption and saving.

Beliefs and new information in political economy

In modern political economy, individuals' political preferences are the result of a combination of self-interest and beliefs about how the world works (Alesina and Giuliano 2011; Alesina, Glaeser, and Sacerdote 2001; Gilens 2009) and what is fair (Alesina and La Ferrara 2005). Standard models of political economy feature voters who form political preferences based, in part, on their present (e.g., Meltzer and Richard 1981) and long–run future economic circumstances, including prospects of upward social mobility, the so-called POUM-hypothesis (Benabou and Ok 2001; Rainer and Siedler 2008). Extending this logic to the medium term (Alesina and La Ferrara 2005; Barfort 2017), future economic circumstances, including job prospects and possible income growth or uncertainty, can affect individuals' political preferences in the present through expectations. However, political economy research has yet to examine how voters adjust, or refrain from adjusting, preferences over redistribution and social insurance in response to unanticipated realization of events ("surprises") at the individual level at odds with the expectations studied in the literature.

We combine the explicit focus on expectations and anticipation from the economics of consumption and savings with models of political preference formation. The explicit focus on the role of expectations and experiences in a dynamic setting has two key implications: First, it provides a framework in which we can link expectations to current preferences and be precise about when events such as job or income loss can be considered new information at the level of the individual voter. By this logic, an individual who expects to be unemployed with high probability

in the coming year would include this in his or her optimization problem when deriving present political preferences. Highly probable unemployment decreases expected income relative to the case of full employment throughout the coming year. In standard political economy models of social insurance (e.g., Iversen and Soskice 2001) this would lead to a preference for higher unemployment benefits or more support for job creation efforts; see also Barfort (2017) for micro-level evidence.

In turn, this implies that researchers investigating the formation of political preferences and attitudes cannot treat changes in economic circumstances, for example an observed transition from employment to unemployment, as new information without knowing expectations *ex ante*. In the limiting case, a respondent may know for certain that he will experience unemployment in the near future, having at the time of the preference elicitation received notice of a termination in employment in the future or perhaps, earlier, seen news of future layoffs. Such notices would alter political preferences at the time of elicitation, but the actual realization of unemployment might provide no new information and thus not, for forward looking voters, lead to (additional) changes in political preferences and attitudes.

Second, the dynamic setting allows us to investigate how voters react to anticipated and unanticipated changes in their economic circumstances. By the logic of the basic economic life cycle/permanent income-hypothesis, when voters experience changes in employment status or income that were anticipated ex ante, this should have no effect on future (i.e., $t + 1$) political preferences. In the case of unexpected changes or shocks that arrive as new information, however, it is reasonable to assume that voters adjust their political preferences to reflect their changing circumstances and, possibly, update their views on how the world works. But little is known about the dynamics of political preferences when voters update their political preferences and beliefs as a consequence of personal experiences.

The realization of unexpected economic change can affect political preferences by changing the relative attractiveness of public programs. In addition, this ef-

fect on political preferences can be a function of existing beliefs about how the economic and society works; finally, new experiences can beliefs and ideology. We consider each in turn. In standard models of political preference formation, self-interested voters, agreeing on how the economy works, will adjust political preferences when circumstances change; for example, an increase in the risk of unemployment or the surprise experience of actual unemployment, would, everything else equal, increase support for social insurance (Iversen and Soskice 2001). In this setting, unanticipated shocks will affect political preferences regardless of beliefs and ideology.

If voters disagree on the interpretation of new information – say, whether adverse economic events reflect too little or too much government involvement in the economy – changes in political preferences, or the absence thereof, can reflect motivated correcting (Skitka et al. 2002) or motivated beliefs (Benabou and Tirole 2016; Taber and Lodge 2006). A number of recent papers (e.g., Alesina and Angeletos 2005; Andreoni and Mylovanov 2012; Dixit and Weibull 2007) show how differing interpretations of the same data can lead to divergence and/or polarization of political preferences and beliefs. In our setting, this would show up as the effect of unexpected shocks on political preferences being conditional on prior beliefs about the relative importance of effort and luck in producing economic success.

Finally, experiences, both positive and negative, can lead people to change their broader beliefs, rather than their stated preferences over government policy. In the model proposed by Piketty (1995, 553), voters share distributive goals but "may develop conflicting views about redistribution [...] because through their various mobility experiences they (rationally) happen to learn and to believe different things concerning the incentive costs of redistributive taxation for society as a whole."[3]

[3] In Piketty, social origin, construed as parental income, constitutes a first experience which will differ between individuals; Giuliano and Spilimbergo (2013) show how beliefs can differ between cohorts exposed to different economic circumstances in early adulthood.

Work on how beliefs about individual effort and the possible role of government map onto political preferences often focus on fairness (Alesina and Angeletos 2005; Alesina and Giuliano 2011), and deservingness (e.g., Gilens 2009; Oorschot 2000), suggesting that these two closely related, but not identical, concepts, are critical determinants of preferences for redistribution.

However, work on fairness and deservingness rarely, if ever, investigates how personal experiences, including the type of unanticipated income and employment shocks that we study, affect political preferences.[4] This is surprising, since personal experiences should have high salience and be obvious candidates for experiences that can change both political preferences and one's view of the world. Below, we investigate empirically if and how unanticipated shocks to individuals' own income and employment – life experiences that constitute new information about costs, needs and links between effort and outcomes – affect preferences, both unconditionally and conditionally on beliefs about fairness, as well as beliefs themselves.

Implications for studies of economic change, expectations, and political preferences

Existing studies leave open the question of what happens when unanticipated or unexpected changes in economic circumstances occur. One literature finds that macroeconomic news is important for economic voting and consumer sentiment

[4] One, mainly US-centered exception to this is the literature on "belief in a just world" (Benabou and Tirole 2006; Lerner 1982), originating from cognitive dissonance (Festinger 1957; Jackman 1972). Here, people are assumed to "feel a strong need to believe that they live in a world that is just, in the sense that people generally get what they deserve, and deserve what they get" (Benabou and Tirole 2006, 700) and react to conflicting data by trying to ignore or reinterpret it, resulting in preferences remaining unchanged. However, that literature is exclusively based on people's views on what happens to others, not themselves. Recent work on deservingness focuses on separating welfare or fairness preferences from deservingness; of particular interest for the present study, Aarøe and Petersen (2014) show that Danes and Americans are strikingly similar in welfare preferences once differences in perceived deservingness are accounted for. As far as we know, no work on deservingness and fairness deals with (trying to (re-)interpret) one's own experiences, and only (to our knowledge) Granberg and Nanneman (1986) consider attitude change following unmet expectations, but do so in the context of support for American presidential candidates.

on average (Eggers and Fouirnaies 2016; MacKuen, Erikson, and Stimson 1992). In this vein, a few recent papers inform survey respondents about key economic variables, including macroeconomic conditions and respondents' own position in the income distribution, after eliciting their own beliefs and study the consequences of preferences for redistribution and vote intentions: (Alt, Marshall, and Lassen 2016) inform voters about unemployment rates and show that this affects economic voting but not political preferences. Kuziemko et al. (2015) find that informing American survey respondents about inequality affects views on inequality, but affects preferences over tax and transfer policy much less, possibly owing to a lack of political trust among US voters. One study combines a survey experiment with individually tailored information: Karadja, Mollerstrom, and Seim (2017) show that right-wing voters in Sweden generally underestimate their rank in the income distribution and when told their true placement, based on information from administrative data, move further rightwards. Together, these studies consider the effects of new information about the economy, but do not consider effects of changes in individual economic circumstances.

Another literature considers the effect of individual-level economic changes on (or correlation with) political preferences. Here, findings are conflicting: Margalit (2013) shows that US voters faced with negative income shocks temporarily support additional government redistribution during the Great Recession; in contrast, Rodon and Wiertz (2017) find no effect of economic shocks on left-right placement in a panel of Dutch voters, and Hall, Yoder, and Karandikar (2017) find no effect of foreclosures on incumbent support in a US context. These disparate results could reflect differences across countries, polities or economic shocks; however, our study suggests that such conflicting findings, both between and within literatures, could result from not accounting for whether changes in economic circumstances were anticipated at the time of preference elicitation and, subsequently, not addressing whether such expectations were in fact borne out.[5] Based on our findings, we

[5] An additional complication is that responses may differ depending on whether shocks are seen as systemic or economy-wide or whether they are seen as reflecting individual behaviors.

recommend that studies of dynamic effects of economic shocks elicit expectations and political preferences concurrently and, in follow up rounds, establish the extent to which such expectations were met.

Data and descriptive statistics

Our main data innovation is to combine individuals' economic expectations and political attitudes with uniquely detailed data on actual outcomes as measured from administrative records. Information on economic expectations and political attitudes come from the Danish Panel Study of Income and Asset Expectations (Kreiner, Lassen, and Leth-Petersen 2013), a rolling panel survey of approximately 6,000 individuals beginning in 2010.

Participants in the survey are randomly sampled from individuals in the Danish Central Person Register (CPR) who had any measure of labor market attachment over a five-year period prior to the survey. Each survey lasts on average 10-12 minutes, and is carried out by Epinion A/S who also conduct the official Danish labor force surveys. Average attrition was 31 percent, and new respondents were again sampled randomly from the CPR. Our empirical design requires that we observe individuals in two consecutive surveys, and the effective sample after attrition thus consists of approximately 15,000 individual-year observations.[6]

We use survey data to measure respondents' political attitudes and economic expectations over the calendar year. We use respondents' attitudes toward welfare policy and vote intention as outcome variables. We capture preferences over welfare policy using two questions. The first asks respondents about their preferred level of unemployment benefits, the second their attitudes toward redistribution from the rich to poor. We recode both variables so higher values imply higher demand for government policy. Both questions closely match similar questions found in well-known studies such as the General Social Survey. In addition to these measures,

[6] In the appendix, we examine issues of selective dropout. Applying a reweighting procedure that corrects our regression estimates for attrition shows no evidence that systematic non-response affects the main findings of this paper. See Appendix A7, Tables A2 and A3.

we use a question that asks about respondents' perceptions of the underlying sources of success in life as an indicator for attitudes towards the fairness of market outcomes (Alesina and Angeletos 2005; Piketty 1995). The exact question wording of all survey items used in the analysis can be found in Section one of the appendix.

We show the distribution of the main attitudinal variables in Figure 1. Preferences toward redistribution from rich to poor are measured on a five-point Likert scale. The figure reveals that most individuals prefer the middle category, and that more respondents think it is the job of government to redistribute from the rich to the poor than think government should stay out. Demand for unemployment insurance is measured on a three-point scale. Most respondents think benefits should remain at their current level, but a substantial number of respondents would prefer benefits to be increased. Few respondents prefer lowering benefits from their current level. Beliefs about the underlying sources of success are measured on a three point scale. Most respondents believe that the predominant source of success is effort, and a considerable fraction believe that effort and luck are equally important. Only few respondents think luck is the primary source of success in life.

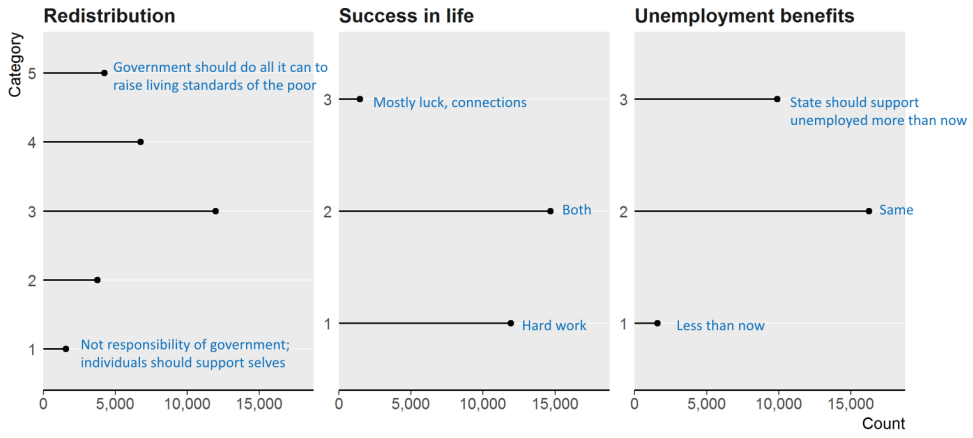

Figure 1: Distribution of attitudinal variables

We merge these survey data with administrative data from Statistics Denmark

through the CPR number. These data are kept on secure servers at Statistics Denmark and can be accessed only under comprehensive security precautions. Only aggregated information such as descriptive statistics or regression coefficients can be extracted. We measure income directly from income-tax returns obtained from the Danish Tax Agency (SKAT). For unemployment, we use data from the Central Register of Labor Market Statistics (CRAM) which are reported directly from government job centers and insurance funds. We append these data with detailed information on a wide variety of background characteristics from several other administrative registers such as the population register. These include detailed demographic information such as age, sex, educational attainment, household composition, etc.

In the resulting data set, we thus observe each survey participant's income and unemployment expectations, current political attitudes, detailed third-party reported information on income and unemployment for all years for which we have measures of expectations, and comprehensive individual-level background information created from administrative data. This data set covers the calendar years 2010-13. In the following section, we describe and validate our expectations measures and the construction of our income and unemployment shock variables.

Measuring subjective expectations and economic shocks

We measure respondents' expectations using probabilistic survey questions which ask respondents to report a set of probabilities that some event will occur. This approach has been found to outperform qualitative approaches in which expectations are elicited using ordinal scales (Hurd 2009; Manski 2004).

We elicit unemployment expectations by asking respondents to provide the best estimate of the probability that they will experience unemployment during the calendar year. We denote the subjective probability of becoming unemployed U^e. Several recent papers provide evidence that individuals have substantial knowledge of future job losses. Using U.S data, Stephens Jr (2004) finds that subjective unemployment expectations predict subsequent job loss. In a recent

paper, Hendren (forthcoming) finds that spouses of individuals who are likely to lose their job are more likely to enter the labor market.

For expected income, we ask respondents to report the minimum and maximum amount they expect to earn during the calendar year. Afterwards, respondents are asked to report the probability that their yearly income will be less than the midpoint between these two numbers. We denote this probability p. Taken together, these answers provide bounds on the support of each individual's probability distribution function, and on the probability mass below the midpoint, but they do not identify these distributions. To proceed we need to impose additional structure on the cumulative distribution function. In this paper, we assume that the distribution function is piece-wise uniform (Attanasio and Augsburg 2016; Attanasio and Kaufmann 2009).

Given this assumption, respondents' expected income can be thought of as a distribution that is the result of mixing two conditional uniform distributions. The first, A, is the "low income" distribution between the minimum and the midpoint, and the second, B, is the "high income" distribution between the midpoint and the maximum. We can calculate expected income for a given calendar year as:[7]

$$Y^e = E[Y] = \mu_A + (1 - p) \times w, \tag{1}$$

where μ_A is the expected value of sampling from the "low income" distribution, and w is the difference in means between the two distributions: $w = \mu_B - \mu_A$.

The unconditional variance of Y can be computed as

$$V[Y] = \sigma^2 + p(1 - p) \times w^2, \tag{2}$$

where the first term, σ^2, is the conditional variance of the two conditional distributions. Because the B distribution is just the A distribution shifted to the right with a factor w, the variance of the two distributions are identical.

Our novel measures of economic shocks involve comparing subjective expectations with actual outcomes, measured ex-post from the administrative registers.

[7] All derivations can be found in Section two of the appendix.

From the survey, we collect economic expectations every January from 2010-13. The timing is chosen to match the timing of the administrative data, which summarizes flow variables such as income or unemployment at the end of the calendar year. This implies that our subjective data match the timing of the outcomes almost perfectly. For example, we can compare expectations of unemployment for the calendar year 2010, elicited in January 2010, with information about actual unemployment measured from administrative data on December 31, 2010. The intuition behind our empirical strategy is as follows: We construct novel measures of income and unemployment shocks using a combination of economic expectations about year t and actual outcomes for that calendar year. We then relate these shock measures to political attitudes and voting intentions measured in January of year $t + 1$, that is, after the shock has occurred. Our data collection strategy allows us relate unemployment and income shocks to political attitudes for four consecutive years. We summarize the timeline of our data collection in Table 1.

Table 1: Data collection timeline

	2010	2011	2012	2013	2014	Source
Expectation						
Income	Y^e_{2010}	Y^e_{2011}	Y^e_{2012}	Y^e_{2013}		Survey (Jan.)
Unemployment	U^e_{2010}	U^e_{2011}	U^e_{2012}	U^e_{2013}		Survey (Jan.)
Outcome						
Income	Y_{2010}	Y_{2011}	Y_{2012}	Y_{2013}		Register (31 Dec.)
Unemployment	U_{2010}	U_{2011}	U_{2012}	U_{2013}		Register (31 Dec.)
Attitude						
Redistribution	R_{2010}	R_{2011}	R_{2012}	R_{2013}	R_{2014}	Survey (Jan.)
Unemployment benefits	UI_{2010}	UI_{2011}	UI_{2012}	UI_{2013}	UI_{2014}	Survey (Jan.)
Vote intention	V_{2010}	V_{2011}	V_{2012}	V_{2013}	V_{2014}	Survey (Jan.)

We now turn attention to our main definition of economic shocks. We create our unemployment shock measure by comparing expectations of unemployment (U^e) during the calendar year with actual unemployment (U). Respondents' unemployment expectations measure the probability respondents assign to a binary

outcome (employed/unemployed). However, unemployment experiences can be markedly different between individuals over a given year. Some are unemployed the entire year while others are unemployed only for a couple of days. Our detailed administrative data allow us to capture this important aspect of unemployment.

Instead of defining an arbitrary threshold for unemployment, we calculate the change in the fraction of time in the labor force an individual has spent unemployed: $\Delta U = U - \text{lag}(U)$. Figure 2 shows a histogram of this variable. The plot shows that a disproportionate fraction of our respondents experience no change in their unemployment status. This is unsurprising as unemployment is a relatively rare event. However, the plot also reveals striking differences among individuals whose unemployment status changed, with some individuals increasing the fraction of the year spent unemployed with more than 50 percent, while others experienced only small changes. The variation around zero suggests that we will be able to estimate differential effects of positive (i.e. moving into employment) and negative (i.e. moving into unemployment) shocks.

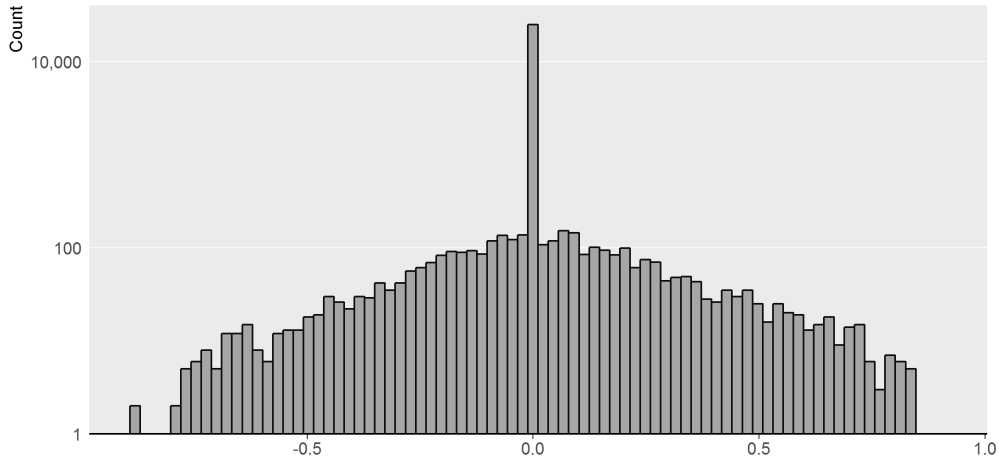

Figure 2: Yearly change in time in labor force spent unemployed

Our measure of an unemployment shock groups individuals based on whether ΔU is positive (i.e. more time spent unemployed), negative (i.e. less time spent

unemployed), or zero (no change), and then interacts these indicator variables of changes in time spent unemployed during the calendar year with the expectation of unemployment measured at the beginning of the year. This setup allows us to differentiate between positive and negative changes in unemployment, and, importantly, to differentiate between anticipated and unanticipated changes.

For income shocks, we first calculate the difference between gross income obtained in a given calendar year, Y, and expected income measured in January of that year: $\theta = Y - Y^e$, where Y^e is computed from Equation (1). We then define an income shock measure using both the expected value and the variance of the expected income distribution. Our main definition of an income shock is defined as follows:

$$S = \begin{array}{ll} S^N & \text{if } \theta < -\sigma^Y, \\ S^P & \text{if } \theta > \sigma^Y, \end{array} \tag{3}$$

where $\sigma^Y = V[Y]^{1/2}$. Intuitively, our shock measure defines an income shock as unanticipated income that exceeds the standard deviation of the expected income distribution. This corresponds to unanticipated income that falls outside the individual's 70 percent confidence interval. Appendix A5, Table A1 shows robustness of our results based on a 90 percent confidence interval.

Our income shock measure has the advantage of explicitly accounting for the uncertainty around respondents' expected income estimates. To see this, consider two individuals who both have $p = 0.5$, but where $(y^e_{min}, y^e_{max}) = (200, 800)$ for individual one and $(y^e_{min}, y^e_{max}) = (400, 600)$ for individual two. Both individuals have $Y^e = 500$, but individual one is much more uncertain about her income than individual two. Our shock measure captures the fact that deviations from expected income are more surprising for individual two than one, precisely because individual two was more certain of her future income. For example, if both individuals earned unanticipated income of 100, only individual two would be classified as having experienced a (positive) income shock. We censor the data by the 2nd and 98th percentile of the income expectations distribution. That is, we

exclude individuals who expect to earn for example 0 DKK (the minimum) and individuals who expect to earn 400 million (the maximum).

In Figure 3, we show the distribution of unanticipated income normalized by the standard deviation of the expected income distribution. The coloring indicates whether individuals were classified as having experienced a negative (light grey) or positive (dark grey) income shock according to Equation (3), or no shock at all (white). The figure shows that most individuals do not experience short term income shocks, and among those who do, more respondents experience small compared to large shocks. We observe considerable variation around zero, which again indicates that we will be able to estimate differential effects of negative and positive income shocks on political attitudes.

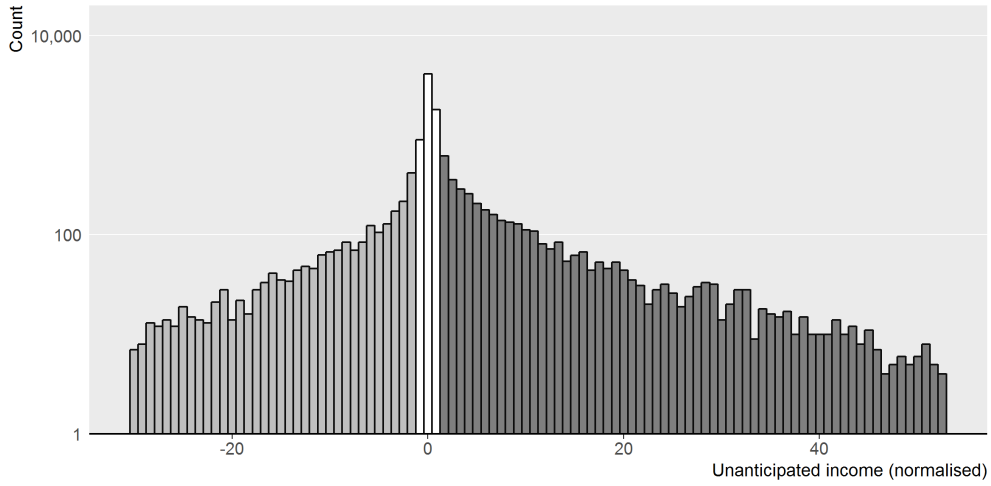

Figure 3: Distribution of income shocks

Validating subjective expectations and economic shocks

We can use the administrative data to validate individuals' expectations of income and unemployment against their true outcomes. We validate the predictive ability of U^e on Danish data regressing subjective unemployment expectations on the fraction of time in the labor force spent unemployed during the year. Results are

presented in Table 2. The table shows that subjective unemployment forecasts predicts actual unemployment with two-way fixed effects and including a rich battery of individual controls, in particular time in labor spent unemployed in the previous year. Column one shows the raw correlation without control variables. Column two and three adds detailed demographic and economic controls, as well as time fixed effects and a lagged dependent variable. Including these detailed batteries of controls reduces the coefficient by about 40 percent, but it remains highly significant at the one-percent level. Columns four and five add individual fixed effects. This only reduces the point estimates by about 5 percent, suggesting that the risk of job loss is largely time-varying within rather than between individuals. The point estimates suggest that for each percentage point increase in the unemployment risk, individuals spend roughly 0.09 percentage points more time unemployed in the subsequent 12 months. Our results thus confirm previous findings from the U.S (Hendren (forthcoming); Stephens Jr 2004) on Danish data.

Table 2: Unemployment expectations predict actual unemployment

	Unemployment (pct of year) [U]				
	(1)	(2)	(3)	(4)	(5)
Unemployment forecast [U^e]	0.145***	0.147***	0.092***	0.083***	0.086***
	(0.023)	(0.024)	(0.023)	(0.031)	(0.031)
Demographic Controls	No	Yes	Yes	No	No
Economic Controls	No	No	Yes	No	Yes
Year FE	No	No	Yes	No	Yes
Lagged Dep. Variable	No	No	Yes	No	No
Model	OLS	OLS	OLS	FE	FE
Observations	18,742	18,642	18,642	18,742	18,742

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). We include gross income as control variable in the fixed effects regressions, in addition to time and individual fixed effects. Robust standard errors clustered at the individual level are provided in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

Turning attention to income, we also investigate the predictive ability of income

expectations in a regression framework. Results are presented in Table 3. We again confirm that subjective expectations capture valuable private information. Even when using administrative data with individual and year fixed effects, and controlling for income in the previous year, each extra point of expected income corresponds to nearly a quarter point of realized income.

Table 3: Income expectations predict actual income

	Gross income [Y]				
	(1)	(2)	(3)	(4)	(5)
Expected income [Y^e]	0.795***	0.692***	0.274***	0.230***	0.214***
	(0.004)	(0.004)	(0.005)	(0.007)	(0.007)
Demographic Controls	No	Yes	Yes	No	No
Economic Controls	No	No	Yes	No	Yes
Year FE	No	No	Yes	No	Yes
Lagged Dep. Variable	No	No	Yes	No	No
Model	OLS	OLS	OLS	FE	FE
Observations	20,942	20,808	20,808	20,942	20,942

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). We include time in labor force spent unemployed as control variable in the fixed effects regressions, in addition to time and individual fixed effects. Robust standard errors clustered at the individual level are provided in parenthesis.
*p < 0.1; **p < 0.05; ***p < 0.01.

As a final way of assessing data validity, we cross-reference the unemployment and income information. First, we evaluate whether respondents' expectations are internally valid. That is, do respondents who expect to become unemployed also have lower expected incomes? As Table 4 shows, the two measures are highly correlated. Going from 0 (no risk of unemployment) to 1 (unemployed with certainty) decreases expected income by about 50-75,000 DKK. This holds with individual and year fixed effects, so it is identified off within-individual variation.

As a second check, we consider the correlation between unanticipated unemployment and unanticipated income. Individuals who experience unanticipated

Table 4: The correlation between expectations measures

	Expected income [Y^e]				
	(1)	(2)	(3)	(4)	(5)
Unemployment forecast [U^e]	−144,726***	−119,038***	−67,431***	−46,497***	−47,422***
	(4,000)	(3,542)	(2,650)	(3,214)	(3,155)
Demographic Controls	No	Yes	Yes	No	No
Economic Controls	No	Yes	Yes	No	Yes
Year FE	No	No	Yes	No	Yes
Model	OLS	OLS	OLS	FE	FE
Observations	15,599	15,526	15,526	15,599	15,599

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). We include gross income and time in labor force spent unemployed as control variable in the fixed effects regressions, in addition to time and individual fixed effects. Robust standard errors clustered at the individual level are provided in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

unemployment should, all else equal, observe a larger discrepancy between realized and expected income. We carry out this analysis by regressing the absolute value of unexpected income, $\theta = Y - Y^e$, normalized by the standard deviation of the expected income distribution, on the absolute value of ΔU, interacted with the respondent's unemployment forecast, U^e. Results are presented in Table 5. Reassuringly, the table shows that individuals with larger absolute changes in unemployment status also have higher absolute deviations between actual and expected income. However, the effect goes away when changes in unemployment were anticipated in the beginning of the year foreshadowing results to be presented in detail below. Nevertheless, as Appendix A4 Figure A2 shows, when we divide individuals according to whether they perceived unemployment risk or not, and whether they experienced an unemployment change that was negative, positive, or zero, all six resulting groups experience small positive income shocks on average, but with widely differing variances.

In sum, our expectation measures have both internal and external validity.

Table 5: Unanticipated unemployment and unanticipated income shocks

	Absolute normalized income shock (abs(θ)/σ)			
	(1)	(2)	(3)	(4)
abs(ΔU)	0.071**	0.100***	0.069**	0.069**
	(0.030)	(0.031)	(0.029)	(0.029)
Unemployment forecast [U^e]	0.090***	0.101***	0.088***	0.088***
	(0.006)	(0.006)	(0.006)	(0.006)
abs(ΔU) × Unemployment forecast [U^e]	−0.099**	−0.110**	−0.103**	−0.103**
	(0.043)	(0.044)	(0.042)	(0.042)
Demographic Controls	No	Yes	Yes	Yes
Economic Controls	No	No	Yes	Yes
Year FE	No	No	No	Yes
Model	OLS	OLS	OLS	OLS
Observations	13,145	13,145	13,098	13,098

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). Robust standard errors clustered at the individual level are provided in parenthesis. *p<0.1; **p<0.05; ***p<0.01.

They are internally consistent with each other, and they have predictive power for both unemployment and income even controlling for very detailed and up-to-date individual-level characteristics and time and individual fixed effects. Furthermore, we also find unanticipated unemployment and income to be internally consistent.

Empirical strategy and identification

Before conducting the empirical analysis, we remove self-employed individuals from the analysis. This is because income and unemployment information for this group is often self-reported, introducing possibly non-random measurement error in our key variables (Kleven et al. 2011).

We estimate the effect of an unemployment shock on attitudes toward social policy using the following empirical model:

$$a_{it+1} = \gamma U^j_{it} + \omega U^e_{it} + \eta U^j_{it} \times U^e_{it} + \beta X_{it} + \tau_t + \alpha_i + \epsilon_{it}, \qquad j \in N, P \quad (4)$$

Here a is individual i's attitude to redistribution or unemployment benefits at time $t+1$. X is a vector of very precise control variables created from administrative data. We only include control variables that are pre-determined at the time expectations are elicited to avoid post-treatment bias. We capture the effect of an unemployment shock of type j (either N: negative, i.e. a one percentage point increase in time spent unemployed, or P: positive, i.e. a one percentage point decrease in time spent unemployed) by interacting U^j with U^e, the respondent's expectation of unemployment at the beginning of the year. The baseline is a respondent who expects with certainty to be employed over the year ($U^e = 0$). The model is estimated using ordinary least squares with robust standard errors clustered at the individual level.

In the case of the effect of an income shock on political attitudes, we estimate the following empirical model:

$$a_{it+1} = \kappa S^j_{it} + \beta X_{it} + \tau_t + \alpha_i + \epsilon_{it}, \qquad j \in (N, P) \quad (5)$$

where S is an income shock of type j as defined in Equation (3). Importantly, we also control for expected income changes, which we calculate as expected income in January of a given year less realized income in the year before. We again estimate the model using ordinary least squares, clustering all standard errors at the individual level.

Do these models estimate causal effects of economic shocks on political attitudes? We consider the following threats to identification: First, unobserved individual-level characteristics relating to expectations formation, including optimism or predictive abilities, might introduce omitted variable bias if such characteristics also correlate with political attitudes. Second, the incidence of economic shocks might be non-random, raising concerns that our estimates fail to generalize to other parts of the population. Third, economic expectations could be

endogenous to political attitudes and party attachment due to motivated reasoning and partisan perceptual screens. We discuss the first two threats in detail below, and defer the discussion of endogeneity due to partisan screens to the robustness section, noting that we find no evidence of such endogeneity.

Dominitz and Manski (2011) document differences in expectations formation of equity-returns. They conclude that expectation formation is interpersonally heterogeneous but intrapersonally stable, that is, the population consists of different expectations types, with each type updating expectations in a different, but stable way (see also Manski 2017). If such differences, whether arising from interpersonal heterogeneity in assigning probability to outcomes – including generalized optimism or pessimism – or in learning, are correlated with political preferences, our estimates could reflect such correlations rather than a causal effect. We account for such differences through controlling for individual fixed effects. This assures that results do not stem from different people forming expectations and forecasting in different (non-measureable) ways.

Political economy theories of redistribution like Meltzer and Richard (1981) emphasize the importance of individuals' relative placement in the income distribution. If income shocks are located non-randomly across the income distribution, for whatever reason, observed correlations between economic shocks and political preferences could reflect standard accounts of redistributive politics, rather than the causal effect of shocks.

In Figure 4, we show the relationship between a respondent's mean income shock, standardized by the standard deviation of the expected income distribution, and his or her mean 1998-2008 income. The figure shows the result of running a local smoother through the raw data (without showing each individual point due to confidentiality restrictions).[8] We overlay the figure with the mean standardized income shock for each income percentile. The figure shows that the lowest part of the 1998-2008 income distribution experience on average slightly larger income

[8] As part of their comprehensive security precautions, Statistics Denmark do not allow plotting of individual level data.

shocks, but except for the bottom and maybe the top decile of the income distribution, income shocks seem to be relatively equally distributed across the different income groups. In the main regressions, we control for individual positions in the income distribution. In the robustness section, we show that omitting the top and bottom ten percent of the income distribution does not matter for our results.

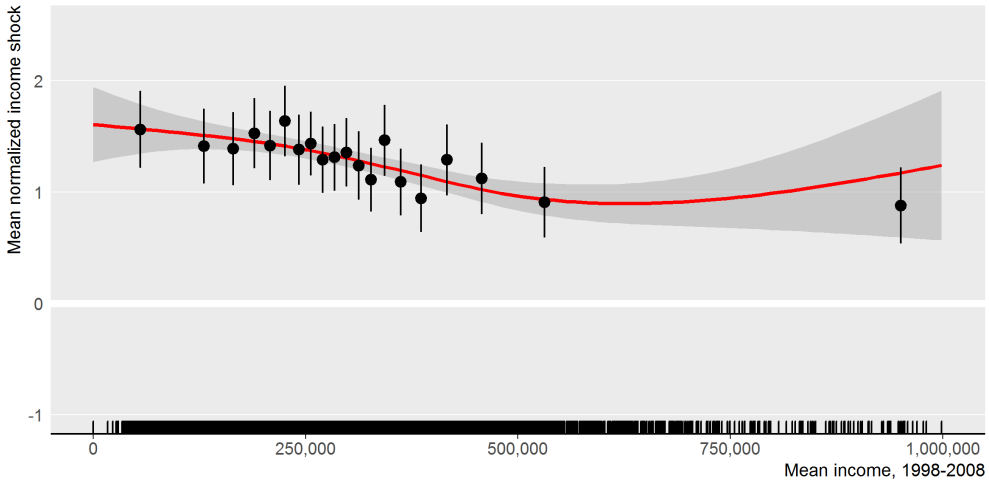

Figure 4: Mean normalized income shocks by income percentiles

Results: The effect of economic shocks on political preferences

Unemployment shocks

The effects of unemployment and income shocks on political preferences can be unconditional, i.e. implicitly assumed to be identical across individuals, or they can be contingent on ideological or partisan predispositions, allowing for voters with different views of the world to interpret shocks differently and to transform such interpretations into political preferences in heterogeneous ways. This section

reports the unconditional results. In the section that follows, we allow the effects of shocks to differ by beliefs.

Table 6 presents our estimates of the effect of unexpected unemployment shocks on political attitudes toward redistribution and unemployment insurance. Columns one through four shows the cross-sectional relationship, while columns five and six show results with individual fixed effects. Column one shows the raw correlations without control variables. Column two adds demographic controls, including age and its square, gender, foreign origin, homeowner, marital status, children, whether in labor force, and education. Column three adds economic controls: observed income and share of time unemployed at the time expectations were elicited. We also control for longer-run income and unemployment experiences by including their 1998-2008 means. Column four adds year fixed effects and a lagged dependent variable to control for persistence in preferences over time. Column five shows the raw associations from estimating with individual fixed effects, so the results do not stem from different people forming expectations in different, non-measureable ways. Column six further adds time-varying economic controls and year fixed effects. To facilitate comparing the effects of income and unemployment shocks, the sample is censored for extreme expected incomes (as described above). Furthermore, we remove individuals who are outside the labor force (who clearly cannot suffer unemployment shocks).

We find no effect of negative or positive unemployment shocks on redistribution preferences. The estimated coefficients are close to zero and change sign depending on the choice of control variables. We find some evidence of an independent effect of unemployment expectations, mirroring results in Barfort (2017). Those feeling more likely to be unemployed in the next year are also more likely to favor more redistribution, but this effect becomes insignificant when year fixed effects are included. Turning to demand for unemployment benefits, we see a similar cross-sectional effect. However, in this case we also see a strong effect of negative unemployment shocks on demand for unemployment benefits. Key to our argument, we find strong evidence that this effect varies according to respondents'

Table 6: Unemployment shocks and expectations: Effects on political attitudes

	A: Redistribution					
	(1)	(2)	(3)	(4)	(5)	(6)
U^P	−0.022	0.024	0.027	0.049	0.096	0.093
	(0.053)	(0.060)	(0.060)	(0.055)	(0.062)	(0.062)
U^N	0.007	−0.004	−0.006	−0.022	0.034	0.020
	(0.068)	(0.068)	(0.068)	(0.062)	(0.077)	(0.079)
U^e	0.081*	0.058	0.071	0.058	0.084	0.095*
	(0.047)	(0.046)	(0.046)	(0.043)	(0.054)	(0.054)
$U^P \times U^e$	0.142	0.128	0.131	0.075	−0.006	0.017
	(0.102)	(0.106)	(0.105)	(0.097)	(0.115)	(0.114)
$U^N \times U^e$	0.004	0.010	0.007	0.021	−0.004	0.016
	(0.105)	(0.104)	(0.104)	(0.095)	(0.114)	(0.114)
Observations	12,940	12,898	12,898	12,898	12,898	12,898
	B: Unemployment benefits					
	(1)	(2)	(3)	(4)	(5)	(6)
U^P	0.048	0.019	0.019	0.017	0.055	0.058
	(0.030)	(0.034)	(0.034)	(0.031)	(0.036)	(0.035)
U^N	0.173***	0.138***	0.138***	0.116***	0.099**	0.092**
	(0.039)	(0.039)	(0.039)	(0.036)	(0.045)	(0.046)
U^e	0.100***	0.077***	0.083***	0.046*	0.028	0.034
	(0.027)	(0.027)	(0.027)	(0.025)	(0.032)	(0.031)
$U^P \times U^e$	0.090	0.007	0.008	−0.026	−0.014	−0.009
	(0.058)	(0.060)	(0.060)	(0.055)	(0.066)	(0.065)
$U^N \times U^e$	−0.113*	−0.120**	−0.124**	−0.108*	−0.116*	−0.113*
	(0.060)	(0.059)	(0.059)	(0.055)	(0.066)	(0.066)
Observations	12,753	12,711	12,711	12,508	12,508	12,508
Demographic Controls	No	Yes	Yes	Yes	No	No
Economic Controls	No	Yes	Yes	Yes	No	Yes
Year FE	No	No	Yes	Yes	No	Yes
Lagged Dep. Variable	No	No	No	Yes	No	No
Model	OLS	OLS	OLS	OLS	FE	FE

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). The controls in the fixed effects regressions are gross income as well as time and individual fixed effects. All models include a constant term (not reported). Robust standard errors clustered at the individual level are provided in parenthesis. *p < 0.1; **p < 0.05; ***p < 0.01.

expectation of unemployment. The *more* the respondent anticipates being unemployed, the smaller is the estimated effect of subsequent actual unemployment on the demand for unemployment benefits. Using the coefficients in column two as an example (the baseline is individuals whose share of time in unemployment changed less than one percentage point), a higher share of time spent unemployed increases demand for unemployment benefits *among respondents who did not anticipate becoming unemployed*: for them the effect (+.138) is 45 per cent larger than the effect of expected unemployment, which is (combining coefficients .138, .077, and -.124) equal to +.095. This pattern and relative magnitudes, which controls for economic circumstances at the time expectations were elicited, persists across the other columns of the table, and are unaffected by the inclusion of individual fixed effects. In fact, in the final column that includes individual and year effects, as well as time-varying economic controls, the effect of unexpected unemployment is +.093, while for expected unemployment it is .013.

We illustrate this result in Figure 5, where the marginal effect of experiencing an increase in unemployment, estimated from column 2 in Table 6, is plotted at different values of U^e. The figure confirms the results from the table, but it reveals an additional insight: increases in unemployment are only significantly related to demand for unemployment benefits among respondents for whom unemployment was unanticipated. When respondents have strong enough expectations of unemployment ($U^e > 0.5$), the effect becomes indistinguishable from zero. The figure also confirms the overall negligible marginal effect of an unemployment increase on attitudes to redistribution, more or less regardless of the level of expected unemployment.

By insurance category

A key revealed preference measure of unemployment concerns is membership of an unemployment insurance fund. Denmark is characterized by a system known as *flexicurity*, combining weak employment protection (flexibility) with a voluntary UI system considered generous by international standards (security). The UI

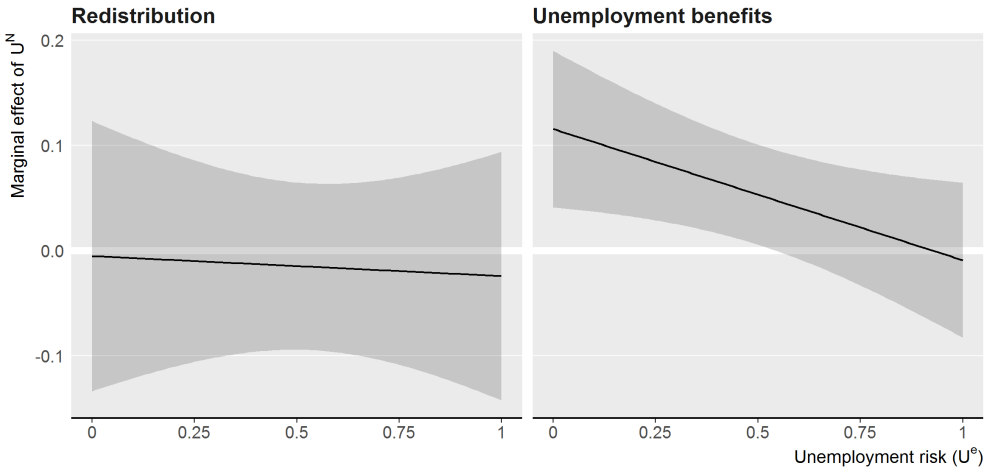

Figure 5: Unemployment expectations moderate relationship between experienced unemployment and political preferences

system is financed through the progressive tax system and provides 100 percent income replacement until a cap at approximately 3000 USD per month, with a maximum duration of two years. People in the labor force but not in the UI system, either by choice or because benefits have run out, receive cash welfare.

Table 7 shows results splitting the sample by UI membership. Echoing results from Table 6, there is no effect of unemployment shocks on preferences over redistribution. The effects are larger for the uninsured, perhaps because having insurance removes the effect of reduced income on preferences, but also because we are not controlling here for beliefs. Looking at UI benefits, the effect of expected unemployment on support for additional UI benefits is positive, significant, and has essentially the same pattern of signs across insured and non-insured individuals. The positive coefficient is consistent with adverse selection in the sense that people expecting to become unemployed would like more, or more generous, coverage at given prices. At the same time, the equality across uninsured and insured groups suggests that they have chosen unemployment insurance to reflect relative utility risks of unemployment.

Table 7: The effect of unemployment shocks by insurance category

	A: Redistribution							
	Insured				Uninsured			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
U^P	−0.038	0.019	0.054	0.097	0.150	−0.118	−0.343	−0.266
	(0.054)	(0.060)	(0.056)	(0.063)	(0.337)	(0.484)	(0.444)	(0.515)
U^N	−0.029	−0.035	−0.035	0.044	0.395	0.170	0.010	0.537
	(0.070)	(0.070)	(0.064)	(0.083)	(0.269)	(0.267)	(0.245)	(0.489)
U^e	0.036	0.062	0.064	0.114*	0.225**	0.109	0.042	0.110
	(0.053)	(0.053)	(0.048)	(0.060)	(0.100)	(0.101)	(0.093)	(0.147)
$U^P \times U^e$	0.177*	0.122	0.058	−0.051	0.887	1.173*	0.711	2.535
	(0.106)	(0.109)	(0.101)	(0.120)	(0.607)	(0.633)	(0.582)	(1.798)
$U^N \times U^e$	0.075	0.038	0.028	−0.045	−0.480	−0.256	−0.044	−0.739
	(0.110)	(0.109)	(0.101)	(0.122)	(0.375)	(0.368)	(0.339)	(0.703)
Observations	11,223	11,190	11,190	11,190	1,717	1,708	1,708	1,708
	B: Unemployment Benefits							
	Insured				Uninsured			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
U^P	0.036	0.006	0.009	0.052	0.050	−0.035	−0.097	0.394
	(0.030)	(0.034)	(0.031)	(0.037)	(0.197)	(0.280)	(0.260)	(0.293)
U^N	0.155***	0.118***	0.099***	0.078	0.450***	0.374**	0.342**	0.640**
	(0.039)	(0.039)	(0.036)	(0.049)	(0.149)	(0.147)	(0.136)	(0.269)
U^e	0.085***	0.089***	0.049*	0.030	0.184***	0.097*	0.062	0.090
	(0.031)	(0.031)	(0.029)	(0.036)	(0.056)	(0.056)	(0.053)	(0.083)
$U^P \times U^e$	0.096	−0.013	−0.040	−0.010	0.392	0.680*	0.430	0.333
	(0.060)	(0.062)	(0.058)	(0.069)	(0.344)	(0.352)	(0.328)	(1.002)
$U^N \times U^e$	−0.090	−0.126**	−0.104*	−0.088	−0.572***	−0.440**	−0.372*	−1.070***
	(0.062)	(0.061)	(0.057)	(0.071)	(0.208)	(0.203)	(0.191)	(0.387)
Observations	11,093	11,060	10,882	10,882	1,692	1,683	1,654	1,654
Demographic Controls	No	Yes	Yes	No	No	Yes	Yes	No
Economic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Lagged Dep. Variable	No	No	Yes	No	No	No	Yes	No
Model	OLS	OLS	OLS	FE	OLS	OLS	OLS	FE

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). The controls in the fixed effects regressions are gross income as well as time and individual fixed effects. All models include a constant term (not reported). Robust standard errors clustered at the individual level are provided in parenthesis. *p < 0.1; **p < 0.05; ***p < 0.01.

However, the effect of experiencing increasing unemployment differs substantially across groups: both insured and uninsured individuals support initiatives for the unemployed significantly more following increased unemployment, but for the uninsured this effect is between three and four times larger than for the insured, with the difference being significant. One possible reason for this result is that individuals choosing not to be a member of the UI system have a lower estimate of unemployment risk than do members and, hence, are on average more surprised when unemployment in fact hits. However, Appendix A3, Figure A1 shows that this is not exactly right: the uninsured are indeed somewhat more likely to predict no unemployment for themselves, but are also more likely to be certain of becoming unemployed, so perhaps the cost of insurance or heterogeneity in risk aversion is also a factor. Nevertheless, combining the similar effects of expected unemployment and the differing effects of experiencing more unemployment, we see that the interaction between the two follows a similar pattern: both for the insured and the uninsured, only surprise unemployment matters, while realizing expected unemployment does not affect preferences.

Income shocks

Parallel results for positive and negative income shocks are presented in Table 8. Income shocks can, obviously, result from unexpected unemployment, but can also be the result of lack of (expected) wage increases, transition to a lower paying, but safer, job without intermediate spells of unemployment, or periods of sickness leave with less than 100 percent replacement rate, etc. Columns one through four add the same control variables as in the previous table. Column five shows the raw within estimates, and column six adds economic control variables and year fixed effects.

First, note that anticipated income changes are uncorrelated with political preferences, consistent with the evidence that such expected changes are reflected in preferences already formed at the time of elicitation. Individuals who experience a negative income shock support lower redistribution and are less inclined to

Table 8: The effect of income shocks on political attitudes

	A: Redistribution					
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.104***	−0.082***	−0.084***	−0.064***	−0.052**	−0.046*
	(0.024)	(0.024)	(0.024)	(0.021)	(0.025)	(0.025)
S^P	0.062***	0.053**	0.053**	0.027	−0.029	−0.032
	(0.021)	(0.021)	(0.021)	(0.019)	(0.022)	(0.022)
Expected income change	−0.0003*	−0.0002	−0.0002	−0.0002	−0.0002	−0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)
Observations	13,690	13,640	13,262	13,262	13,690	13,690
	B: Unemployment benefits					
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.061***	−0.038***	−0.042***	−0.020*	0.010	0.014
	(0.013)	(0.013)	(0.013)	(0.012)	(0.014)	(0.014)
S^P	0.016	0.016	0.017	0.013	0.003	−0.001
	(0.011)	(0.011)	(0.011)	(0.010)	(0.012)	(0.012)
Expected income change	−0.0001	−0.0001	−0.0001	−0.00004	−0.0001	−0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Observations	13,495	13,445	13,071	12,853	13,495	13,495
Demographic Controls	No	Yes	Yes	Yes	No	No
Economic Controls	No	No	Yes	Yes	No	Yes
Year FE	No	No	No	Yes	No	Yes
Lagged Dep. Variable	No	No	No	Yes	No	No
Model	OLS	OLS	OLS	OLS	FE	FE

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). The controls in the fixed effects regressions are gross income and time in labor force spent unemployed, as well as time and individual fixed effects. Robust standard errors clustered at the individual level are provided in parenthesis. *p < 0.1; **p < 0.05; ***p < 0.01.

support increases in unemployment insurance; but only the former result holds once we take account of individual fixed effects. One possible interpretation, to which we return below, is that voters hit by negative income shocks, who perceive redistribution to be for the benefit of undeserving recipient and, at the same time, do not see themselves as beneficiaries of redistributive programs. There are no robust results for positive income shocks.

Updating and ideology: Heterogenous effects of shocks on political attitudes

Beliefs potentially guide how people interpret events such as unemployment or income loss and their political response to such events. Would one hold a different opinion about unemployment benefits if one felt unlucky rather than deserving of the unemployment shock? Figure 6 displays the marginal effects analogous to Figure 5 but with observations separated by belief in the fairness of market outcomes. It shows that unemployment shocks, unlike income shocks, do not induce significantly different (or any) response in redistribution preferences. Turning attention to unemployment benefits, there is some evidence that the effect is larger for individuals who believe effort is the most important factor for success in life (that subgroup effect is statistically significant), but the difference between the two groups is not statistically significant. This is consistent with a self-interest argument: you benefit yourself in the case of unemployment benefits rather than feeling the tax cost in the case of redistribution preferences, but further investigation into the roles of income shocks and possibly unemployment insurance are needed.

For income shocks, we investigate the conditional effect of shocks by beliefs using the following regression model:

$$a_{it+1} = \kappa S^j_{it} + \gamma B_{it} + \zeta S^j_{it} \times B_{it} + \beta X_{it} + \tau_t + \alpha_i + \epsilon_{it}, \qquad j \in (N, P), \quad (6)$$

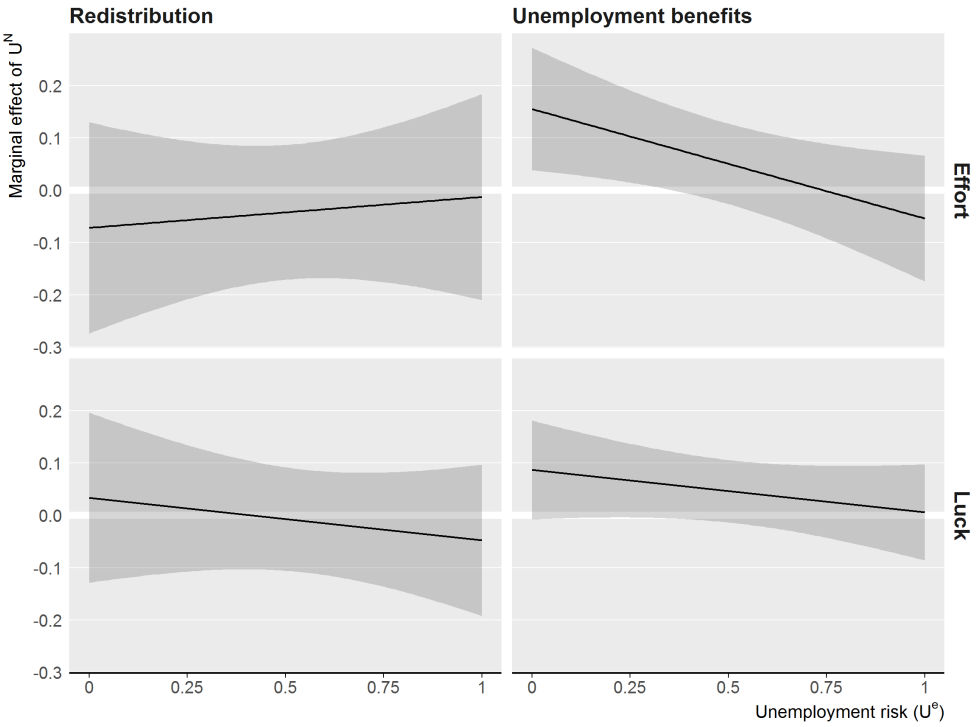

Figure 6: Divergence by issue type and beliefs: Unemployment shocks

where B is an indicator value that takes the value one if the individual thinks that luck is at least as important as effort for success in life, and zero otherwise. It is important to note that beliefs are measured at the same time as we elicit expectations to avoid introducing post-treatment bias. We again include two-way fixed effects and cluster standard errors at the individual level.

We show the full results of the regression model in Table 9. The table reveals an important asymmetric effect of income shocks on both indicators of welfare policy preferences. Individuals who believe that effort determines success in life are less likely to support redistribution than are those who believe luck and effort are at least equally important. This effect persists until individual fixed effects are included: there is not enough change at the individual level in these beliefs to identify an overall effect. Those who believe that success is a matter of effort also

decrease their demand for welfare policies when they experience a negative income shock, but the combined effect is estimated almost exactly at zero for individuals who believe luck and effort are at least equally important. We find essentially no response of either group to a positive income shock. While the effects are largely consistent across general redistribution and unemployment benefits, an important difference is that the effects only hold with individual fixed effects for redistribution preferences.

To emphasize that income shocks produce (more) divergent results for redistribution than for unemployment insurance, Figure 7 graphs the marginal effects of a negative income shock and its interaction with beliefs. The first panel plots the effects on redistribution preferences. It is immediate that with respect to general redistribution, there is a gap between those who believe that effort is all that counts and those who do not, regardless of which variables and fixed effects are controlled. Those who believe in effort significantly want less redistribution when they suffer a negative income shock, and those who do not believe in effort generally do not want significantly less redistribution. This is a difference in marginal effects, not due to the persistent effect of any overall difference attributable to ideology, but rather it follows from the interaction of unanticipated income shocks and beliefs.

The second panel plots the effects for unemployment benefits. Here the separation between ideological groups induced by negative income shocks is much smaller, and not always clearly significant. When we add time-fixed effects, no significant differences are identified. Recall from Figure 5 that preferences for more unemployment benefits respond strongly to unanticipated unemployment. It is quite reasonable that unemployment shocks should trigger larger unemployment benefits responses. It is also noteworthy how much less these responses depend on beliefs than was the case with general redistribution: those experiencing unanticipated unemployment are more likely to see themselves as gaining from an increase in benefits.

Table 9: Income shocks, beliefs and divergence

	A: Redistribution					
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.219***	−0.172***	−0.146***	−0.109***	−0.091**	−0.082**
	(0.037)	(0.037)	(0.038)	(0.035)	(0.041)	(0.041)
S^P	0.031	0.040	0.048	0.031	0.045	0.033
	(0.031)	(0.031)	(0.032)	(0.029)	(0.033)	(0.033)
Luck [B]	0.333***	0.299***	0.291***	0.172***	−0.036	−0.032
	(0.023)	(0.023)	(0.023)	(0.022)	(0.027)	(0.027)
Expected income change	−0.000	−0.000	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$S^N \times$ Luck [B]	0.141***	0.140***	0.122**	0.115**	0.136**	0.144***
	(0.051)	(0.050)	(0.051)	(0.047)	(0.054)	(0.053)
$S^P \times$ Luck [B]	−0.001	−0.003	−0.007	0.004	0.002	0.008
	(0.041)	(0.041)	(0.041)	(0.038)	(0.043)	(0.043)
Observations	13,690	13,640	13,262	13,262	13,690	13,690
	B: Unemployment benefits					
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.142***	−0.103***	−0.093***	−0.064***	−0.012	−0.010
	(0.021)	(0.021)	(0.021)	(0.020)	(0.023)	(0.023)
S^P	0.003	0.015	0.025	0.016	0.009	0.004
	(0.018)	(0.018)	(0.018)	(0.017)	(0.019)	(0.019)
Luck [B]	0.173***	0.155***	0.144***	0.079***	−0.001	−0.006
	(0.013)	(0.013)	(0.013)	(0.012)	(0.015)	(0.015)
Expected income change	−0.000	−0.000	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$S^N \times$ Luck [B]	0.065**	0.057**	0.042	0.059**	0.002	0.011
	(0.029)	(0.029)	(0.029)	(0.027)	(0.031)	(0.031)
$S^P \times$ Luck [B]	−0.009	−0.013	−0.017	−0.011	−0.035	−0.028
	(0.024)	(0.023)	(0.024)	(0.022)	(0.025)	(0.025)
Observations	13,495	13,445	13,071	12,853	13,495	13,495
Demographic Controls	No	Yes	Yes	Yes	No	No
Economic Controls	No	No	Yes	Yes	No	Yes
Year FE	No	No	No	Yes	No	Yes
Lagged Dep. Variable	No	No	No	Yes	No	No
Model	OLS	OLS	OLS	OLS	FE	FE

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). The controls in the fixed effects regressions are gross income and time in labor force spent unemployed, as well as time and individual fixed effects. Robust standard errors clustered at the individual level are provided in parenthesis. *p < 0.1; **p < 0.05; ***p < 0.01.

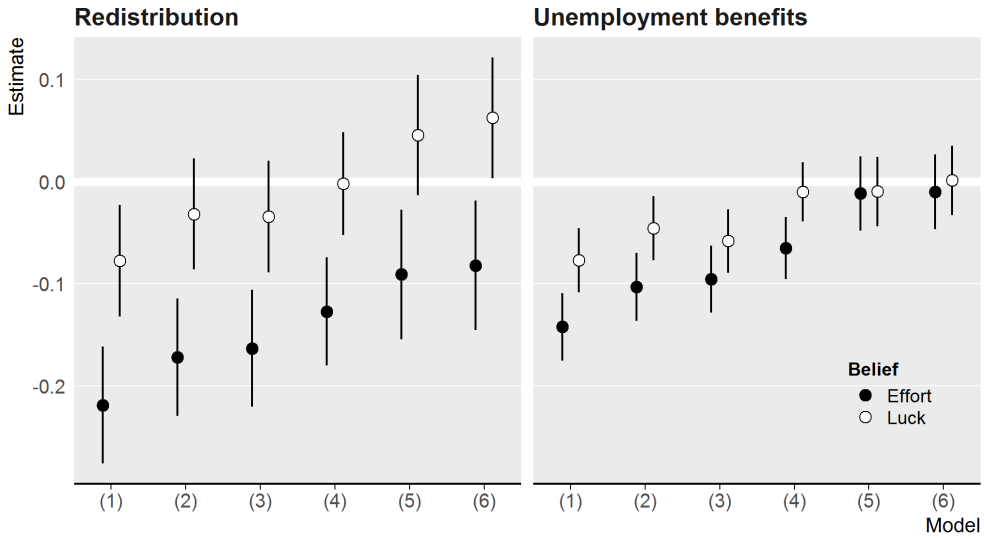

Figure 7: Divergence by issue type and beliefs: Income shocks

Economic shocks and vote intentions

Next, we reestimate Equation (4) and Equation (5) using respondents' stated vote intention as outcome variable. We investigate the effect of economic shocks on two distinct outcomes: whether respondents support the incumbent block and whether they support the center-right or center-left coalition of parties in Denmark, noting that results are robust to various definitions of such coalitions.[9] Both outcomes are binary and all models are estimated with a linear probability model with robust standard errors. We report estimates first for unemployment shocks and then for income shocks.

Table 10 presents results for unemployment shocks. The first three columns report results for incumbency support and the last three report results for center-right support. Columns one through three add economic and demographic controls, and year fixed effects. Unsurprisingly, given the large economic voting literature,

[9]Denmark held a national election in September of 2011 that saw the incumbent centre-right led government coalition replaced with a centre-left coalition. This change in incumbency allows us to distinguish incumbency effects from general left-right effects.

we observe a strong effect of a negative unemployment shock on incumbent support. However, consistent with the results for unemployment insurance in Table 6, the effect is reduced and finally becomes non-existent as individuals increasingly expected that experiencing unemployment was more probable. This is, we believe, a new result in the literature on political accountability for economic outcomes. It suggests that incumbents make the case for governments' expectations management: if incumbents can convince voters that the employment outlook is bad, increasing unemployment may not have a detrimental effect on political support. Turning attention to columns four to six, we find no evidence that negative unemployment shocks affect support for center-right parties. We detect some evidence of a positive effect of positive shocks to unemployment, but the effect is insignificant once we add year fixed effects.

Results for income shocks are presented in Table 11. We find little evidence that income shocks affect incumbent support. However, turning attention to support for the center-right, we find a strong conditional effect of negative income shocks conditional on beliefs, similar to those reported in Table 9. Negative income shocks make respondents who hold pro-market ideologies more likely to support the center-right, whereas we find no effect for individuals who believe market outcomes are at least partly due to luck. The is relatively consistent across specifications and suggests that a negative income shock increases center-right support for pro-market individuals approximately five percentage points, or about 10 percent.

In sum, incumbents, regardless of political color, are punished for unemployment, since this is not really seen as an ideological topic: politicians of both Left and Right want more jobs. On the other hand, income shocks affect preferences for redistribution of resources in society, which is about political ideology rather than the economic stewardship associated with job creating.

Table 10: Unemployment shocks and vote intentions

	Incumbent block			Centre-right		
	(1)	(2)	(3)	(4)	(5)	(6)
U^P	−0.026	−0.006	0.014	0.047*	0.059*	0.025
	(0.028)	(0.032)	(0.032)	(0.028)	(0.032)	(0.031)
U^N	−0.093**	−0.094**	−0.077**	−0.006	0.017	−0.006
	(0.038)	(0.039)	(0.038)	(0.038)	(0.038)	(0.036)
U^e	0.042**	0.037*	0.036*	−0.043**	−0.013	−0.010
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.019)
$U^P \times U^e$	0.083*	0.112**	0.085	−0.089*	−0.114**	−0.077
	(0.050)	(0.053)	(0.052)	(0.050)	(0.052)	(0.050)
$U^N \times U^e$	0.105*	0.116**	0.106*	−0.021	−0.040	−0.025
	(0.056)	(0.056)	(0.055)	(0.056)	(0.056)	(0.053)
Economic Controls	No	Yes	Yes	No	Yes	Yes
Demographic Controls	No	Yes	Yes	No	Yes	Yes
Year FE	No	No	Yes	No	No	Yes
Model	LPM	LPM	LPM	LPM	LPM	LPM
Observations	11,851	11,851	11,804	11,851	11,851	11,804

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). *p < 0.1; **p < 0.05; ***p < 0.01.

Robustness and extensions

How certain can we be that income and unemployment shocks affect political attitudes? The following brief section presents additional robustness check of our main results.

First, we make sure that our income shock results are not driven by individuals located at the extremes of the income distribution by omitting the top and bottom percentile of individuals based on their mean 1998-2008 income. Table 12 shows that the overall pattern of results remain. All signs of significant coefficients in Table 9 are unchanged, though standard errors, reflecting a smaller sample size, are larger. Overall, we conclude that the results on belief divergence that we report

Table 11: Income shocks and vote intentions

	Incumbent block			Centre-right		
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.029	−0.026	−0.023	0.080***	0.071***	0.057***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.016)
S^P	0.031*	0.026	0.020	−0.042**	−0.037**	−0.016
	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)	(0.015)
Luck [B]	0.125***	0.111***	0.061***	−0.187***	−0.159***	−0.089***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.017)	(0.016)
Expected income change	−0.0001	−0.0001	−0.0001	0.0001	0.0001	0.00004
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
$S^N \times$ Luck [B]	0.014	0.011	0.013	−0.044*	−0.040*	−0.042*
	(0.025)	(0.025)	(0.024)	(0.025)	(0.023)	(0.022)
$S^P \times$ Luck [B]	−0.006	−0.013	−0.008	−0.0003	0.008	0.001
	(0.022)	(0.022)	(0.021)	(0.022)	(0.021)	(0.019)
Economic Controls	No	Yes	Yes	No	Yes	Yes
Demographic Controls	No	Yes	Yes	No	Yes	Yes
Year FE	No	No	Yes	No	No	Yes
Model	LPM	LPM	LPM	LPM	LPM	LPM
Observations	11,851	11,851	11,804	11,851	11,851	11,804

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). *p < 0.1; **p < 0.05; ***p < 0.01.

are not driven by discrepant behavior among the very poor or very rich.

Next, we consider the potential endogeneity of our shocks measure to respondents' political preferences. Our key hypothesis that economic shocks affect political preferences could be challenged if, for example, right-wing voters hold more optimistic income or unemployment expectations under a centre-right government. This would make our shock measures endogenous to respondents' party preferences and thus invalidate our identification strategy.

In Table 13, we investigate in detail the potential endogeneity of our income shock measure to respondents' party attachment. In columns one and two, we predict the absolute size of the income shock by whether or not the respondent

Table 12: Income shocks: Robustness to outliers

	A: Redistribution					
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.183***	−0.130***	−0.131***	−0.082*	−0.036	−0.028
	(0.045)	(0.045)	(0.046)	(0.042)	(0.050)	(0.050)
S^P	0.024	0.039	0.049	0.028	0.046	0.039
	(0.036)	(0.036)	(0.036)	(0.034)	(0.039)	(0.039)
Luck [B]	0.304***	0.263***	0.261***	0.157***	−0.030	−0.024
	(0.026)	(0.026)	(0.026)	(0.024)	(0.030)	(0.030)
Expected income change	−0.000*	−0.000	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$S^N \times$ Luck [B]	0.135**	0.127**	0.120**	0.096*	0.101	0.106
	(0.061)	(0.061)	(0.061)	(0.057)	(0.065)	(0.065)
$S^P \times$ Luck [B]	−0.031	−0.031	−0.035	−0.012	0.001	0.004
	(0.047)	(0.047)	(0.047)	(0.044)	(0.050)	(0.050)
Observations	9,883	9,842	9,842	9,842	9,883	9,883
	B: Unemployment benefits					
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.144***	−0.105***	−0.101***	−0.068***	−0.022	−0.021
	(0.026)	(0.026)	(0.026)	(0.024)	(0.028)	(0.028)
S^P	−0.003	0.009	0.018	0.006	−0.013	−0.014
	(0.021)	(0.021)	(0.021)	(0.019)	(0.022)	(0.022)
Luck [B]	0.147***	0.125***	0.120***	0.061***	−0.008	−0.013
	(0.015)	(0.015)	(0.015)	(0.014)	(0.017)	(0.017)
Expected income change	−0.000	−0.000	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$S^N \times$ Luck [B]	0.085**	0.074**	0.057*	0.070**	0.029	0.037
	(0.035)	(0.035)	(0.034)	(0.032)	(0.037)	(0.037)
$S^P \times$ Luck [B]	−0.013	−0.015	−0.024	−0.011	−0.016	−0.012
	(0.027)	(0.027)	(0.027)	(0.025)	(0.028)	(0.028)
Observations	9,734	9,693	9,693	9,530	9,734	9,734
Demographic Controls	No	Yes	Yes	Yes	No	No
Economic Controls	No	No	Yes	Yes	No	Yes
Year FE	No	No	No	Yes	No	Yes
Lagged Dep. Variable	No	No	No	Yes	No	No
Model	OLS	OLS	OLS	OLS	FE	FE

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). The controls in the fixed effects regressions are gross income and time in labor force spent unemployed, as well as time and individual fixed effects. Robust standard errors clustered at the individual level are provided in parenthesis. *p < 0.1; **p < 0.05; ***p < 0.01.

intended to vote for the incumbent at the time his or her expectations were elicited. In columns three and four, we predict whether or not the respondent is classified as having experienced an income shock. If incumbent supporters form (unreasonable) expectations then we would expect to observe a correlation between shocks and incumbent support. As the first four columns make clear, we find no evidence of such a relationship. However, this could be because pro-incumbent supporters are very optimistic, and therefore more likely to get negative shocks, and anti-incumbent supporters very pessimistic, and therefore more likely to get positive shocks. In columns five through eight, we focus on whether respondents get positive or negative income shocks and, again, find no evidence that incumbent support is predictive of either.

Table 13: Non-endogeneity of income shocks with respect to past vote

	$\text{abs}(\theta)$		$P[\text{abs}(\theta) > \sigma]$		$P[S^N = 1]$		$P[S^P = 1]$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Support incumbent block	0.003	0.002	0.009	0.003	0.001	−0.003	0.008	0.006
	(0.003)	(0.003)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)
Demographic Controls	No	Yes	No	Yes	No	Yes	No	Yes
Economic Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	11,920	11,854	11,920	11,854	11,920	11,854	11,920	11,854

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Persistence of shock effects

Unemployment shocks

Figure 5 presented the marginal effects of an unemployment shock in time period t on political attitudes in period $t + 1$. An important question is how long such shocks persist. We estimate with a fixed sample throughout component panels, allowing possibility of repeat shocks to avoid post-treatment bias.[10] The first (lefthand) panels of Figure 8 summarize the marginal effects (with economic controls) for the base case of an unemployment shock on beliefs about luck and attitudes about redistribution and unemployment benefits. The effect in the case of unemployment benefits is both larger than the others, and is significant, as we saw above, while the effect on redistribution preferences is not, nor is the effect on the underlying belief in luck.[11] What does an unemployment shock, an unanticipated increase in time unemployed, predict for attitudes in year $t + 2$? For unemployment benefits in panel 2, the extent to which the effect of the shock depends on unemployment risk attenuates, and still more for attitudes in $t + 3$, consistent with the simplest models in which the effect of the shock damps away exponentially in each subsequent period. By the last year, any effect on attitudes is small and insignificant, and no longer relates in any way to the extent to which the initial unemployment shock was unanticipated. In the case of support for more redistribution the effects suggest that for those least expecting the initial shock, support grows a little with each passing year, but the variance of the estimates grows faster and the effects are never significant. After the first year, effects on belief in luck no longer relate to initially perceived risk, and remain insignificant. The bottom line is that unemployment shocks have a significant one-period effect on unemployment benefit preferences and never have a significant effect on beliefs.

[10] Full sample versions of Figures 8 and 9 in this section are in Appendix A6, Figures A3 and A4.

[11] We acknowledge that these magnitudes do not differ significantly from each other.

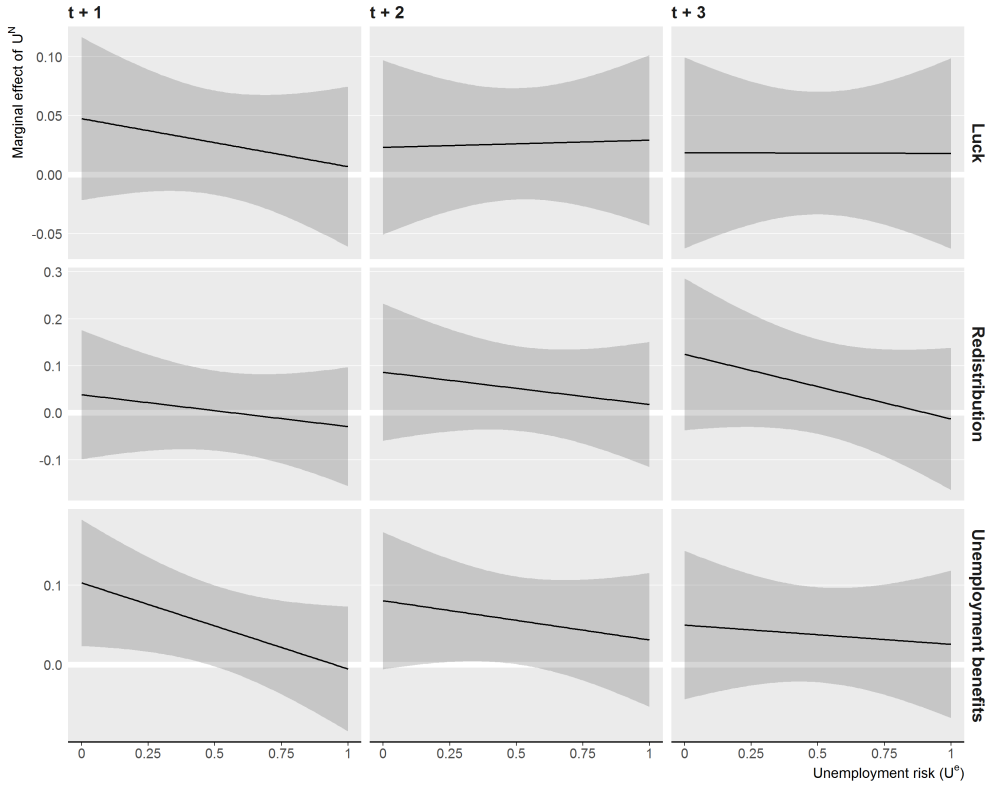

Figure 8: Persistence of unemployment shocks on preferences and beliefs

Income shocks

Figure 9 charts the impact of income shocks for each of the six models in Table 8, for beliefs as well as the two preference variables, and (similar to the previous section), for effects in each case in $t + 1$, $t + 2$, and $t + 3$. This analysis of income shocks shows that the estimated divergence between the cases of belief in luck and effort as well (as the magnitude of the effects) damps away in both the case of redistribution and unemployment benefits, though the pattern is less clear in the latter case. The divergence by belief in the case of redistribution preferences largely vanishes by the second period. Perhaps the clearest message is that neither income nor unemployment shocks change beliefs in any way we observe, and the

two belief groups we study do not respond to shocks differently either. We believe this nonresponsiveness to unanticipated negative shocks justifies the use of beliefs as conditioning responses of redistribution preferences to shocks.

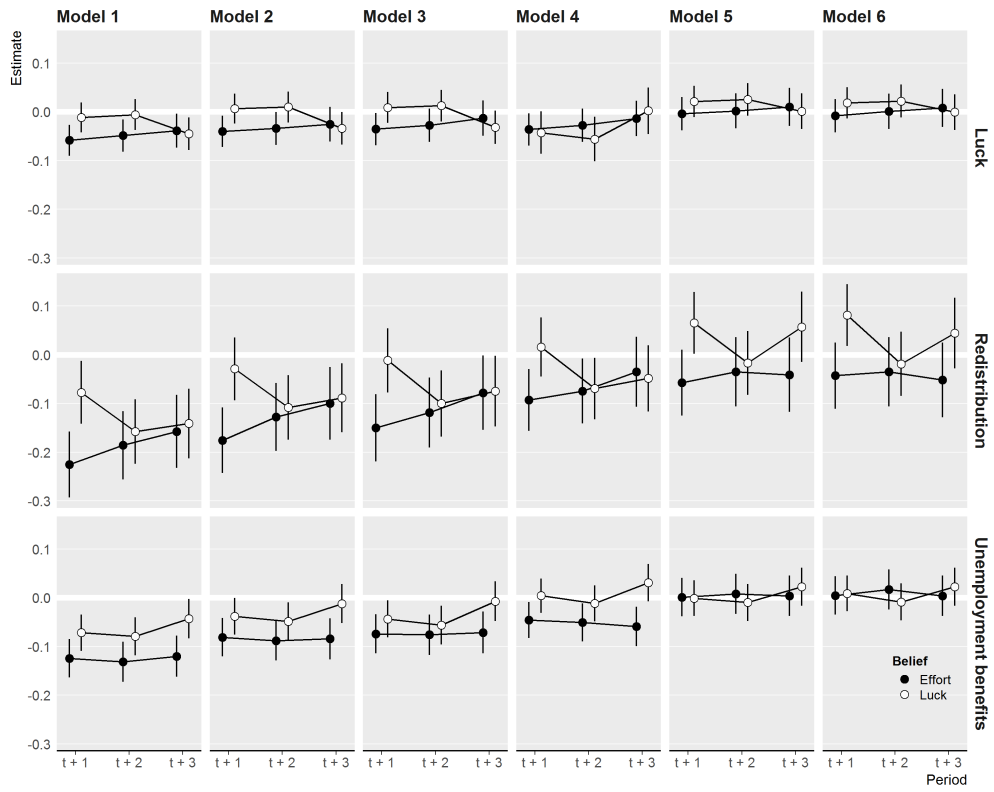

Figure 9: Persistence of income shocks on preferences and beliefs

Conclusion

In the present paper, we study the dynamics of political preferences in the short run. By linking expectations and actual experiences at the individual level, we demonstrate the importance of distinguishing between the impact of anticipated and unanticipated economic events on preferences. We focus on two central individual economic outcomes of key importance for welfare and, for this reason,

frequently studied in political economy: employment status and income. First and foremost, we find that unanticipated shocks to employment and income affect political preferences for social insurance and redistribution, while anticipated shocks and changes do not. This provides a political economy perspective on the workhorse model of economics, the life-cycle/permanent income model, in which all available information, including expectations about future outcomes, affects current preferences over consumption and savings. In that model, changes in consumption plans are observed only when new information arrives. In our case, expectations likewise affect current preferences over redistribution, and only new information, in the form of unexpected realizations of employment and income, appear to affect preferences. A natural next step is for future research to consider how unanticipated and anticipated wealth shocks, a frequently studied topic in the aftermath of the financial crisis (e.g., Mian, Rao, and Sufi 2013), as well as employment and income shocks, affect political preferences.

We show that the distinction between anticipated and unanticipated shocks carry over from political preferences to voting intentions: in particular, unanticipated unemployment induces economic voting, i.e. voting against the incumbent of any political stripe, while anticipated unemployment does not move votes. This raises the issue of how voters perceive and trust new information about job creation efforts and national unemployment (Alt, Marshall, and Lassen 2016) and translates this into expectations at the individual level (Alt et al. 2017) and, at the same time, reinforces the idea that the benefits of incumbents' political optimism, which may be capitalized into current preferences, should be seen against the costs of not delivering and, through unanticipated shocks, disappointing voters.

Our results also add to the accumulating literature on how either partisan political or deeper ideological cleavages like beliefs about fairness or desert condition responses of preferences to economic conditions. Nevertheless, we also argue that constructing measures of shocks from subjective expectations and combining these with administrative data ex post makes it possible to estimate causal effects of unanticipated shocks. In our case, the threat to our argument comes from

reverse causality due to motivated reasoning, which could occur if voters feel relatively more optimistic or pessimistic depending on the partisan identity of the office holder. While that could generate patterns of shocks consistent with what we observe, we detect no evidence of such partisan expectation formation: the unanticipated shocks we observe are uncorrelated with political and ideological differences. However, this need not hold across all political settings (Gerber and Huber 2009) and needs to be carefully considered on a case by case basis.

Our analysis, furthermore, emphasizes that events (or the availability of "new" information, not previously anticipated) about voters' personal economic situation need not affect political preferences at all – if they were expected. That has implications for studies of how retrospectively observed changes in prices, wages, assets and employment prospects affect political preferences and choice. As an example, actual job loss is sometimes the result of a long process. As we show, it can have an effect on incumbent support and, potentially, on beliefs about the role of luck vs. effort in producing economic outcomes when it happens, but from the point of view of an individual, when does it actually "happen"? Perhaps, when it becomes a fact. But that comes after when it becomes "certain", which comes after when it becomes "likely", which comes after when it becomes "possible", and so on. Changes in preferences, even withdrawal of incumbent support, could come at any time and from multiple sources.[12] To avoid misspecification when estimating the effect of experiencing unemployment, like any (partly) foreseeable shock including income changes, transitions from employment to unemployment or the other way around, and wealth shocks such as foreclosure or house price bubbles bursting, one needs to elicit expectations of such outcomes jointly with political preferences, and to study the outcomes in a dynamic setting.

Finally, our results raise an issue for empirical research on "preferences for redistribution". That literature (not pointing a finger at anyone in particular) has treated the general redistribution question (whether government should reduce

[12] Alt et al. (2017) shows that unemployment concerns traveling through weak-link networks can alter unemployment expectations and political preferences.

inequality) and the question of whether unemployment benefits should be increased as more or less parallel indicators of support for redistribution, perhaps because both questions appear from time to time in major data collections like the European Social Survey. Our extensive, detailed data show that these two questions are not simply alternative measures of a general left-right disposition, but actually function differently. Unemployment benefits preferences respond to unemployment shocks while the redistribution question elicits responses to income shocks, and the conditionality of the latter income effects on beliefs, emphasizes how voters differentiate these two questions.

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The Effect of Income and Unemployment Shocks on Political Preferences

[Appendix][*]

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A1. Question wording of survey variables

Redistribution Responses to the question "Some people think that the government should do all it can to raise living standards for poor Danes; they score 1 on the scale. Others think that this is not the responsibility of the government and that individuals should support themselves; they score 5 on the scale. Where would you place yourself?"

Unemployment benefits Responses to the question "Many people have lost their job due to the economic crisis. Do you think the government should do more to support people who become unemployed?"

Success in life Responses to the question "Some people think that success requires hard work; they score 1 on the scale. Others think that it is mostly a question of luck or connections; they score 3 on the scale. Where would you place yourself?"

Minimum (y^e_{min}) and maximum (y^e_{max}) expected income Responses to the following question: "Think about your income for this year. What is the [minimum]/[maximum] income you would realistically expect to earn this year?"

Probability of income between minimum and midpoint (p) Responses to the following question: "What is the probability that your income this year will be less than $(y^e_{min} + y^e_{max})/2$?"

Unemployment prospect (U^e) Responses to the following question: "What is the probability that you will experience unemployment in the coming year? If you answer 0 that means the event will definitely not happen. If you answer 100 that means the event will definitely happen."

A2. Derivation of income expectations

We can think of Y as a distribution that is the result of mixing two conditional uniform distributions index by a random parameter variable θ. The probability that we sample from the "low income" distribution, that is, that income falls between the minimum and the midpoint, is a Bernoulli random variable with parameter p: $\theta \sim \mathcal{B}(p)$. Conversely, the probability that we sample from the "high income" distribution, that is, that income falls between the maximum and the midpoint is $1 - p$.

We can write the two distributions as

$$A : Y \mid \theta = 1 \sim \mathcal{U}(y^e_{\text{min}}, y^e_{\text{mid}})$$
$$B : Y \mid \theta = 0 \sim \mathcal{U}(y^e_{\text{mid}}, y^e_{\text{max}})$$

We note that the conditional mean of the "high income" scenario is just the low income scenario shifted to the right with a factor $w = \mu_B - \mu_A$.

We thus have the following distributional quantities of Y:

$$E[Y \mid \theta = 1] = \mu_A \qquad E[Y \mid \theta = 0] = \mu_A + w$$

Note that the conditional variances for the two scenarios are the same:

$$V[Y \mid \theta] = V[Y \mid \theta = 1] = V[Y \mid \theta = 0] = \sigma^2 \tag{1}$$

The unconditional expected value of Y is the weighted average of the conditional means:

$$\begin{aligned} E[Y] &= p \times E[Y \mid \theta = 1] + (1 - p) \times E[Y \mid \theta = 0] \\ &= p \times \mu_A + (1 - p) \times \mu_B \\ &= \mu_A + (1 - p) \times w \end{aligned} \tag{2}$$

The unconditional variance of a random variable Y which is indexed by another random variable θ, can be decomposed as the sum of two components:

$$\begin{aligned} V[Y] &= E[Y^2] - E[Y]^2 \\ &= E[E[Y^2 \mid \theta]] - E[E[Y \mid \theta]]^2 \\ &= EV[Y \mid \theta] + E[Y \mid \theta]^2 - E[E[Y \mid \theta]]^2 \\ &= E[V[Y \mid \theta]] + E[E[Y \mid \theta]^2] - E[E[Y \mid \theta]]^2 \\ &= E[V[Y \mid \theta]] + V[E[Y \mid \theta]] \end{aligned} \tag{3}$$

The first component, given in Equation 1, is the expected value of the conditional variances and the second component is the variance of the conditional means.

The second component can be calculated as

$$\begin{aligned} V[E[Y \mid \theta]] &= p \times E[Y \mid \theta = 1]^2 + (1-p) \times E[Y \mid \theta = 0]^2 \\ &\quad - (p \times E[Y \mid \theta = 1] + (1-p) \times E[Y \mid \theta = 0])^2 \\ &= p \times \mu_A^2 + (1-p) \times [\mu_A + w]^2 \\ &\quad - (p \times \mu_A + (1-p) \times \mu_B)^2 \\ &= p(1-p)w^2 \end{aligned} \tag{4}$$

The unconditional variance of Y can therefore be written as

$$V[Y] = \sigma^2 + p(1-p)w^2 \tag{5}$$

A3. Unemployment expectations by insurance status

Figure A1 shows the distribution of unemployment expectations among insured (panel 1) and uninsured (panel 2) individuals.

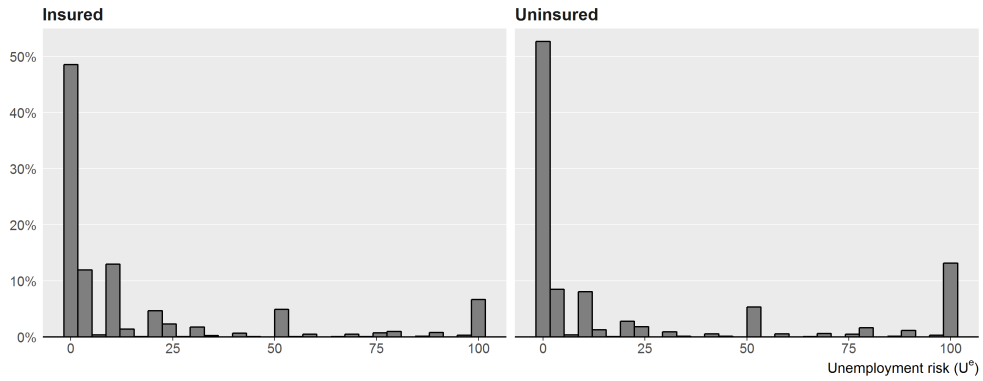

Figure A1: Unemployment expectations by insurance status

A4. Relationship between income and unemployment shocks

In the following section we investigate the extent to which unemployment shocks are also income shocks. We do this by first classifying individuals according to whether they experienced positive or negative changes in unemployment, or no change at all. Afterwards, we divide these groups based on whether they expected to experience unemployment with positive probability at the beginning of the year, or whether they expected with certainty to be fully employed. We then show the distribution of normalised income shocks within each of these six sub groups. Results are presented in Figure A2, with the dotted red lines displaying the mean within each subgroup. The figure shows, as expected, that income shocks for individuals who experienced no change in actual unemployment are closely centered around zero. Among the other groups, income shocks are still

centered around zero, but with substantially higher variance. We thus see that for some individuals, unemployment shocks are at the same time income shocks, whereas for others the story is more complicated and unemployment shocks do not mechanically lead to income shocks, perhaps because these individuals have unemployment insurance.

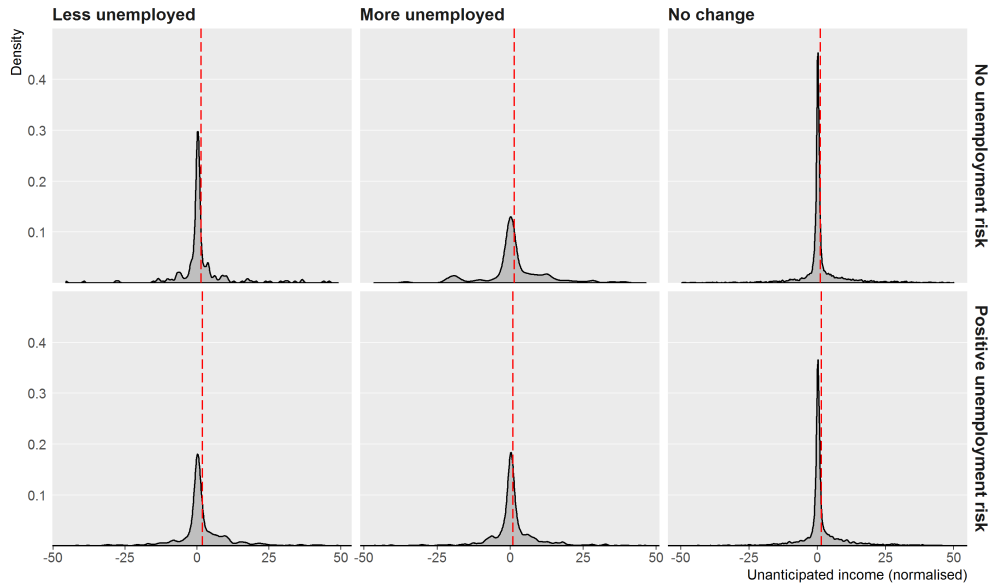

Figure A2: Relationship between income and unemployment shocks

A5. Robustness of income shock results to choice of threshold

The main paper defined an income shock as unanticipated income that exceeds the standard deviation of the expected income distribution. This corresponds corresponds to unanticipated income that falls outside the individual's 70 percent confidence interval. In this section, we investigate the robustness of our main results to the choice threshold. We do so by defining an income shock as unantici-

pated income that exceeds the individual's 90 percent confidence interval. That is, we now define an income shock, S, as

$$S = \begin{array}{ll} S^N & \text{if } \theta < -1.65 \times \sigma^Y, \\ S^P & \text{if } \theta > 1.65 \times \sigma^Y. \end{array} \tag{6}$$

We then redo the main regression (Table 9) in the main paper. Results are presented in Table A1. The point estimates presented in the table are slightly larger, but generally very close to those presented in the main paper, and there is thus little evidence that the the results presented in the main text are sensitive to the choice of threshold.

Table A1: Income shocks based on 90% threshold

	A: Redistribution					
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.231***	−0.181***	−0.161***	−0.117***	−0.081*	−0.073*
	(0.038)	(0.038)	(0.039)	(0.036)	(0.042)	(0.042)
S^P	0.030	0.043	0.048	0.033	0.062*	0.048
	(0.032)	(0.031)	(0.032)	(0.030)	(0.034)	(0.034)
Luck [B]	0.333***	0.298***	0.290***	0.175***	−0.024	−0.021
	(0.022)	(0.022)	(0.023)	(0.021)	(0.026)	(0.026)
Expected income change	−0.000	−0.000	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
S^N × Luck [B]	0.152***	0.149***	0.131**	0.114**	0.104*	0.115**
	(0.052)	(0.052)	(0.053)	(0.049)	(0.056)	(0.056)
S^P × Luck [B]	0.004	0.001	−0.001	−0.003	−0.019	−0.011
	(0.042)	(0.041)	(0.042)	(0.039)	(0.044)	(0.043)
Observations	13,690	13,640	13,262	13,262	13,690	13,690
	B: Unemployment benefits					
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.151***	−0.111***	−0.105***	−0.069***	−0.007	−0.007
	(0.022)	(0.022)	(0.022)	(0.021)	(0.024)	(0.024)
S^P	−0.001	0.012	0.021	0.011	0.008	0.002
	(0.018)	(0.018)	(0.018)	(0.017)	(0.019)	(0.019)
Luck [B]	0.169***	0.150***	0.140***	0.077***	−0.002	−0.006
	(0.013)	(0.013)	(0.013)	(0.012)	(0.015)	(0.015)
Expected income change	−0.000	−0.000	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
S^N × Luck [B]	0.082***	0.073**	0.059*	0.065**	−0.005	0.007
	(0.030)	(0.030)	(0.030)	(0.028)	(0.032)	(0.032)
S^P × Luck [B]	0.002	−0.002	−0.007	−0.004	−0.033	−0.026
	(0.024)	(0.024)	(0.024)	(0.022)	(0.025)	(0.025)
Observations	13,495	13,445	13,071	12,853	13,495	13,495
Demographic Controls	No	Yes	Yes	Yes	No	No
Economic Controls	No	No	Yes	Yes	No	Yes
Year FE	No	No	No	Yes	No	Yes
Lagged Dep. Variable	No	No	No	Yes	No	No
Model	OLS	OLS	OLS	OLS	FE	FE

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). The controls in the fixed effects regressions are gross income and time in labor force spent unemployed, as well as time and individual fixed effects. Robust standard errors clustered at the individual level are provided in parenthesis. *p < 0.1; **p < 0.05; ***p < 0.01.

A6. Persistence of shock effects: Estimates from unbalanced sample

Tables 8 and 9 in the main paper investigated whether income or unemployment shocks had persistent effects on political beliefs and preferences. To avoid compositional effects in which the effective sample changed from year to year due to attrition, we estimated the effect of an unemployment (Table 8) or income shock (Table 9) on the subset of individuals who remained in the survey for three consecutive years. For completeness, we present equivalent results for the unbalanced sample below. Figure A3 shows results for unemployment shocks and Figure A4 shows results for income shocks. As can be seen, the results are very similar to those reported on the restricted sample in the main paper.

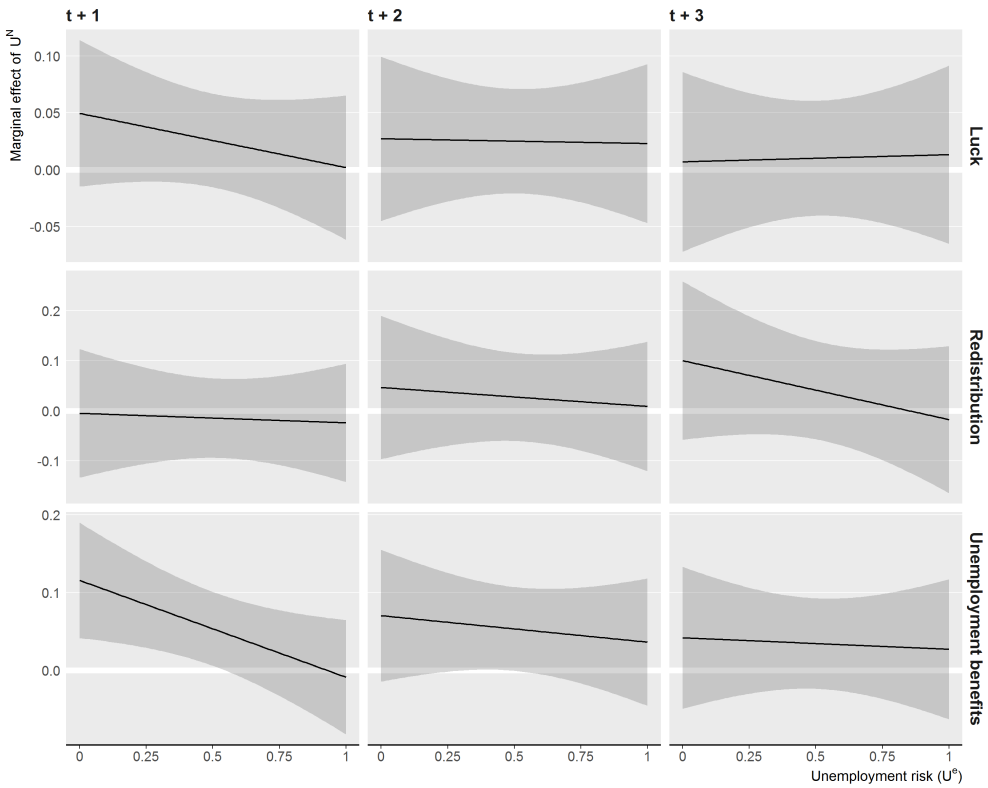

Figure A3: Persistence of unemployment shocks: Estimates from unbalanced sample

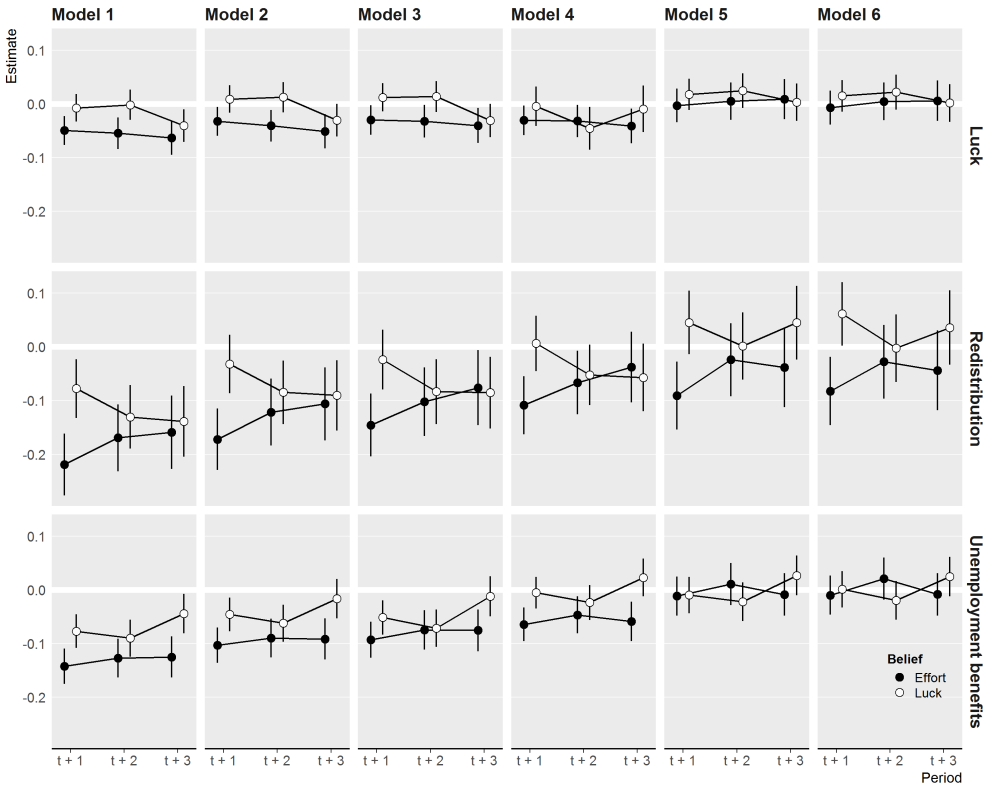

Figure A4: Persistence of income shocks: Estimates from unbalanced sample

A7. Selective nonparticipation

This section examines potential issues with selective nonparticipation among survey participants. The concern is that respondents drop out of the survey based on particular traits which creates selection bias in our estimates. As mentioned in the paper, average attrition from one survey to the next was approximately 30 percent.

One strenght of our combined survey and administrative data is that we have access to administrative data on the characteristics of individuals who drop out. To assess whether our results are driven by selective nonparticipation, we implement a correction based on inverse probability weighting. We estimate a logit model for attrition across all survey participants. We use information about mean income and unemployment from 1998-2008 as well as all available demographic variables as explanatory variables in the logit model. This generates, for each survey participant, a predicted probability of continuing in the survey. We then weight each observation with the inverse of this probability in our main regressions.

Tables A2 through A3 show the results. Throughout, the point estimates are close to those of the unweighted regressions presented in Tables 6 and 9 in the main text, and there is thus little evidence that the results presented in the main text are affected by selective dropout.

Table A2: Unemployment shocks and political attitudes with reweighting to correct for attrition

	A: Redistribution					
	(1)	(2)	(3)	(4)	(5)	(6)
U^P	−0.018	0.006	0.009	0.040	0.106*	0.103*
	(0.052)	(0.059)	(0.059)	(0.054)	(0.061)	(0.061)
U^N	0.030	0.019	0.015	−0.001	0.053	0.036
	(0.067)	(0.066)	(0.066)	(0.061)	(0.076)	(0.078)
U^e	0.104**	0.074	0.086*	0.068	0.083	0.093*
	(0.046)	(0.046)	(0.046)	(0.042)	(0.054)	(0.054)
$U^P \times U^e$	0.137	0.120	0.124	0.070	0.006	0.027
	(0.099)	(0.103)	(0.103)	(0.095)	(0.112)	(0.112)
$U^N \times U^e$	−0.029	−0.030	−0.032	−0.006	−0.003	0.020
	(0.103)	(0.102)	(0.101)	(0.093)	(0.112)	(0.112)
Observations	12,898	12,898	12,898	12,898	12,898	12,898
	B: Unemployment benefits					
	(1)	(2)	(3)	(4)	(5)	(6)
U^P	0.052*	0.019	0.018	0.018	0.057	0.059*
	(0.029)	(0.033)	(0.033)	(0.030)	(0.035)	(0.035)
U^N	0.179***	0.141***	0.140***	0.121***	0.109**	0.100**
	(0.038)	(0.038)	(0.037)	(0.035)	(0.044)	(0.045)
U^e	0.110***	0.084***	0.090***	0.052**	0.034	0.039
	(0.027)	(0.026)	(0.026)	(0.025)	(0.032)	(0.031)
$U^P \times U^e$	0.088	0.002	0.003	−0.027	−0.006	−0.002
	(0.056)	(0.058)	(0.058)	(0.054)	(0.064)	(0.064)
$U^N \times U^e$	−0.121**	−0.124**	−0.127**	−0.111**	−0.127*	−0.123*
	(0.059)	(0.058)	(0.058)	(0.054)	(0.065)	(0.065)
Observations	12,711	12,711	12,711	12,508	12,711	12,711
Demographic Controls	No	Yes	Yes	Yes	No	No
Economic Controls	No	Yes	Yes	Yes	No	Yes
Year FE	No	No	Yes	Yes	No	Yes
Lagged Dep. Variable	No	No	No	Yes	No	No
Model	OLS	OLS	OLS	OLS	FE	FE

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). The controls in the fixed effects regressions are gross income as well as time and individual fixed effects. All models include a constant term (not reported). *p < 0.1; **p < 0.05; ***p < 0.01.

Table A3: Income shocks, beliefs and divergence with reweighting to correct for attrition

	A: Redistribution					
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.192***	−0.146***	−0.141***	−0.106***	−0.080*	−0.072*
	(0.038)	(0.038)	(0.038)	(0.035)	(0.042)	(0.042)
S^P	0.039	0.043	0.051	0.034	0.048	0.036
	(0.032)	(0.032)	(0.032)	(0.029)	(0.034)	(0.034)
Luck [B]	0.333***	0.300***	0.293***	0.175***	−0.033	−0.029
	(0.023)	(0.023)	(0.023)	(0.022)	(0.027)	(0.027)
Expected income change	−0.000	−0.000	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$S^N \times$ Luck [B]	0.126**	0.123**	0.116**	0.110**	0.131**	0.137**
	(0.052)	(0.052)	(0.052)	(0.048)	(0.055)	(0.055)
$S^P \times$ Luck [B]	−0.011	−0.009	−0.013	−0.002	−0.002	0.004
	(0.042)	(0.042)	(0.042)	(0.038)	(0.044)	(0.044)
Observations	12,458	12,458	12,458	12,458	12,458	12,458
	B: Unemployment benefits					
	(1)	(2)	(3)	(4)	(5)	(6)
S^N	−0.133***	−0.095***	−0.089***	−0.061***	−0.012	−0.011
	(0.022)	(0.022)	(0.022)	(0.020)	(0.024)	(0.024)
S^P	0.012	0.023	0.031*	0.021	0.011	0.007
	(0.018)	(0.018)	(0.018)	(0.017)	(0.020)	(0.020)
Luck [B]	0.169***	0.153***	0.143***	0.078***	−0.002	−0.006
	(0.014)	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)
Expected income change	−0.000	−0.000	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$S^N \times$ Luck [B]	0.059**	0.049*	0.033	0.054**	0.007	0.014
	(0.030)	(0.030)	(0.029)	(0.027)	(0.032)	(0.032)
$S^P \times$ Luck [B]	−0.016	−0.018	−0.025	−0.017	−0.035	−0.029
	(0.024)	(0.024)	(0.024)	(0.022)	(0.025)	(0.025)
Observations	12,274	12,274	12,274	12,074	12,274	12,274
Demographic Controls	No	Yes	Yes	Yes	No	No
Economic Controls	No	No	Yes	Yes	No	Yes
Year FE	No	No	No	Yes	No	Yes
Lagged Dep. Variable	No	No	No	Yes	No	No
Model	OLS	OLS	OLS	OLS	FE	FE

Demographic controls are constructed from administrative data and include age, age^2, female (dummy), foreign background (dummy), homeowner (dummy), children (dummy), single (dummy), outside the labor force (dummy), education (vocational), education (bachelor's degree), education (master's degree or PhD). Baseline is high school education or less. Economic controls, constructed from administrative data, include time in labor force spent unemployed, gross income, average time in labor force spent unemployed (1998-2008) and mean gross income (1998-2008). All models include a constant term (not reported). The controls in the fixed effects regressions are gross income and time in labor force spent unemployed, as well as time and individual fixed effects. *p < 0.1; **p < 0.05; ***p < 0.01.