
Article

Does high unemployment mobilize the unemployed? Evidence using Swedish register data

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Abstract

This article contributes to recent scholarship on the effects of economic hardship on political participation, and particularly on the issue of how individual and contextual effects interact. More specifically, we study whether the effect of individual unemployment on turnout depends on the level of aggregate unemployment. In contrast to most existing researchers on this topic, we argue that contextual unemployment may reinforce the negative effects of individual unemployment. We also contend that previous studies have not adequately considered how the composition of the unemployed population differs across times of high and low unemployment. Our empirical approach uses state-of-the-art Swedish register panel data, which gives us good opportunities to control for the selection of the unemployed population over the business cycle. We find that once this problem of dynamic selection is taken into account, the negative effect of individual unemployment on electoral turnout is actually stronger in high unemployment contexts.

1. Introduction

The repercussions of the Great Recession have led to a renewed academic interest in the political consequences of economic hardship. Scholars have studied how unemployment and financial distress relate to political ideology (Wiertz and Rodon, 2021), partisan choice (Lindvall, 2014; Lindgren and Vernby, 2016), left- and right-wing populism (Funke et al., 2016; Dehdari, 2021; Gidron and Mijs, 2019) and overall political activity (Kern et al., 2015).

One intriguing finding from this line of research is that the typical negative relationship between individual unemployment and political participation is weaker in contexts of high unemployment. Various theoretical explanations have been advanced to account for this

empirical regularity (Lim and Sander, 2013; Grasso and Giugni, 2016; Aytaç et al., 2018; Carreras and Castañeda-Angarita, 2019; Incantalupo, 2015), but they all build on the mobilization effect discussed by Rosenstone (1982) in his seminal study on the topic. According to the mobilization hypothesis, the experience of unemployment evokes feelings of grievance and anger, which can spur political participation. Rosenstone (1982) contrasted this effect with the withdrawal hypothesis, which holds that economic adversity makes people less politically active because it forces them to focus on their immediate material survival rather than on politics. The theoretical arguments referred to above provide a twist to Rosenstone's argument by maintaining that the relative strength of the mobilization and withdrawal effects on the individual level depends on how widespread unemployment is at the contextual level. More precisely, the idea is that the withdrawal effect should become less salient, and the mobilization effect more important, as overall unemployment increases.

This argument is, however, at odds with the findings of the classical Marienthal study by Jahoda et al. (1971). This pioneering study provided a detailed account of how the unemployed withdrew from both political and social life as mass unemployment hit the small Austrian community of Marienthal in the late 1920s. There were no signs then that a high overall unemployment rate served to offset the negative effect of individual unemployment on political activity, in the way that more recent authors have argued. On the contrary, Jahoda et al. reasoned, it was the very fact that the whole community was struck by unemployment that created a culture of resignation and political withdrawal among the unemployed.

There are, of course, many reasons why the relationship between unemployment and political activity may look different today than it did a century ago. Nevertheless, we believe, the argument that high contextual unemployment serves to mobilize the unemployed needs further scrutiny. First, there are theoretical reasons to expect high unemployment instead to *reinforce* the negative effects of individual unemployment. Widespread unemployment may foster a sense of hopelessness, as well as making it harder to find a new job and producing more severe economic distress among the unemployed—factors which, taken together, may impel such persons to focus on making ends meet and to refrain from participating in politics. Second, we would argue, the findings of previous studies may result from an empirical fallacy, in the form of dynamic selection. That is, if the composition of the unemployed population varies over the business cycle, this may account for why unemployment seems to have larger negative effects on political participation in contexts of low unemployment.

Thus, the aim of this study is to examine how the effect of individual unemployment on political participation varies with aggregate unemployment. Is there an interaction effect between individual and contextual unemployment, and if so, does high overall unemployment mitigate or reinforce the detrimental effects of individual unemployment? In this study, we leverage high-quality Swedish register data in an effort to ascertain how individual unemployment relates to voter turnout in contexts of high and low unemployment. Using validated turnout data from seven consecutive Swedish elections, we can replicate the basic relationship reported in previous research: the unemployed are more likely to vote when overall unemployment increases. Our subsequent analysis, however, shows that this relationship is largely attributable to differences in the composition of the unemployed population across contexts of high and low unemployment. When studying how contextual unemployment affects turnout among the unemployed, we find—if we control adequately for individual characteristics—that the effect of contextual unemployment instead becomes

negative: i.e. high overall unemployment reinforces rather than mitigates the negative effect of individual unemployment.

The rest of this article proceeds as follows. In the next section, we discuss previous research and theory. The third section explains the Swedish institutional context, as well as our methods and data. The empirical results follow there upon and we conclude by discussing policy implications and avenues for further research.

2. Theory and previous research

The literature on the political consequences of economic hardship has a long history, stretching back at least to the seminal work of [Jahoda et al. \(1971\)](#). The topic has, however, received increased attention after the advent of the Great Recession a decade ago. One important question that has been studied is how the economic downturn affected various types of political participation ([Kern et al., 2015](#); [Carreras and Castañeda-Angarita, 2019](#)).

Following the influential study of [Rosenstone \(1982\)](#), scholars writing on the relationship between economic adversity and political participation typically acknowledge that experiences of unemployment and economic hardship can either discourage political participation through a *withdrawal* effect, or encourage participation through a *mobilization* effect. The withdrawal argument predicts that the unemployed will tend to withdraw from politics to attend to more pressing matters, such as looking for a job and making ends meet. According to the mobilization argument, on the other hand, being out of work will evoke feelings of grievance and anger among the unemployed, making them more likely to participate politically.

The empirical support for the two hypotheses, however, is mixed. Studies that focus on the effects of individual unemployment typically point in the direction of the withdrawal hypothesis ([Gallego, 2007](#); [Emmenegger et al., 2015](#); [Kern et al., 2015](#)); however, those which study the relationship between overall unemployment and aggregate participation rates more often find support for the mobilization hypothesis ([Lim and Sander, 2013](#); [Burden and Wichowsky, 2014](#); [Cebula, 2017](#)).

To complicate things further, there is also evidence that the relationship between unemployment and political participation can vary considerably across different elections ([Southwell, 1996](#); [Lim and Sander, 2013](#)). This has led scholars to start examining whether, and if so how, the effect of unemployment depends on various contextual factors. One possibility that has been raised in several studies is that individual economic hardship has different effects on political participation depending on the overall state of the economy. In particular, a number of scholars have suggested, we should expect the unemployed to be politically more active, or at least less inactive, when overall unemployment is high. Or, put differently, high unemployment should serve to mobilize the unemployed politically.

Two different explanations have been proposed for why this might be the case. The first, psychological in nature, focuses on how the unemployed perceive their situation ([Lim and Sander, 2013](#); [Kern et al., 2015](#); [Grasso and Giugni, 2016](#); [Incantalupo, 2015](#)). When the overall unemployment rate is low, the argument goes, those out of work tend to blame themselves for not having a job, which makes the experience of unemployment more stigmatizing. When unemployment is higher, on the other hand, those out of work are more likely to view themselves as unlucky victims of external circumstances beyond their control. Therefore, as [Kurer et al. \(2019, p. 8\)](#), explain:

[T]he effect of individual unemployment on participation in elections crucially depends on the current unemployment rate: in a context of rising unemployment rates, joblessness is increasingly conceived as a societal problem for which the government should provide a remedy and consequently rather mobilises than de-mobilises unemployed voters.

Kern et al. (2015, p. 469) reason in a similar manner, but they relate the argument to the theory of *double relative deprivation*:

It has been argued that the individuals who are most likely to become engaged in collective action are those who perceive a combination of individual and collective deprivation . . . Following this logic we expect that in counties that have suffered strongly as a result of the economic crisis, especially those who have personally experienced deprivation will be most likely to participate.

The hypothesis that high aggregate unemployment will impel the unemployed to become more active politically—because it makes unemployment a social rather than a personal problem—has been put forward in a number of other studies as well (Lim and Sander, 2013; Grasso and Giugni, 2016; Incantalupo, 2015). Moreover, the same type of psychological mechanism has also been invoked to explain why the health and happiness levels of the unemployed seem to improve in recessions (Heggebo and Elstad, 2018).

A couple of recent studies, however, have elaborated alternative explanations—more political ones—for the mobilizing effect of aggregate unemployment. Carreras and Castañeda-Angarita (2019, p. 110), for example, advance the argument that individuals in vulnerable economic situations are the ones most likely to become mobilized during bad economic conditions because they are ‘more likely to suffer the negative consequences of economic downturns’.

By contrast, Aytaç et al. (2018) argue that the reason why the unemployed can be expected to be politically more active during bad economic times is that the economy receives more attention when it is doing poorly. This is because political challengers have an incentive to try to blame incumbents for a bad economic performance. This *blame discourse*, according to the authors, will in turn anger the unemployed and strengthen their intention to vote (Aytaç et al., 2018, p. 12)

Most of these studies have also found empirical support for the positive interaction between individual and aggregate unemployment (but Kern et al., 2015 are an exception, as are Lim and Sander, 2013). The studies by Incantalupo (2015) and Aytaç et al. (2018), however, are of particular interest for this study, as they share our empirical focus on electoral participation. Both of these studies use repeated cross-sectional data from the large US-based Current Population Survey (CPS). While they use somewhat different estimation strategies, both Incantalupo and Aytaç et al. find the unemployed to be relatively more likely to vote when the aggregate level of unemployment is high—which the authors interpret as support for their respective theories.

These results notwithstanding, we draw insights from the literature on how economic hardship affects voting for radical parties, and believe there are reasons for why unfavourable macroeconomic conditions very well may ‘deepen’ and not only ‘dampen’ the effects of individual hardship (Rooduijn and Burgoon, 2018). In our view, an omission in the literature discussed above is that it has not sufficiently considered the possibility of such a deepening negative effect of aggregate unemployment on the political participation of the unemployed. Jahoda et al. (1971, p. 36) claimed that the distinguishing character of

Marienthal, hit as it was by mass unemployment, was that of ‘a weary community’, a community that on the *collective level* was afflicted by monotony and resignation. Most unemployed people in the community had become ‘resigned’ and embraced ‘an attitude of drifting along, indifferently and without expectations, accepting a situation that cannot be changed’ (p. 52). The case of Marienthal was certainly in many ways extreme, but we believe there are reasons why a high level of unemployment may have detrimental effects also today on the political engagement of the unemployed. Although the magnitude of the effects is likely to stand in relation to the level of unemployment, and thus will be considerably less drastic today than in Marienthal, we reason that the mechanisms as such to be of a more general character. These mechanisms are interconnected, but we may broadly categorize them as economic concerns, issues of political efficacy, psychological effects and matters related to technological change.

First, a high rate of unemployment means it will be difficult for unemployed persons to find a new job, as competition is likely to be fierce for the few available job offers. Moreover, high levels of unemployment typically go hand-in-hand with a general economic downturn that puts stress on government finances, which in turn may cause worries to the unemployed about cuts in unemployment benefits and other parts of the welfare state that are crucial for their livelihood. These factors imply that the level of economic distress among the unemployed is likely to increase with the aggregate unemployment rate. And as [Rosenstone \(1982\)](#) argues, when economic conditions become severe, the unemployed tend to focus on eking out a living rather than on engaging in politics.

Second, when unemployment is widespread, and especially if it is persistent over time, the unemployed may lose trust in the capacity of the government to create jobs and in other ways to improve the situation of the unemployed. In addition, when unemployment is high and it is hard to find a new job, numerous failed attempts to get a job may impair an individual’s self-efficacy, which can also weaken his or her political efficacy ([Marx and Nguyen, 2016](#)). These factors are likely further to depress the motivation of unemployed persons to participate politically.

Third, it is well-documented that unemployment has adverse psychological consequences in terms of depression and loss of self-esteem ([Paul and Moser, 2009](#)), which in turn also affects political participation negatively ([Ojeda, 2015](#)). In contexts where unemployment is high, there is a considerable risk that such effects will multiply as depression can be ‘contagious’ ([Joiner and Katz, 1999](#)). Furthermore, the knowledge that unemployment is widely shared may discourage any hope among the unemployed in that change is possible, making political engagement seem futile.

Fourth, economic downturns tend to act as a catalyst for technological change and economic restructuring. One typical example is the decline of manufacturing jobs, a trend that has been pronounced not the least in Sweden in connection to recent economic crises ([Thelen, 2019](#)). This implies that in an economic downturn, a disproportional share of those who lose their jobs come from occupations and economic sectors that are losing ground due to such structural trends. A typical recent example is the decline in routine work and increase in automation, which has been put in relation to support for the radical right ([Im et al., 2019](#)). To lose your job due to such structural changes could accentuate subjective social status decline, reinforce negative psychological effects related to unemployment and also decrease chances of finding a new job ([Kurer, 2020](#)). Effects that in turn are likely to further discourage political participation among the unemployed.

In relation to these mechanisms, an important question is at what geographical level the contextual effect of unemployment comes into play. Previous contributions have often focused on a relatively high aggregate level, such as US states (Aytaç et al., 2018) or countries (Grasso and Giugni, 2016). While the relevant level to some extent differs depending on the mechanism, we reason that the pivotal factor is the local labour market. That is, the relevant geographical level is the area within which it is possible to commute to work—as several of the mechanisms relate to how contextual unemployment determines the chances of getting a new job. Naturally, the unemployed may also move to where there is work. However, many are probably unwilling or unable to do so. To the extent that some actually do move to get a job, this is likely to be more resourceful individuals who are competitive on the labour market and who also are more likely to be politically active. This relationship actually points to an additional channel through which contextual unemployment may impair the political participation of the unemployed. Because when the resourceful move from an area characterized by high unemployment, those who are unemployed but unable to move may distance themselves from the political system as such movement patterns may infuse a sense of being ‘left-behind’ (Dijkstra et al., 2020).

In sum, we do not believe that the issue of whether high contextual unemployment mitigates or amplifies the negative effects of individual unemployment can be answered on strictly theoretical grounds, since there are mechanisms operating in opposite directions. The nature of the relationship is therefore ultimately an empirical matter.

However, studying the joint impact of contextual and individual unemployment on political participation is a very challenging task. One particular difficulty, which in our view has received insufficient attention in previous research, is that the composition of the unemployed population is known to vary with the state of the economy (Heggebø and Elstad, 2018). That is, there is dynamic selection into and out of the unemployment pool over the business cycle, in that workers with higher qualifications and better skills are the last to become unemployed during downturns and the first to gain employment during upturns. Workers of this type will therefore make up a larger share of the unemployed when overall unemployment is high. Because these more resourceful workers in general are likely to be more prone to participate politically, such a selection effect can make it appear like high aggregate unemployment makes the unemployed more politically active.

Thus, if we are accurately to capture how the effect of individual unemployment on voter turnout varies with the aggregate rate of unemployment, we need to address the issue of dynamic selection. The preferred strategy in previous studies has been to control explicitly for various individual characteristics, such as age, gender and educational attainment. The main drawback of this approach is that it is usually very difficult to determine whether the featured controls are sufficient to make the group of unemployed persons comparable across the business cycle. There is, in particular, a substantial risk that unobserved differences remain between the unemployed in high and low unemployment contexts.

An alternative approach is possible, however, when we have access to panel data. We can then follow individuals over time to examine whether they behave differently when unemployed in contexts of high and low unemployment. This approach will automatically adjust for all individual characteristics, observable as well as unobservable, that remain stable over time. Although not perfect, this strategy rests on considerably less stringent modelling assumptions than the standard control-based approach. In this study, we use the high-quality data at our disposal to examine whether the findings of previous studies remain

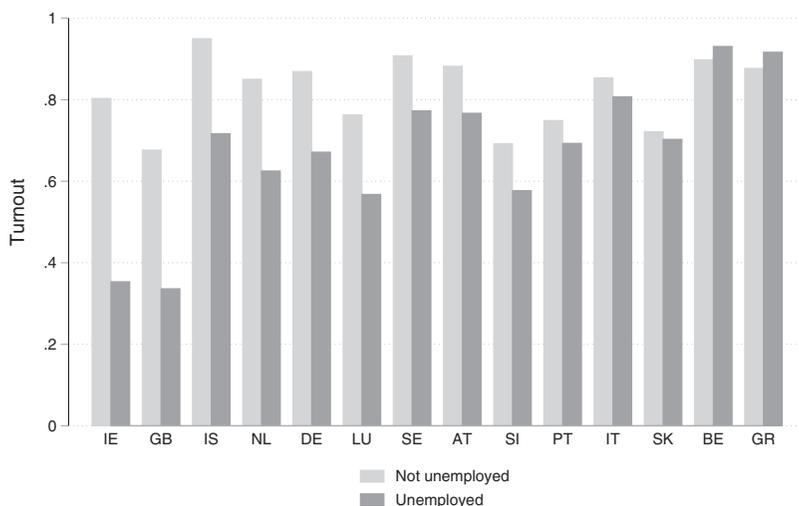


Figure 1 Turnout among the unemployed and the not unemployed in national elections in Sweden and other European countries, 2002–2016; ESS data. Ordered from large to small difference in turnout. *Notes:* Cumulative ESS data 2002–2016 for the EEA (European Economic Area) countries. Self-reported turnout in the last national election among respondents 20–65 years old. ‘Unemployed’ have stated unemployment as their main activity during the 7 days before being surveyed. ‘Not unemployed’ include all others. Sample restricted to respondents who were surveyed at most 6 months after the last national election, which also limits the number of countries (only countries with at least 200 observations are included). Note that Belgium and Luxembourg have compulsory voting.

robust when this particular method for handling dynamic selection is used. We next discuss how we will go about doing so.

3. Institutions, methods and measurements

In this section, we first present the Swedish institutional context, after which we describe our data and our empirical model.

3.1 Institutional context

In an effort to improve on earlier work, we base our empirical approach on state-of-the-art Swedish data from administrative registers. Sweden has a parliamentary system with party-list proportional representation, and general elections to the national parliament are held every fourth year.¹ All Swedish citizens who live or have lived in Sweden and are at least 18 are entitled to vote. Turnout in Swedish elections is high compared with that in most developed democracies, with an average turnout of 83.9% in the seven general elections between 1991 and 2014. Turnout among the unemployed, however, is on average 12 percentage points lower. As [Figure 1](#) shows, the size of this unconditional turnout gap puts Sweden in

1 Before the general election of 1994, elections were held every third year.

the middle when a sample of European countries is ranked in terms of this gap, even though turnout on average tends to be lower in Europe at large.

Unemployment insurance is an important institutional factor, since several of the mechanisms behind the effects of individual and contextual unemployment are related to economic distress. Swedish unemployment insurance is organized according to the Ghent system; thus, the insurance is voluntary and administered by the trade unions (Rothstein, 1990). Benefits are income-related and cover 80% of previous earnings up to an income ceiling. To be eligible for unemployment benefits, a person has to have worked for some time before being unemployed, to be registered at the Public Employment Service and to be looking actively for a job. While Swedish unemployment insurance historically has been quite generous in international comparison, the net replacement rate over the last couple of decades has been close to the average in other Western countries. This is mainly explained by the fact that the income ceiling is lower than most workers' pre-unemployment earnings. During the 2001–2014 period, the net average replacement rate for a Swedish worker earning the average wage was 52.4%, as compared to 52.5% among the EU28 and 49.2% in the USA.²

Not only is Sweden attractive to study due to data availability, there are also factors making Sweden a 'hard case' for a possible discouraging effect of contextual unemployment on political participation by the unemployed. Sweden has a strong tradition of active labour market policies that serve to educate and activate the unemployed, thereby improving their chances of finding a job (although these kinds of policies became less prominent towards the end of the period we are studying, e.g. Bonoli, 2012; Thelen, 2014). Such policies may lessen the possible negative effects of high contextual unemployment in terms of collective depression and apathy among the unemployed. Furthermore, the generosity of Swedish unemployment insurance during this period has been somewhat more generous or, during the later period, on a par with the average in other Western countries. We would thus expect Swedish unemployment insurance to have a similar, or possibly stronger, capacity to offset mechanisms related to economic distress as compared with that in other countries. Moreover, Sweden has a high level of institutional trust, as well as a PR electoral system that can stimulate political efficacy even among more marginalized groups (Karp and Banducci, 2008). These factors may further weaken the potential negative impact of contextual unemployment on the political efficacy among the unemployed.

3.2 Data

Our main data come from Swedish administrative registers, which provide individual-level data on turnout, unemployment and other socio-economic as well as demographic variables. The data cover the seven general elections from 1991 through 2014, and they come from two different sources. For the elections in 1994 and 2010, we have access to high-quality population data on turnout that stem from the scanning and digitizing of publicly available election rolls (Lindgren et al., 2019). We pair these data with data from the electoral participation survey conducted by Statistics Sweden. This survey collects data on turnout through register information for repeated cross-sections of, on average, 50 000 eligible voters per

2 These numbers refer to single persons without children who had previous earnings equal to the average wage and who have been unemployed for 6 months; housing benefits are not included (OECD data). In countries with voluntary unemployment insurance, such as Sweden, it is assumed that the person is part of the voluntary scheme.

election. Similarly, we can construct an unbalanced panel incorporating approximately 2 859 000 individuals for whom we have at least two records on turnout, amounting to about 5 825 000 observations. In these data, there are around 432 000 occurrences of unemployment in connection with an election.

The construction of the dataset implies that it is heavily dominated by observations from the elections in 1994 and 2010—elections which both took place in the aftermath of large economic crises. To ensure that our estimates are not biased by this fact, we weigh the observations for each election in accordance with the total number of eligible voters at the time. For this purpose, we use the weights supplied by the election-participation survey. In essence, this means that we count the sample-based observations many times more, to make the complete dataset representative of the joint population of eligible voters for all seven elections (see [Supplementary material](#) for further details).

The unit of analysis in our data is individuals nested in election years. An individual is defined as unemployed during a year if he or she lacks a workplace in November and has been registered as unemployed at the Public Employment Service during the year. We use November because it is the month when administrative employment data are collected in Sweden; it also has the benefit of being reasonably close in time to the general elections, which are always held in September. We do not have access to individual unemployment spells within years and the fact that unemployment is measured *after* the election may seem problematic. However, Swedish law and collective agreements regulating the termination of employment contracts imply that most individuals who are about to lose their job between September and November would know about this on Election Day. More specifically, Swedish regulations of early notices require employers to inform the Public Employment Service and trade unions at the workplace about layoffs of five employees or more at least 2 months before the execution of the layoffs. Furthermore, the period of notice for terminating a permanent contract for an individual employee, who has been employed for more than 2 years, is at least 2 months.³ Consequently, most individuals who are about to lose their job between Election Day and November will be aware of this already on Election Day; the exception being employees with a relatively short employment period who are laid off together with less than four other employees. That is, most of those who are employed on Election Day but unemployed in November, will already be affected by the economic distress connected to unemployment when they chose to go to the ballot or not in September. We also run alternative models on the timing of unemployment in relation to Election Day in our robustness checks.

We restrict the sample to individuals between 20 and 65 years of age. For contextual unemployment, we rely on the municipalities, of which there are nearly 300 in Sweden.⁴ The average population of a municipality varies between 30 000 and 33 000 during the time period studied, although the population differs considerably between urban and rural areas. We employ the same definition for unemployment on the municipal as on the individual level, aggregated and divided by the total number of employed and unemployed persons within the municipality in question.

3 Note that this is a summary of the institutional background. Further details are given in the [Supplementary material](#).

4 The exact number of municipalities has varied during the period under study. In the analyses, we keep the number fixed at 277.

Our data have several advantages compared with those used in previous literature. First, since they are panel data, we can study how the voting behaviour of the same individual is affected by unemployment in contexts of high and low unemployment. This makes it possible to control for all time-invariant unobserved individual-level variables. Second, most previous research on how contextual unemployment conditions the effect of individual unemployment uses American or European data in which contextual unemployment is measured at the state or country level (e.g. Kern et al., 2015; Cebula, 2017; Aytaç et al., 2018; Kurer et al., 2019; Incantalupo, 2015). However, such aggregate measures are rather crude indicators of the contextual unemployment experienced by voters, since economic conditions typically vary considerably within states and countries with millions of residents. Relying on the Swedish municipal level for contextual unemployment enables us to more adequately examine how local labour markets condition the effect of individual unemployment on turnout. Another argument in favour of using the municipal level is that Swedish municipalities constitute an important political entity with a directly elected municipal assembly. This entails that media reports on the economy and general development in municipalities and that there is a public political discourse on the municipal level. Alternative geographical levels, such as commuting areas, are likely to be less known by voters. Third, relying on administrative data eliminates problems of non-response, over-reporting of turnout and other data-quality issues that may bias estimates. Fourth, the high-quality data furnished by the administrative records, coupled with additional population data on the turnout for elections before the period studied, give us very good opportunities to test the influence of other individual-level variables on turnout among the unemployed, as well as the conditioning effect of contextual unemployment. We have, for example, data on the voting behaviour of the parents of the individuals in our sample.

We complement our Swedish data with data from the European Social Survey (ESS) on EU countries.⁵ We use the cumulative ESS dataset consisting of all available rounds from 2002 to 2016. For unemployment, we rely on a measure of whether the respondent has been unemployed during the last 7 days. We measure contextual-level unemployment annually at the country level using OECD data.

3.3 Estimation

Our general model can be described as follows:

$$V_{iem} = \alpha + \beta_1 D_{iem} + \beta_2 U_{em} + \beta_3 D_{iem} \times U_{em} + \delta_e + X_{iem} + \varepsilon_{iem} \quad (1)$$

where V_{iem} is a dichotomous measure of turnout for individual i in election e , living in municipality m . D_{iem} is a dummy indicator for whether the individual is unemployed (the measure detailed out above). U_{em} represents the unemployment rate in municipality m at election e . δ_e denotes a set of election dummies. X_{iem} is a vector of individual-level control variables. In our preferred specification the vector also includes individual and municipal-fixed effects. Our coefficients of interest are consequently β_1 and β_3 , which together represent how the relationship between individual unemployment and turnout is conditioned by contextual unemployment.

5 We include all EU countries and add the EEA (European Economic Area) countries of Norway and Iceland.

The individual-fixed effects absorb all average differences between individuals, implying that we rely on the within-individual variation in turnout, thereby mitigating the problem that the composition of the unemployed population tends to differ in periods of low and high unemployment. Put differently, we are in this way able to rule out bias connected to dynamic selection of the unemployed as long as this selection is related to time-invariant individual-level factors (observed as well as unobserved). We also add municipal-fixed effects to avoid that cross-sectional differences in unemployment and turnout across municipalities may bias our results, for instance, that there is selection among the unemployed who moves in and out of municipalities with a high level of unemployment.⁶ In our robustness checks, we test models that allow for further flexibility on the municipal level and also control for possible municipal time-variant factors that could potentially bias the estimates.

Finally, despite the fact that our key dependent variable is binary, we rely on a linear probability model to obtain our estimates. There are two main reasons for this. First, the linear probability model greatly facilitates the interpretation of the interactive relationship between contextual and individual unemployment, which becomes much more complex in the non-linear setting (Ai and Norton, 2003). Second, if we were to use logit regression to estimate our models with individual-fixed effects we would, for technical reasons, need to restrict attention to the small subset of individuals who changed their voting behaviour over time, whereas the linear probability model allows for the use of all available data (King, 2001). We reason that this is preferable considering that we are interested in estimating the population-level effect of unemployment on turnout and that we risk selection bias if we instead would focus on the subset who have changed their voting behaviour (Timoneda, 2021, see also Beck, 2020). However, logit models are tested as a robustness check.

Furthermore, we cluster the standard errors at the municipal level in the Swedish register data and at the country level in the ESS data. These standard errors are also robust to the inherent heteroscedasticity of the residuals in the linear probability model.

4. Empirical results

We start by replicating the results from previous studies both in Europe at large and in Sweden using the ESS data. Thereafter, we use our high-quality Swedish data to provide a descriptive overview of the composition of the unemployed in contexts of high and low unemployment, as well as their voting behaviour. We continue by presenting our preferred regression models, which more adequately consider dynamic selection, and by exploring the mechanisms. Finally, we go through our robustness checks.

4.1 Replicating the results from previous research in Sweden and Europe

In Figure 2, we demonstrate how the relationship between individual unemployment and turnout varies with contextual unemployment in the ESS data. The underlying OLS model uses turnout as the dependent variable and it includes an interaction between individual unemployment and contextual unemployment at the country level. We use the country level in our main specification due to data limitations and precision issues related to dividing a small sample on sub-national regions. A relatively comprehensive set of individual controls are

6 Since individuals are nested within municipalities, municipal fixed effects—in addition to individual-fixed effects—only have an impact on individuals who move across municipalities.

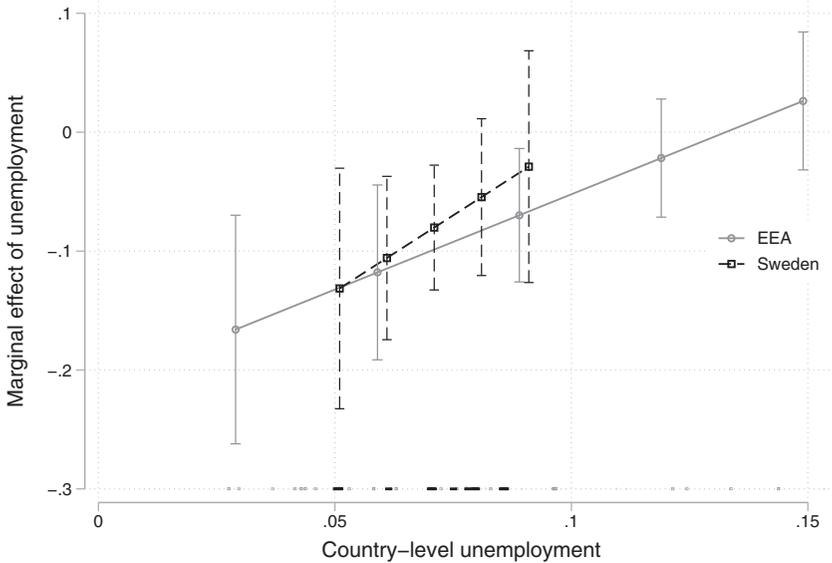


Figure 2 The marginal effect of individual unemployment on turnout at different levels of contextual unemployment in Europe and Sweden. ESS data, 95% confidence intervals.

Notes: The plot is based on a model interacting individual and contextual unemployment using the same ESS sample as in Figure 1, thus limiting the sample to respondents that have been surveyed within 6 months after the last national election. Separate models are estimated for the EEA countries (the European Economic Area) and for Sweden. Marginal effects are restricted to the range of country-level unemployment. Controls: country-fixed effects, sex, immigration background, birth year-fixed effects, dummies for seven educational levels and dummies for all other activities except paid work (reference category). Confidence intervals are based on country-clustered robust standard errors for the EEA sample, and on Huber-White robust standard errors for the Swedish sample. Underlying models are presented in the Supplementary material.

employed, including sex, immigration background, birth year-fixed effects and education-level dummies. On the contextual level, country-fixed effects control for among other things average differences in turnout across countries. To explore whether the Swedish case differs from the larger European one, separate models are fitted for Sweden and for the other European countries.

The positive slope signifies that the negative effect of individual unemployment is mitigated by contextual unemployment, in line with the results from previous research (Aytaç et al., 2018; Incantalupo, 2015). The higher the country-level unemployment, the less is the negative effect of individual unemployment. At a low level of unemployment (5%), the unemployed are about 13 percentage points less likely to vote both in Sweden and in the other European countries. At the average level of unemployment (about 7% both in the Swedish and in the European sample), the unemployed are 8 percentage points less likely to go to the ballot in Sweden, whereas the corresponding figure in the other European countries

is 10 percentage points. However, when country-level unemployment increases further, the negative relationship between individual unemployment and turnout approaches zero and becomes statistically insignificant. We have also run models using regional-level unemployment for the European countries and the results are very similar, although more imprecise (see [Supplementary Table A.4](#)).

The empirical pattern is strikingly similar in Sweden as in the other European countries, although the small Swedish sample results in imprecise estimates.⁷ If anything, the positive interaction between individual and contextual-level unemployment is stronger in Sweden than in Europe at large. These results show that the pattern of a possible mobilizing effect of contextual unemployment is not just a US phenomenon; it is also a widespread relationship in Europe at large as well as in Sweden.

4.2 Who are unemployed in times of low and high unemployment?

As discussed in the theory section, a possible objection to the findings indicating a mobilizing effect of contextual unemployment on the political participation of the unemployed is that these results could be explained by dynamic selection. That is, the composition of the unemployed may vary over the business cycle such that more resourceful individuals become unemployed when the unemployment rate is high; and these individuals may have a higher propensity to be politically active, regardless of whether they are unemployed or not. If this is the case, however, we should be able to observe these differences in resources among the unemployed according to the contextual level of unemployment.

Our Swedish register data offer good opportunities for exploring such empirical patterns. [Figure 3](#) uses binned scatter plots to demonstrate the relationship between on the one hand municipal unemployment and on the other hand individual turnout, parental turnout as well as two measures of educational performance. The latter three plots are included to show how the individual-level resources and the background of the unemployed vary with municipal unemployment. Parental turnout and educational performance (grades) are selected as they are variables that are well-known to correlate with political participation (e.g. [Persson, 2015](#); [Gidengil et al., 2016](#)) but are unlikely to be affected by individual unemployment. Municipal unemployment varies both across municipalities and over time.

The upper-left plot presents individual turnout among the unemployed. Again, we find that turnout tends to be higher among the unemployed when contextual unemployment is high. The upper-right plot instead shows the relationship between turnout for the parents of unemployed persons under conditions of low and of high contextual unemployment. Interestingly, the same pattern appears in this plot as in that for individual turnout. In high-unemployment contexts, the turnout of the parents of the unemployed is higher, even if the magnitude of the relationship is weaker than for individual turnout. Since parental turnout is mainly measured at elections prior to the turnout data for the individuals under study, it is highly unlikely that the voting behaviour of the latter would have affected their parents' turnout. A more plausible explanation for the observed pattern is that unemployed persons in high-unemployment contexts tend to be more positively selected and to come from politically more active families.

7 The interaction coefficient for the European sample is significant at the 99% level, whereas the corresponding coefficient does not reach significance for the Swedish sample ($P = 0.118$).

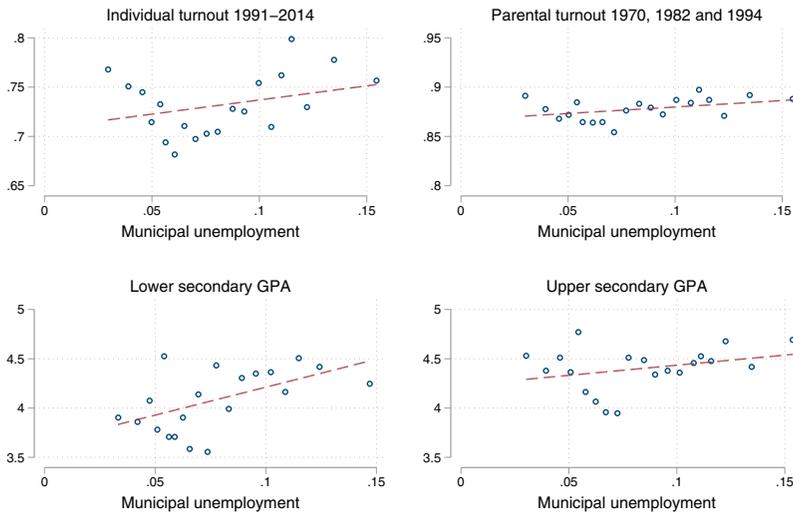


Figure 3 The composition of the unemployed population at varying levels of contextual unemployment, Swedish register data.

Notes: Municipal unemployment refers to the unemployment rate for the individuals in our sample at the elections 1991–2014. Parental turnout refers to the average turnout for the given individual's mother and father in the general elections of 1970, 1982 and 1994. Grade-point average (GPA) data are only available for the younger cohorts. Lower secondary GPA data are available for graduates from 1988 (typically born in 1972), while upper secondary GPA data include graduates from 1973 (typically born in 1952). Both lower- and upper-secondary GPA data are presented in deciles relative to the year of graduation, to avoid issues with grade inflation over time.

As for how the individual-level resources of the unemployed population changes over the business cycle, the lower pane of [Figure 3](#) shows the grades of the unemployed at different levels of contextual unemployment. The pattern resembles that found for turnout. Unemployed persons tend to have better average grades from both the lower secondary and upper secondary levels when contextual unemployment is high. As unemployment in adult years cannot affect grades during adolescence—while the opposite relationship is rather likely—the lower pane of [Figure 3](#) corroborates the proposition that there is dynamic selection of the unemployed depending on the overall unemployment rate.

To sum up, these plots support the idea that the unemployed in high-unemployment contexts are more positively selected both in terms of parental background and individual-level resources than in low-unemployment contexts. However, it still remains to find out how much the results of previous research are affected by this selection problem.

4.3 Main regression results

In [Table 1](#), we present findings from the models that exploit the full potential of the Swedish panel data. As in the case of the previous ESS models, we rely on a typical interaction specification, whereby we can examine how the effect of individual unemployment on turnout varies with contextual unemployment. To facilitate interpretation, municipal unemployment

Table 1 Contextual unemployment and turnout among the unemployed: Swedish data 1991–2014. Controlling for dynamic selection into unemployment

	(1)	(2)	(3)	(4)	(5)
Unemployed	-0.0844*** (0.00420)	-0.0797*** (0.00421)	0.0166 (0.0404)	-0.0130** (0.00595)	-0.0125** (0.00583)
Muni unempl	-0.345*** (0.0491)	-0.334*** (0.0457)	-0.322*** (0.0469)	-0.0654 (0.0491)	0.0381 (0.0838)
Ue × Muni ue	0.293*** (0.0970)	0.285*** (0.0951)	0.156 (0.186)	-0.265*** (0.102)	-0.276*** (0.100)
Parental turnout		0.198*** (0.00547)	0.190*** (0.00580)		
Adj. R2	0.071	0.084	0.085	0.86	0.86
Observations	5 825 837	5 825 837	5 825 837	5 825 837	5 825 837
Municipalities	277	277	277	277	277
Birth year FEs	Yes	Yes	Yes	No	No
Education control	Yes	Yes	Yes	No	No
Individual FEs	No	No	No	Yes	Yes
Municipal FEs	No	No	No	No	Yes
Ue × ind. controls	No	No	Yes	No	No

Notes: Municipality-clustered standard errors in parentheses. Municipal unemployment is mean-centered. All models include election dummies and Models (1)–(3) include controls for gender and immigration background. The education control adds dummies for seven levels of education. The sample is restricted to have the same number of observations across all of the models and includes only individuals with at least two observations on turnout. So-called singletons are excluded. The large number of interactions with *Unemployed* in Model (3) makes it difficult to interpret the standard OLS coefficient for *Unemployed*. In this table, therefore we report the average marginal effect of this variable across all observations.

* $P < 0.10$; ** $P < 0.05$; *** $P < 0.01$.

is means-centered in these models. This implies that the coefficient for *Unemployed* expresses the effect of unemployment on turnout at the average level of municipal unemployment (which equals 6.6%).

Model (1) employs the traditional control-based approach, using similar controls as in the ESS models above. According to this model, the unemployed are on average about 8 percentage points less likely to go to the ballot than the not unemployed—effectively the same result as for Sweden with the ESS data. As can be expected, furthermore, there is a positive interaction, meaning that the negative effect of individual unemployment decreases in high-unemployment contexts. In substantial terms, this would indicate that the unemployed are about 10 percentage points less likely to vote in municipalities with the lowest level of unemployment (an unemployment rate of 2%), whereas this negative effect amounts to approximately 6 percentage points at a high level of unemployment (15%).

Model (2) adds control for the turnout of the parents in the general elections of 1970, 1982 and 1994. Considering the strong influence of family background factors on political participation (e.g. Gidengil et al., 2016, Oskarsson et al., 2018), this should be a control with quite a lot of leverage on individual turnout. As expected, it is indeed positively

correlated with the turnout; however, it does not have much influence on our central estimates: i.e. on the coefficient for *Unemployed* and its interaction with municipal unemployment.

In Model (3) we use a much more flexible specification, allowing all of the individual controls to vary with individual unemployment by interacting these controls with the dummy for individual unemployment. This is one way of addressing the issue of dynamic selection of the unemployed, as the effect of individual unemployment is allowed to vary across variables that may affect turnout among the unemployed (different levels of education, for instance). For example, if high contextual unemployment means that a larger proportion of the unemployed population is well-educated—a trait which in turn is linked to higher turnout—then this selection effect could give rise to a positive interaction between individual and contextual unemployment. By separately estimating the effect of individual unemployment for different educational levels, we can at least partially factor out such effects of selection on the interaction coefficient between individual and contextual unemployment. The interaction coefficient does indeed become smaller and it loses its statistical significance, although this is partly due to the fact that the large number of interaction coefficients decreases statistical precision. However, the smaller size of the point estimate indicates that almost half of the original interaction effect reflects a conditional relationship with the other individual controls (which are correlated with municipal unemployment), rather than being the result of true interaction with municipal unemployment.

Even though Model (3) controls for dynamic selection to a greater extent than earlier models do (by including a number of ‘interaction controls’), the controls are limited by the available variables on observable characteristics. Exploiting the fact that we have panel data, therefore, we apply individual-fixed effects; this controls for all time-invariant variables, by relying on within-individual variation in turnout. That is, if contextual unemployment truly changes the effect of individual unemployment—whether in the direction of mobilization or in that of withdrawal—we would expect to see a difference in turnout when the same individual is exposed to high and low contextual unemployment. Model (4) in Table 1, consequently, applies individual-fixed effects. Since there is no or very little variation within individuals in the other individual controls, these are excluded. The result is that the lower-order coefficient *Unemployed* becomes substantially smaller, but it remains negative and significant at the 95% level. This implies that, when an individual becomes unemployed, he or she on average becomes 1.3 percentage points less likely to go to the ballot. Where the interaction coefficient is concerned, the effect has now completely turned around: it is now clearly *negative* at a confidence level of 99% and almost of the same magnitude as the positive coefficient in Models (1) and (2). In other words, Model (4) tells us that a high level of contextual unemployment makes the unemployed less likely to vote, strengthening the negative effect of individual unemployment.

Model (5) adds municipality-fixed effects, enabling us to explore whether general differences across municipalities could affect the estimates. However, these additional fixed effects make little difference to the central estimates. Thus, when controlling for unobserved individual characteristics, we find that a high level of contextual unemployment does not at all mobilize the unemployed; rather, it reinforces a withdrawal effect. The marginal-effects plot in Figure 4 facilitates interpretation. When contextual unemployment is low—less than about 6% on the municipal level—there is no significant negative effect of individual unemployment on turnout. However, when contextual unemployment rises to a high level (15%),

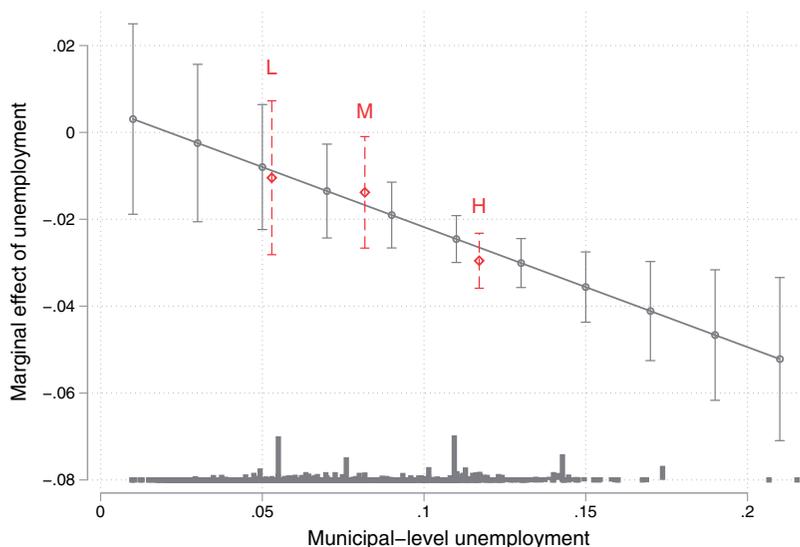


Figure 4 The marginal effect of individual unemployment on turnout at different levels of contextual unemployment. Swedish register data, based on Model (5) in Table 1; 95% confidence intervals.

Notes: The dashed spikes represent binned estimates at low, medium and high levels of contextual unemployment, to assess the linear character of the interaction. A Wald test cannot reject the null hypothesis of a linear interaction. See Hainmueller et al. (2019).

the negative effect of individual unemployment increases substantially to -3.6 percentage points compared with the effect of -1.3 percentage points at the average municipal unemployment level (6.6%).

A substantively interesting question, not the least in relation to the mechanisms at hand, is whether it is the within- or between-municipality variation in unemployment that drives the negative interaction. Put differently, is the negative reinforcing effect of contextual unemployment a result of how the unemployed react to unemployment changes *over time* in their municipality or because of how voters react when moving *between* municipalities with different levels of contextual unemployment?

In an attempt to answer this question, we decompose the variation in municipal unemployment using an approach inspired by Fairbrother (2014) and Schunk (2013). The results, presented in the [Supplementary material](#), suggest that it is predominantly the variation in unemployment within municipalities that explains the negative reinforcing contextual effect (see [Supplementary Table A.5](#)). The between-municipality component is also negative, but fairly imprecisely estimated. We interpret these results as lending support to the proposition that the unemployed mostly react to the state of their local labour market, and that movement patterns between municipalities play a minor role in the conditional effect of contextual unemployment.

These results imply that the rather substantial positive interaction effect between individual and contextual unemployment found in our earlier models is the result of dynamic selection into unemployment over the business cycle. Put differently, the mobilizing effect of high unemployment—suggested in previous research and replicated by us in Sweden as well as in

Europe at large—should not be given a causal interpretation. Rather, it can be explained by the fact that individuals of higher socioeconomic status also risk becoming unemployed in high-unemployment contexts.

Our results in [Table 1](#) also underline the limits of relying on control variables to address issues of selection. The controls included in Models (2) and (3) are quite extensive by most standards—rarely are such high-quality variables available in survey data—but the differences in the estimates are still substantial compared with those in the last two models that apply individual-fixed effects. The large difference between these estimates suggests there are important *unobserved* variables that affect turnout and which are related to unemployment on both the individual and the contextual level.

In contrast to many previous studies on the topic, we thus find that individual unemployment asserts a stronger negative effect on voting in high unemployment contexts. In the theoretical section, we discussed some potential mechanisms that could give rise to such a relationship. Unfortunately, whereas our register data have several advantages, they are admittedly more limited when it comes to studying mechanisms as we lack data on individual-level attitudes.

However, in an attempt to make some progress on this important issue, we explore the proposed mechanisms in two ways, focusing on the role of economic distress. First, we study how the economic circumstances of the unemployed impact the effect of contextual unemployment. More precisely, we can differentiate to what extent the unemployed are solely dependent on their own work income or live in a family where others may contribute to their livelihood—typically a partner. As we argue that contextual unemployment mainly affects the political participation of the unemployed by its influence on economic distress—because high contextual unemployment makes it harder to find a new job—we reason that someone who is unemployed and lacks an ‘insurance’ in the form of a partner with a reasonable income will become more negatively affected by high contextual unemployment. In other words, we would expect an unemployed person who lives alone, or with a partner with a low income, to be more likely to abstain when contextual unemployment rises. Second, we can approximate the opportunities for the unemployed to get a new job by separating individuals based on their education. The highly educated tend to in general have considerable better employment outlooks and should therefore be less affected by the economic distress of being unemployed in times of weak labour markets. That is, the highly educated who do lose their job may expect to reasonably quick get a new job even when contextual unemployment is high, whereas those with less education can be expected to be more strongly affected by high contextual unemployment and in turn economic distress.

In [Figure 5](#), we demonstrate how the marginal effect of unemployment is conditioned by contextual unemployment, partner income (left panel) and education (right panel). High or low partner income is defined based on whether the total work income of other adult family members belongs to the upper or lower half of that distribution. At low levels of municipal unemployment, there is little difference in the turnout of the unemployed depending on partner income. However, when contextual unemployment increases, turnout among the unemployed who lack a partner with a reasonable income is more adversely affected than turnout among those who may rely on their partner’s income. At a high level of unemployment (15%), the marginal effect of unemployment among those with a partner with a high income is -2.0 percentage points, whereas the corresponding effect amounts to -4.0 percentage points for those without such a partner. A similar pattern is revealed for the impact of

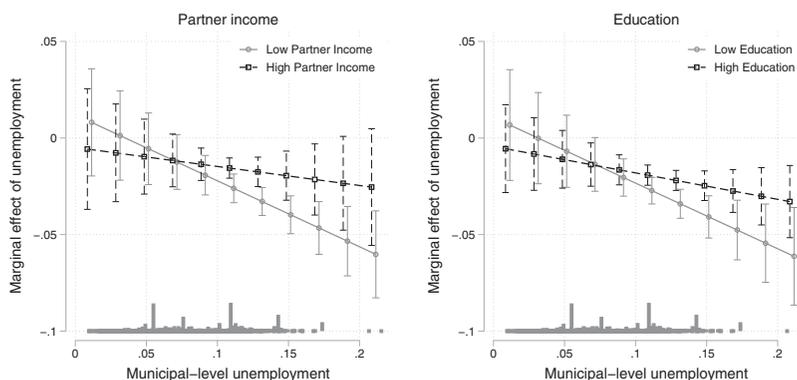


Figure 5 The marginal effect of individual unemployment on turnout at different levels of contextual unemployment, partner income and education. Swedish register data. 95% confidence intervals.

Notes: The plots are based on three-way interaction models using the same sample as in [Table 1](#) where the effect of individual unemployment is allowed to vary across municipal unemployment, partner income and education. High and low partner income is defined using the median of the sum of the income of other family members relative to year. We also include single households in the group with a low partner income. High and low education is based on median education, also relative to year. The confidence bands are slightly offset to not overlap. The difference in the marginal effect depending on partner income is significant at the 95% level for municipal unemployment levels between 10% and 20%. The corresponding figure for low and high education is 11% to 19% in municipal unemployment. Further details and the underlying models are presented in the [Supplementary material \(Table A.18\)](#).

education. At low levels of unemployment, education does not matter much for the turnout of the unemployed, whereas when unemployment increases the unemployed with a low level of education becomes increasingly less likely to go to the ballot. The effect is somewhat less pronounced than for partner income, but at an unemployment rate of 15% the marginal effect of unemployment is -2.5 percentage points among the highly educated and -4.1 percentage points among the less educated.

Although far from conclusive, these findings are thus consistent with the view that high contextual employment increases the negative effect of individual unemployment on political participation by increasing the sense of economic distress among the unemployed.

4.4 Robustness checks

We run a number of robustness checks to validate our main results in [Table 1](#). All of these are reported in detail in the [Supplementary material](#). First, the correct geographical level for contextual unemployment could be debated. We emphasize mechanisms related to economic distress and opportunities to find a new job and therefore use municipalities to measure the strength of the local labour market at a level that also constitutes a well-known polity. However, mechanisms related to, for instance, interactions among the unemployed would point towards a more fine-grained geographical level, whereas mechanisms related to national media and actions by the central government would suggest using the national level. We test the sensitivity of our estimates to this issue by re-running our models on several

alternative geographical levels. We use local labour market regions, the national level as well as small areas with a population of on average 2000 individuals. Reassuringly, the main results are similar regardless of the geographical level (see [Supplementary Tables A.6–A.8](#)).

Second, some scholars argue that the length of unemployment is important for the effect of individual and contextual unemployment on turnout ([Rosenstone, 1982](#); [Aytaç et al., 2018](#)). However, using our preferred specification with individual-fixed effects, we find a negative conditioning effect of contextual unemployment regardless of the length of the individual spell of unemployment ([Supplementary Table A.9](#)). A connected issue is the timing of our unemployment indicator as it is measured in November while the election takes place in September. We argue that this timing largely makes sense in relation to a mechanism emphasizing economic distress, as most persons who are about to lose their job would already be aware of this on Election Day. However, we test whether our estimates are sensitive to the timing of unemployment by using data on the duration of unemployment. We do this both by rerunning the main models and defining only those who have been unemployed for at least 120 days as unemployed, which would imply unemployment also on Election Day in the case of a single unemployment spell, as well as by varying the minimum duration of unemployment required to be defined as unemployed from 5 days up to the full year. The interaction coefficient for the conditioning effect of contextual unemployment on individual unemployment is remarkably stable regardless of specification ([Supplementary Table A.10](#) and [Figure A.1](#)).

Third, we test jobloss as an alternative measure of individual unemployment. This approach implies that we only look at unemployed persons who have recently lost their job and who used to have a reasonable wage income. The rationale behind this approach is to approximate the exogenous shock of losing your job. The general pattern of the results is similar to those of our main results, with a positive interaction in the control-based models that subsequently turns negative in the models that employ individual-fixed effects. The negative effects in these latter models, however, are smaller and less precise than in the main results ([Supplementary Table A.11](#)). This is likely a result of that we focus on a small group of unemployed that tend to be more resourceful.

Fourth, we allow for more flexibility on the municipal level to minimize the risk that systematic differences across municipalities bias our results. We interact the municipal-fixed effects with individual unemployment and the election dummies, thus absorbing possible differences across municipalities in the effect of individual unemployment and differences in electoral participation between municipalities over time. Furthermore, we interact the municipal-fixed effects with the individual-fixed effects, effectively using only the within-municipality variation for each individual. This specification thus factors out variation related to individuals moving in and out of municipalities, possibly in response to unemployment, which could drive selection. This model largely implies that we only study individuals who have not moved across municipalities since there are few individuals for whom we have more than two observations.⁸ The interaction between individual and contextual unemployment remains robust in all of these more flexible models ([Supplementary Table A.13](#)). As a

8 This is because for individuals to not become ‘singleton groups’ with this specification—and therefore dropped—at least two observations are needed within one municipality. Thus, for someone who moves, at least three observations are needed.

complement to these fixed-effects models, we also run a set of models where we explicitly control for some time-variant municipal-level variables which could be related to both turnout and the interaction between individual and contextual unemployment, namely, the share of migrants, the municipal population, the share of the population that is employed in manufacturing and the share that is not economically active. These controls are also interacted with individual-level unemployment to check whether the interaction between individual and contextual unemployment may be confounded by these other municipal-level variables. The interaction between individual and contextual unemployment is hardly affected by adding these control variables (Supplementary Table A.14).

Fifth, we test whether our results are sensitive to our choice of relying on the linear probability model by running logit models, using conditional logit for the individual-fixed effects models. However, the latter models are not possible to estimate in a way that is directly comparable with our main results, where the principal difference is that the conditional logit models are estimated only on individuals who have changed their voting behaviour over time. The logit estimates are similar to our main results in that there is a positive interaction between individual and contextual unemployment in the control-based models, whereas the interaction turns negative in our preferred specification with individual and municipal-fixed effects. However, the interaction coefficient is smaller and not statistically significant (Supplementary Table A.17). If we run the linear probability model on the restricted logit sample, the results are similar to the conditional logit model. Consequently, whereas the logit results support the view that the positive interaction effect found in previous research is driven by dynamic selection, the negative interaction effect is no longer statistically significant when using the conditional logit model. Although we believe that the linear probability model is preferable in this case because it avoids the sample selection problem involved in studying only a small subset of our data (Timoneda, 2021), the logit analysis indicates that more research may be needed before definitely concluding that the negative effect of individual unemployment on turnout is actually magnified in high unemployment contexts.

Finally, we also check that the results are not driven by our particular choice of sampling weights (Supplementary Tables A.15 and A.16).

5. Conclusion

Our results strongly indicate that the mobilizing effect of high contextual unemployment on the unemployed found in previous research is the result of dynamic selection rather than a causal effect. When we study how the same individual responds to different levels of contextual unemployment, we instead find a *negative* conditioning effect of contextual unemployment on turnout. That is, a higher level of contextual unemployment makes unemployed persons less likely to go to the polls. We have also shown that the unemployed population includes more resourceful individuals when the unemployment rate is high, which further corroborates the argument that the positive interaction effect between individual and aggregated unemployment is due to dynamic selection in and out of the unemployment pool.

Our findings are consistent with the results of the classical Marienthal study by Jahoda et al. (1971): i.e. unemployment leads to a withdrawal from political participation. Furthermore, this withdrawal effect only becomes stronger when the strain of

unemployment is shared by large numbers of others—notwithstanding the argument that economic grievances should furnish the unemployed with a strong motivation to mobilize (cf. Kern et al., 2015; Kurer et al., 2019). This result is as distressing today as it was in the 1930s; it implies that high unemployment can have adverse consequences on trust in democratic institutions. There is no mobilizing effect of high unemployment that can counteract a withdrawal from political action on the part of the unemployed in the event of economic downturns and mass unemployment. This is worrying in times of polarization and the dualization of labour markets (e.g. Kalleberg, 2009; Autor and Dorn, 2013; Lindvall and Rueda, 2014; Thelen, 2014), in which wage inequalities are not only growing greater; the risk of unemployment is being distributed more and more unequally besides. These trends risk resulting in greater inequalities in political participation as well.

While this study has its strength in a stronger empirical approach than that used in most previous research on political participation among the unemployed, it is not without limitations. First, we only study voter turnout; and the interaction between individual and contextual unemployment may very well look different for other forms of political participation. Scholars have argued, for instance, that feelings of grievance due to economic hardship primarily result in protest actions (Kern et al., 2015; Kurer et al., 2019). Nevertheless, voting is one of the least demanding forms of political participation; so it could be argued that a possible mobilizing effect should be discernible for turnout too. However, the effect of unemployment on other forms of political participation under different contextual circumstances is a topic that needs additional research. Second, while our approach—with its panel data and individual-fixed effects—relies on weaker assumptions than do typical cross-sectional approaches for identifying causal effects, it is not perfect. For example, there is often a considerable time gap in our data between our observations on individual turnout, and it is possible there may be time-dependent individual-level factors that bias our estimates. Third, while we argue there are reasons to expect the negative reinforcing effect of contextual unemployment to be weaker in Sweden than elsewhere (which gives some support for generalizing the Swedish case) the conditioning effect of contextual unemployment may play out differently in other countries. Our comparisons with other European countries suggest that Sweden is quite typical, but our results need to be tested in other countries as well. One interesting avenue for future research would thus be to examine the generalizability of our findings by performing similar studies in contexts with even higher unemployment rates than those found in Sweden during the study period, e.g. among youths in Southern Europe in the wake of the Great Recession. Similarly, it would also be valuable to further investigate how national institutions, such as unemployment insurance and other forms of social protection, may affect the findings of this study.

Acknowledgements

We thank the valuable comments from Costin Ciobanu, Miroslav Nemčok, Jan Teorell and seminar participants at the ECPR General Conference 2019 and the SWEPSA annual meeting 2019.

Funding

This work was funded by the Swedish Research Council (*Vetenskapsrådet*) under grant 2017-00764.

Supplementary material

Supplementary material is available at *Socio-Economic Review Journal* online.

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