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Master thesis

From Layoffs to Ballot Boxes: A Causal Examination of the Impact of Unemployment for Electoral Participation

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Abstract

What are the implications of individual unemployment for the incentive to vote? I use micro-level panel data on a large sample of Norwegian citizens to explore how periods of unemployment affect individual turnout in elections. To move beyond correlation, I exploit the panel data structure to address the omitted variable bias that arises with unobserved individual heterogeneity. I establish the causal link using within-individual variation in a multiway fixed effects model with individual fixed effects. I show that unemployment on the day of election increases an individual's propensity to vote, contrasting the observed lower turnout among the unemployed in aggregate data. Leveraging monthly labor market data, I conduct an extensive analysis of the immediate impact of unemployment at the moment of election and of the persistence of unemployment scars from past experience. While the mobilizing effect is the largest for joblessness in the month of the election, unemployment spells continue to affect turnout two years after they are endured for the young. The effect of prolonged unemployment experience within a year of election is larger than for short experience, but the extent to which turnout is affected by past unemployment lies mainly in the recency of the experience. These results negate the presumption founded on evidence from other countries that unemployment is a causal driver of the systematically high abstention rates among the unemployed.

Acknowledgements

Table of Contents

1   Introduction

One's labor market status could have direct implications for the incentive to vote. Labor market outcomes determine economic and political resources that can shape policy preferences and the perceived value of expressing them in the polls (Lipset & Rokkan, 1967; Riker & Ordeshook, 1968; Fiorina, 1978; Rosenstone, 1982). In modern societies, where workers dedicate the bulk of their adult lives to the labor market and where active employment is the norm, labor market outsiderness may influence one's self-perception and relation to society (Meld. St. 32 (2020-2021)) in ways that could affect the motivation to participate in elections for years to come (Schlozman & Verba, 1979; Emmenegger et al., 2017).

Statistically, the unemployed are less likely to vote than the employed: data from the 2019 Norwegian local elections reveal a turnout gap between the unemployed and the employed of 25 % (Statistics Norway, n.d., table 13778). While an interesting statistical observation, a substantial part of the discrepancy in electoral participation will according to existing empirical evidence be attributable to systematic socio-economic differences between employment groups (Adman, 2004; Emmenegger et al., 2017; Österman & Lindgren, 2021; Österman & Brännlund, 2023; Azzollini, 2023). Consequently, the causal effect of unemployment on participation in elections is concealed within a medley of alternative explanations for why the unemployed tend to refrain from voting. To establish the causal link, the variation in participation that arises from unemployment alone must be set apart from that of co-varying factors that otherwise contribute to the negative correlation that appears when votes have been cast and counted.

Previous research indicates that individual unemployment decreases turnout also when individual confounding factors have been controlled for (Österman & Lindgren, 2021; Österman & Brännlund, 2023; Azzollini, 2023), but joblessness has additionally been linked to the mobilizing mechanisms of protest voting (Emmenegger et al., 2015) and populist mobilization (Helske & Kawalrowiscz, 2023). Contradicting theoretical predictions and empirical evidence make the impact of unemployment on electoral participation an interesting point of analysis and signal that more research is needed.

In this thesis, I address the observed difference in electoral participation between the unemployed and the employed with an investigation into how individual turnout responds to unemployment when all heterogeneity is controlled for. I build a new dataset on a large

sample of Norwegian citizens using administrative micro-level panel data on individual turnout in elections, monthly labor market status, and demographic variables provided by Statistics Norway. The rich panel data provide for rigorous heterogeneity controls to adjust for observable and unobservable heterogeneity at the individual level, along with time-specific and geographic contextual factors. To tackle the strong selection that drives this relationship, I build on the empirical framework used in the most recent and methodologically advanced contributions to the literature by Österman and Brännlund (2023) and Azzollini (2023). I link individual electoral participation to individual unemployment by estimating a multiway fixed effects model with individual fixed effects in STATA 18 using the assembled dataset. Fixed effects estimation identifies the causal effect by controlling for the time-invariant unobserved heterogeneity that significantly confounds the relationship between unemployment and turnout. In particular, the individual fixed effect controls for the heterogeneity that originates from the unavoidable reality that people are different for reasons that are hard to capture in data (Angrist & Pischke, 2008). Using the within-estimator allows me to pinpoint how an individual's turnout changes after a transition into unemployment by holding all time-invariant characteristics intrinsic to them constant and comparing each individual to their past self (Ludwig & Brüderl, 2019).

Specifically, I am interested in the role of the timing of unemployment spells relative to elections. While the different demographic makeup of the unemployed and employed in the electorate is well-recognized in the literature, an aspect that has received less attention is the heterogeneity of individuals' labor market histories also leading up to the election. Static cross-sectional comparisons of turnout between employment groups on election day disregard that those who are unemployed on this day are more likely to have experienced unemployment in the past. The potential influence of past experience is often overlooked or assumed away due to data limitations. To my knowledge, only two prior studies have directly addressed these unemployment scars in electoral participation empirically and find evidence of persistent negative effects of unemployment in European countries (Azzollini, 2021; Azzollini, 2023). I return to these results in the literature review.

I aim to capture the full scope of how unemployment affects turnout. This is to improve the insufficient evidence on the distinctive impacts of unemployment endured at different times relative to elections. Accordingly, I follow the call for future research by Österman and Brännlund (2023) to explore the time dimension of this relationship more closely and ask: what are the immediate and medium-long run effects of individual unemployment on electoral participation? To this end, I exploit temporal variation in monthly

labor market status and analyze both the immediate and scarring effects of unemployment. This joint focus distinguishes my analysis from the literature, where it has been convention to study the two separately. For extended insights, I conduct two heterogeneity analyses. First, I investigate the role of the duration of experience. Second, I examine how the effects vary with the timing of unemployment over the life cycle, following the examples of Emmenegger et al. (2017), Österman and Brännlund (2023), and Azzollini (2023).

The monthly employment records used in this analysis present a methodological advancement over the annual data that has limited prior studies. Precise observation of the timing of unemployment spells makes it possible to clearly distinguish between unemployment in election months and experiences ahead of elections. This enables a comprehensive analysis of the temporal dynamics of the effects of unemployment through means not available to previous researchers.

I present consistent evidence that personal unemployment increases the propensity to vote in Norway, contradictory to empirical results from similar institutional and methodological contexts. The mobilizing effects of individual unemployment diverge from the initial hypothesis and demonstrate that the disparity in electoral participation rates between the unemployed and the employed is entirely driven by observable and unobservable confounding factors. Controlling for past experience, I find that turnout increases by 2.1 percentage points in response to unemployment on election day. Estimates reveal a sharp on-impact change in the propensity to vote exactly as the spell spans the election month. Experience with unemployment ahead of the election leaves scars in participation that fade from a 0.7 percentage point to a 0.3 percentage point increased voting propensity over a two-year span and disappear completely after two years. These results demonstrate that an account of unemployment occurring at different times over the election cycle is necessary to gauge the full implications of unemployment for electoral participation. Moreover, they point in interesting directions for potential causal channels.

This thesis contributes to the literature in several aspects. First, my results show that unobservable individual heterogeneity is a substantial source of endogeneity in examinations of voter turnout and unemployment and corroborate the conclusion in recent research that this must be accounted for to have credible causal inference. Second, I extend the analysis of this relationship to a previously unexplored geographic and institutional setting. The contrast between my results and prior findings emphasizes that the effects of unemployment can vary across institutional contexts. Next, I improve upon the academic understanding of the total impact of unemployment by calling attention to the conceptual difference between being

unemployed on election day and unemployment experienced ahead of the election, which may have different effects on the incentive to vote. Relatedly, I show that monthly accounts of unemployment eliminate the threat to validity that arises from annual employment data. On that account, this thesis makes a methodological contribution with the development of a research design that takes advantage of greater measurement precision with high-frequency labor market data to adequately address the time dimension in the relationship between unemployment and participation. Using Norwegian data, I replicate the methodological approach behind the Swedish results and show that my research design outperforms a weaker design based on annual data.

The remainder of this thesis proceeds as follows: section 2 gives an account of the theoretical framework and empirical evidence of the political behavior of the unemployed. Section 3 describes the data. Section 4 presents my identification strategy, empirical model, and estimation method. The results are presented in section 5. In section 6, I discuss my results and provide additional empirical insights from supplementary analyses. Finally, section 7 concludes.

2   Theory and literature review

The study of the electoral participation of the unemployed takes place within a wide field of electoral research, and the literature is divided on whether unemployment mobilizes or discourages voting.

On the one hand, withdrawal hypotheses propose that unemployment restricts individuals' socio-economic resources and induces psychological costs which disincentivize voting. Since active participation requires time and effort, voting has been classified as a "surplus phenomenon" that individuals only engage in when these resources are unconstrained (Rosenstone, 1982). The unemployed may optimally allocate less time and effort to political information acquisition and voting (Rosenstone, 1982), for example to devote more resources to active job search. Unemployment can involve a psychological toll of economic hardship and uncertainty about the future that reduces an individual's mental capacity for less immediate issues such as politics, leading to political disengagement (Brody & Sniderman, 1977; Rosenstone, 1982).

Unemployment may hinder the accumulation of political resources that are found to foster participation, like social organization and trade union membership, social contacts, and

access to political information (NOU 1980:7; Adman, 2004; Cox et al., 2023). The workplace can be a sociopolitical arena where workers are exposed to political discussion, acquire civic skills, and learn to vote through democratic workplace practices and interaction with politically active coworkers (Pateman, 1970; Brady et al., 1995; Adman, 2004). Time spent in unemployment may therefore depress future participation by hindering the formation of a political identity and political habits (Brady et al., 1995). Evidence indicates that the socialization mechanism could be decisive for the young: once developed and solidified during the critical "impressionable years", political preferences and participation behavior are sticky over the life cycle (Mannheim, 1952 [1928]; Dinas, 2010; Emmenegger et al., 2017).

Moreover, unemployment can affect perceptions and beliefs that relate to the value that one attaches to voting: job loss and unsuccessful job search may feel like a personal failure and have negative spillover effects on the trust in one's own capabilities, including self-perceived political judgment and competence (Schlozman & Verba, 1979; Emmenegger et al., 2015; Marx & Nguyen, 2016). Labor market outsidedness and social marginalization related to unemployment can also affect how individuals view themselves in relation to the rest of society (Turner, 1985; Schöb, 2013; Emmenegger et al., 2015). In turn, individuals can lose faith in political solutions and their vote's significance (Downs, 1957) up against the interests of the majority group of the employed. If the self-perceived internal and external political efficacy is low, the incentive to vote may be weak (Emmenegger et al., 2015; Marx & Nguyen, 2016).

On the other hand, unemployment may mobilize individuals to the polls. For one, the unemployed do not face the opportunity cost of foregone wages in the participation decision. The employed, however, may have to take time off work to vote on election day. Further, unemployment increases available leisure time which can foster participation if allocated to political search and political activities (Schlozman & Verba, 1979). Increased employment has for example been correlated with lower media consumption, political knowledge, and turnout in elections in the United States (Charles & Stephens, 2013).

If economic policy is perceived as a solution to personal labor market difficulties, unemployment could incite prospective pocketbook voting motivated by the desire to ensure political prioritization of pro-employment policies (Downs, 1957; Lipset, 1960; Fiorina, 1976; Brody & Sniderman, 1977; Schlozman & Verba, 1979; Emmenegger et al., 2015). The unemployed may have more to lose from not expressing their interests at the polls if abstention lowers the probability that pro-redistribution and pro-employment parties come to

power. Unemployment can also fit into retrospective models of voting behavior: unemployed voters could be urged to vote to electorally punish incumbent politicians for their personal economic condition (Key, 1966; Brody & Sniderman, 1977; Fiorina, 1978).

Abrupt life changes and economic distress induced by unemployment could radicalize political preferences and prompt individuals to seek drastic political change. Unemployment has been found to mobilize through engagement in protest voting in the Netherlands (Emmenegger et al., 2015) and increased responsiveness to populism in Sweden (Helske & Kawalrowiscz, 2023). Furthermore, unemployment exposes individuals to public welfare and labor services like the unemployment insurance system, which could stimulate political engagement through increased awareness and sensitivity to policy outcomes (Bauer, 2018). Self-experienced labor market challenges can also foster social conscience and solidary political preferences, possibly adding to the expressive and non-instrumental aspects of voting like a sense of civic duty (Riker & Ordeshook, 1968; Fiorina, 1976; Feddersen & Sandroni, 2006).

Like the potential causal channels, existing empirical evidence on the effects of unemployment on turnout is inconclusive. Most research is focused on participation outcomes of unemployment on election day. A positive correlation between unemployment and turnout rates has been demonstrated at the national and county level in the United States (Charles & Stephens, 2013; Burden & Wichowsky, 2014; Cebula, 2017; Cebula, 2019) and the sub-national level in the European Union (Azzollini, 2021). These indicate that turnout increases when unemployment is widespread. While the macro-level relationship in national and regional data can provide insights into the aggregated effects of unemployment, these geographic cross-sectional correlations are merely descriptive and conceal the extent to which high unemployment rates mobilize employed or unemployed voters.

With greater access to individual-level data, the ability to link individual turnout responses to individual joblessness has improved. Cross-sectional studies typically correlate individual unemployment to decreased political and electoral participation when observable heterogeneity is controlled for (Jahoda et al., 2017 [1933]; Rosenstone, 1982; Azzollini, 2021). Still, cross-sectional research fail to identify causal connections, as results based on between-variation at particular time points are likely to reflect remaining unobserved heterogeneity.

Recent research has contributed methodologically to the literature with the use of panel data and panel data methods like propensity score matching (Azzollini, 2023) and

individual fixed effects models (Azzollini, 2023; Österman & Lindgren, 2021; Österman & Brännlund, 2023). Improved individual-level controls with panel data demonstrate that unobserved factors drive cross-sectional correlations, yet researchers find that parts of the employment gap in participation remains. Though propensity score matching methods may fail to properly account for unobserved individual heterogeneity and results are susceptible to biases arising from methodological choices (Guo et al., 2020), negative estimates withstand in individual fixed effects estimations (Azzollini, 2023; Österman & Lindgren, 2021; Österman & Brännlund, 2023).

Evidence from Sweden and the United Kingdom suggests that unemployment decreases participation by approximately -1.5 to -3.0 percentage points (Österman & Brännlund, 2023; Azzollini, 2023). Results lend support to the socialization hypothesis by indicating that the negative effects are driven by the young, with small or statistically insignificant effects for older age groups (Azzollini, 2023, Österman & Brännlund, 2023). Swedish data could point to participation peer effects via the workplace: estimates indicate that the decrease in turnout is less pronounced for unemployed individuals whose previous coworkers participate to a larger degree, but the result lacks statistical significance (Österman & Brännlund, 2023).

Only two studies have directly assessed unemployment scars in electoral participation from unemployment experience in the past. They report evidence of electoral participation lasting several years following unemployment in the pan-European cross-section (Azzollini, 2021) and with British panel data (Azzollini, 2023). Contrarily, when the persistence of the effect is studied in an event study design on the restricted sample of individuals that eventually become unemployed, Azzollini (2023) finds a positive scarring effect that persists at least a decade after experiencing unemployment for those above 36 years, though the estimate is imprecise and not robust to fixed effects estimation with individual slopes. For the young, turnout is reported to remain depressed for five years after the first unemployment spell (Azzollini, 2023). Evidence of persistent unemployment scars contradicts Rosenstone (1982), who showed that the negative correlation in the cross-section dissipated for individuals who had been unemployed for more than sixteen weeks. On this basis, Rosenstone (1982) concluded that individuals return to their predisposed level of participation when the high initial economic and psychological costs of the unemployment transition subside as unemployment benefit payments start arriving and the individual adjusts to the shock of job loss. Lasting effects of unemployment have been detected in other political outcomes that could be linked to participation: Emmenegger et al. (2017) links youth unemployment to

depressed political interest, and Wiertz and Rodon (2019) find evidence of a leftward shift in political ideology in the Netherlands (Wiertz & Rodon, 2019), though the quantification of qualitative survey data in these studies can be debated.

Empirical work has so far been centered around strategies to overcome the identification problem that arises with individual heterogeneity but has put less emphasis on the conceptual distinction between facing unemployment on election day and carrying unemployment experience from the past. Most research has approached either one in isolation. This is despite a literature whose theoretical predictions of the turnout response, and the causal channels behind it, depend on whether an individual is unemployed on election day or the spell was experienced in the past. To my knowledge, only Azzollini (2023) accounts for both and finds that having unemployment experience negatively scars participation in the United Kingdom, but finds no immediate effect for transitions into unemployment in the election year.

Existing evidence is founded on annual data. The unobserved timing of unemployment spells within years presents a threat to the validity of these results, as researchers are constrained to unemployment measures that fail to distinguish between pre- and post-election transitions into unemployment. This data limitation has been coped with in different ways, each susceptible to measurement error and methodological biases. Analyses that rely on self-reported panel survey data are in addition vulnerable to attrition, recall, and social desirability biases (Rosenman et al., 2011; Althubaiti, 2016). Contradicting results, data limitations, and inadequate research designs in prior studies suggest that existing evidence fails to capture all aspects of how unemployment affects electoral participation. These gaps in the literature call for further research.

I posit five hypotheses to guide my analysis. On the grounds of the observed difference in turnout between the unemployed and the employed, the causal mechanisms proposed in the literature, and available empirical evidence, these hypotheses are:

H1) Unemployment on election day and unemployment experience decrease the propensity to vote.

If the latter has an effect, it can be labeled as an unemployment scar in electoral participation.

H2) The effect depends on the timing of unemployment relative to the election: the effect of being currently unemployed is different from the unemployment scars from past experiences.

H3) Unemployment scars in participation fade over time: the effect of unemployment experience is decreasing in the elapsed time between the experience and the election.

H4) The effect of experience depends on the length of the unemployment experience: the effect increases with the total time spent out of work.

H5) The effects of unemployment on election day and unemployment experience depend on the timing of the unemployment spell in the life cycle: the effect is decreasing with age.

3   Data

To study the effect of personal unemployment on individual electoral participation, I construct a dataset combining administrative micro-level panel data on turnout in elections, labor market status, and socio-economic characteristics for a large sample of Norwegian individuals.

My dataset includes three waves of observations, corresponding to the elections of 2015, 2017, and 2019. All elections were held in September. A general election was held in 2017, while 2015 and 2019 were municipal elections. Due to the gradual digitalization of municipal electoral data, I restrict the sample to individuals registered in the 27 municipalities included in the digital electoral register in 2015. This results in a balanced panel comprised of 1 487 732 individuals, of which 977 404 form the primary estimation sample. The dataset is organized at the individual-election year level. Table 1 gives a descriptive summary of the estimation sample.

Data on individual turnout are from the Electronic Election Administration System, which is the digital system applied in election administration in Norway (Valgdirektoratet, 2021). My unemployment measures are constructed using monthly labor market data from the Arena registry from the Norwegian Labor and Welfare Administration (NAV) spanning 120 months between January 2010 and December 2019. An individual is identified as unemployed in a month if they are registered as fully unemployed in the week of the 28th of that month.

Finally, data on individual socio-economic characteristics are retrieved from the National Population Register. I collect information on individuals' age, gender, immigration background, income, and educational attainment. The sample is restricted to individuals between 25-62 years of age in election years. This age interval corresponds to the core years of active labor force participation for the working population and excludes plausible students and retirees who are unlikely to be in the labor force**.** Gender and immigration background are captured by indicator variables that signal whether the individual is female and a first- or second-generation immigrant, respectively. Income is the natural logarithm of an individual's total net income. The education control is a categorical variable that specifies the individual's

highest completed education within four levels of educational attainment: lower secondary school (1), upper secondary school (2), undergraduate tertiary education (3), and graduate and postgraduate tertiary education (4).

The administrative data used in this study pose an advantage for inference through high coverage, reliability, and quality, along with lower probability of data collection biases related to self-reported data (Cole et al., 2022). Individual-level electoral and labor market data eliminate the issues of aggregation bias from relying on macroeconomic turnout and unemployment rates. Monthly employment records allow for greater measurement precision of unemployment within and between observation years. Though the timing issue of measuring unemployment in the election month after election day persists, misidentification is mitigated with monthly data. Elections were held at most two weeks prior to the data collection week. This short period of potential measurement error is unlikely to distort identification. Immediate termination is uncommon in Norway as most employment contracts are subject to terms of notice with a minimum legal period of notice set at four weeks (Working Environment Act, 2005, §15-2). Akin to the argument of Brännlund & Österman (2023), the legal regulations of employment contracts in Norway make it implausible that individuals who become unemployed in the two weeks between the election and the data collection point would be unaware of their job departure on election day.

Table 1. Descriptive statistics for the estimation sample

	Mean	Std. Dev.	Min	Max
Turnout in election	0,729	0,444	0	1
Unemployed election month	0,019	0,137	0	1
Unemployment experience	0,085	0,279	0	1
Recent experience	0,057	0,231	0	1
Distant experience	0,057	0,231	0	1
Duration of experience	0,457	2,077	0	23
Duration of recent experience	0,233	1,227	0	12
Duration of distant experience	0,224	1,159	0	11
Duration of experience if unemployed	5,353	3,256	1	23
Duration of recent experience if unemployed	4,115	3,256	1	12
Duration of distant experience if unemployed	3,922	3,011	1	11
Age	43,029	10,552	25	62
Gender	0,500	0,500	0	1
Immigrant background, 1st or 2nd gen	0,118	0,322	0	1
Net income (natural log)	12,716	0,995	0	19,260
Educational attainment (NUS, 1-4)				
1	0,173	0,379	0	1
2	0,294	0,456	0	1
3	0,364	0,481	0	1
4	0,168	0,374	0	1

Notes: Estimation sample. Ages 25-62. N 2 819 637, n 977 404.

4 Empirical strategy

This thesis aims to study how individuals' electoral participation changes in response to periods of personal unemployment. Causal inference on this relationship demands an empirical strategy that overcomes a multifold identification problem.

4.1 Research design: the quest for causality

4.1.1 An electoral snapshot

I take as a starting point the 25 percentage points gap in average turnout between the unemployed and the employed in the Norwegian population in the 2019 election that introduced this thesis (Statistics Norway, n.d., table 13778).

Since each individual is observed only this one day in September, we are left without an account of whether they vote when in other labor market positions or when circumstances are otherwise different. With only employment status observed, the difference in turnout is fully attributed to the fact that some were employed and others were unemployed, represented by Φ in the simple linear regression model:

$$vote_i \ = \ a \ + \ \Phi unemployed_i \ + \ \varepsilon_i \tag{1}$$

where *vote* is the binary 0-1 outcome variable of electoral participation by individual i, taking the value of 1 if the individual voted in the election and 0 otherwise, *unemployed* is a 0-1 indicator of whether the individual was unemployed in the election month, ε_i is the error term of unobserved factors that contributed to i's participation outcome.

A naïve comparison of turnout rates in the static cross-section results in the prediction that an individual will be 25 percentage points less likely to vote if they lose their job by the next election. If employment status was random in the population, we could expect that any other determining factors of participation would null out over all observations and be uncorrelated with employment (Wooldridge, 2012). Then a comparison of the average jobseeker to the average job holder would be a sufficient approximation to counterfactual outcomes and Φ = -25 percentage points would have identified the unbiased causal effect of unemployment.

However, there are many factors other than employment status that play into whether an individual votes or not, and this gap did not only arise because some individuals were on unemployment benefits rather than a payroll in September 2019. The average of errors is not

likely to be zero, but instead vary systematically with employment status in a manner that disturbs the relationship between turnout and unemployment. Since we cannot know how much of the 25 percentage point gap is caused by unemployment and how much can be explained by ε, the simple comparison in equation (1) is inapplicable for causal inference.

In a democracy where voting is voluntary, abstention does not lead to job loss. We can therefore rule out reverse causality as an explanation. The obstacle for causal inference in equation (1) is instead omitted variables and the inability to account for all relevant factors behind the observed difference in participation rates. Importantly, the turnout gap conceals that employment groups are systematically different and that some individuals are more likely to be unemployed based on factors that also make them predisposed to abstention in elections. Consequently, the employed are not a valid control group for the unemployed. Since the ideal experiment of random assignment of unemployment in a randomized controlled trial in the population is both infeasible and unethical, the empirical challenge lies in a research design that accounts for all ways in which participation and unemployment vary in the population to disentangle the effect of unemployment from that of all potential confounders contained within ε.

4.1.2   Controlling for demographics

Some sources of heterogeneity are observable: individual unemployment risk and voting propensity share common socio-economic determinants such as age, gender, educational attainment, income, and area of residence. Electoral participation is for instance lower among younger individuals, males, and the less educated at the same time as these characteristics are positively correlated with unemployment (Bø, 2005; Kleven, 2019; Meld. St. 32 (2020-2021); Statistics Norway, n.d., table 10440; Statistics Norway, n.d., table 13358; Statistics Norway, n.d., table 13785). Moreover, turnout rates are systematically different across municipalities (Statistics Norway, n.d., table 08243), and local labor market characteristics can determine unemployment prevalence and the composition of the unemployed stock in a given municipality (Andreev, 2016).

Controlling for observable confounding factors will filter out parts of the gap that can be attributed to other sources, previously camouflaged in ε. A one-period cross-sectional selection-on-observables approach results in following regression model:

$$vote_{im} = a + \Phi unemployed_{im} + \varphi X_{im} + \mu_m + \varepsilon_{im} \qquad (2)$$

where *vote* is the binary 0-1 outcome variable of electoral participation by individual i in municipality m, taking the value of 1 if the individual voted in the election and 0 otherwise, and *unemployed* is a 0-1 indicator of whether the individual was unemployed in the election month. X_{im} is the vector of individual-level socio-economic variables and includes gender, immigration background, educational attainment, income, and a fully flexible age control using yearly dummy variables for age. The municipality fixed effect, μ_m, is the set of dummy variables representing each municipality to hold all factors common to individuals residing in the same municipality constant, such as institutional characteristics and local norms. ε_{im} is the error term of all remaining unobserved contributors to i's participation outcome.

Φ is what remains of the turnout gap now that we know what part of ε in equation (1) was attributable to the different demographic composition of employment groups. Factoring in these observable covariates improves upon identification as we go from comparing turnout in the election of any unemployed and employed persons to comparing individuals with a similar predisposed propensity to vote based on their socio-economic profile.

Nonetheless, an estimation of equation (2) is still inadequate to establish a causal connection between unemployment and individual turnout. Certain sources of heterogeneity are hard to capture with cross-sectional data, and *ceteris* is seldom *paribus.* Analysis based on the state of affairs at one specific point in time produces conclusions that reflect undetected systematic differences between the employed and the unemployed and contextual factors at the time of comparison that are correlated with unemployment in the election month.

4.1.3   Controlling for experience

unemployed captures any individual who were unemployed in September of the election year. A hidden facet in the cross-section, often overlooked in empirical work, is the substantial variation in individuals' labor market experiences in any other month of the year. These are experiences that individuals bring into the election month and can have contributed to the decision to vote or abstain. Since employment status is strongly correlated over time and workers with unemployment history are more prone to become unemployed in the future (Arulampalam et al., 2000), many of the workers captured by *unemployed* will have past experience. Individuals who experience repeated and chronic unemployment could be

negatively selected, for reasons unrelated to observable demographics, which could drive the negative correlation.

Moreover, Φ could reflect the impact from past experiences rather than the isolated effect of being unemployed on election day. Then Φ could be a poor estimate for how turnout will change if an individual becomes unemployed shortly before the election. This is also because an individual who experiences unemployment for the first time might not respond in the same way as an individual with years of unemployment under their belt. Omitting unemployment experience could negatively bias the estimate if unemployment causes political alienation and permanently scars individuals' psyche, social identity, and self-perceived political efficacy as hypothesized in the literature. Alternatively, a current state of unemployment and past experience could have different implications for the cost-benefit analysis behind the participation decision and draw in opposite directions for the incentive to vote, also for the same individual. For example, having unemployment experience may activate political engagement in a way that incentivizes voting, while there could be large economic and mental costs to being unemployed at the moment which hinder the currently unemployed from allocating time and effort to cast their vote. If this were the case, the inability to precisely account for individuals' unemployment history could be a source of bias in the estimated effect of being unemployed on election day. Since demographic variables are strong determinants of labor market vulnerability, the employed individuals to whom we are comparing the currently unemployed will likely also have a history of unemployment. Failing to take the full picture into account could then lead to an overestimation of the impact of unemployment on turnout.

Observations on individuals' employment status in past periods controls for unemployment experience and introduce dynamics that enrichen the static model:

$$vote_{im} = a + \beta CURRENT_{im} + \theta EXPERIENCE_{im} + \varphi X_{im} + \mu_m + \varepsilon_{im} \quad (3)$$

where *EXPERIENCE* is an indicator equal to 1 for individuals who have experienced at least one unemployment spell in the two years leading up to the election.

To examine the dynamic relationship more closely, I dissect past labor market history further. This is to distinguish between fresh experience of unemployment within the recent past and experience that dates further back in time:

$$vote_{im} = a + \beta CURRENT_{im} + \lambda RECENTEXPERIENCE_{im} + \gamma DISTANTEXPERIENCE_{im} \quad (4)$$

$$+ \varphi X_{im} + \mu_m + \varepsilon_{im}$$

where *RECENTEXPERIENCE* captures unemployment experience in the 12 months leading up to the election, and *DISTANTEXPERIENCE* indicates unemployment experience in the semi-distant past 1-2 years (13-23 months) before the election.

Figure 1 illustrates the time dimension of unemployment spells relative to elections:
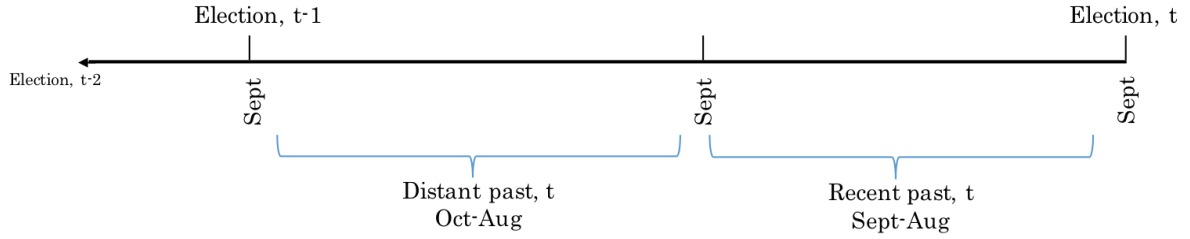
Figure 1 The relative time dimension. The figure illustrates the time dimension of unemployment relative to elections. Unemployed in election month is defined as unemployed in September of election years. Recent past is defined as within the immediate year of the election month in time *t*; the 12 months from September the previous year to August on the eve of the election in time *t*. Distant past is the year (11 months) from October following the previous election in time *t-1*, up to August one year prior to election *t*. Likewise for the two years between elections *t-2* and *t-1*.

4.1.4 Controlling for the electoral context

Equation (4) closes in on inference by comparing turnout in the election between individuals who share both demographic characteristics and labor market histories. Even so, conclusions based on voter turnout in a single election are unfit to predict how a transition into unemployment will affect participation in other elections. Participation rates and the composition of voters and abstainers can vary from election to election, depending on election-specific circumstances like the political climate, prominent issues of the electoral campaign, and the closeness of the electoral race. Notably, turnout is overall higher in general elections as opposed to local elections (Kleven, 2019). Additionally, the extent of unemployment in an economy varies cyclically and seasonally (Andreev, 2016), and there is a selection in the composition of employment groups at different points of the business cycle (Österman & Lindgren, 2021). To control for such unobservable election-specific trends and make estimates more generalizable, the identification strategy requires electoral data from several elections held at different points in time. This allows for the inclusion of an election-fixed effect, δ_t, in the following pooled cross-sectional regression equation:

$$vote_{imt} = a + \beta CURRENT_{imt} + \lambda RECENTEXPERIENCE_{imt} + \gamma DISTANTEXPERIENCE_{imt} \quad (5)$$

$$+ \varphi X_{imt} + \delta_t + \mu_m + \pi_{mt} + \varepsilon_{imt}$$

The election-fixed effect filters out the influence of contextual factors that are common across individuals in all municipalities in specific elections, while the election year-municipality interaction term, π_{mt}, is added to control for different time trends across municipalities. The other variables are defined as in the previously presented models.

4.1.5 Controlling for unobserved individual heterogeneity

The heterogeneity controls in equation (5) provide for a more consistent estimate of the effect of unemployment on electoral participation than the static and unconditional comparison between employment groups that we started with in equation (1). Although comparing observationally similar individuals allows for inference under less restrictive assumptions (Wooldridge, 2012), the model still relies on variation between individuals to estimate how the electoral participation of one and the same person is affected by a labor market transition. Yet, people are different in relevant aspects beyond what demographic features in a national register can convey. Unobservable individual characteristics like personality traits, genetics, chronic health issues, early life experiences, and resources from childhood could also confound the relationship by affecting both individual unemployment risk and voting propensity. Equation (5) neglects that people are inherently different and that unemployment risk ultimately varies at the individual level.

Identification is improved by exploiting that the dataset feature repeated observations of the same people: instead of comparing separate individuals, we can compare each person to themselves. Adding an individual fixed effect for each person in the sample to the model avoids confounding with individual idiosyncrasies by capturing all time-invariant characteristics of a person, observable as unobservable (Angrist & Pischke, 2008).

This establishes the causal link. We now know whether the individual votes before, during and after unemployment, and we can in a more consistent manner identify changes in participation in response to changes in employment status apart from any variation in socio-economic conditions since the last time they were observed.

Assessing within-individual variation as opposed to between-individual variation appears as an intuitive choice in microeconomic analysis of individual responses to subjective unemployment experiences (Hsiao, 2005; Ludwig & Brüderl, 2019; Ludwig & Brüderl, 2021). The inclusion of an individual fixed effect also reduces bias arising from the age-period-cohort collinearity problem which hinders perfect controls for aging, period, and cohort effects in empirical analyses with panel data, since the individual fixed effect will

absorb potential cohort factors and can be omitted from the regression model (Ludwig & Brüderl, 2021). Moreover, it controls for different directions of selection bias depending on macroeconomic circumstances (Österman & Lindgren, 2021). For example, a significant number of the unemployed around the 2015 election could be positively selected, owing to the oil crisis which led to a surge in job loss for relatively resourceful workers in the petroleum sector at the time (Ekeland, 2017). These individuals could be inherently prone to higher participation and lead to underestimation of the effect.

My identification strategy mounts to the following multiway fixed effects model of the participation decision:

$$vote_{imt} = a_i + \beta CURRENT_{imt} + \lambda RECENTEXPERIENCE_{imt} + \gamma DISTANTEXPERIENCE_{imt} \quad (6)$$

$$+ \varphi X_{imt} + \mu_m + \pi_{mt} + \varepsilon_{imt}$$

The individual fixed effect, a_i, determines unique intercepts for each individual (Angrist & Pischke, 2008) and establishes their baseline participation level. The model variables are otherwise defined as before, except for gender and immigrant background, which are time-invariant characteristics captured by the individual fixed effect and can be dropped from the vector of individual covariates, and the election-year fixed effect, which becomes redundant with the combination of the individual fixed effect and the flexible age control.

The coefficients of interest are β, λ, and γ which identify how unemployment at distinct periods relative to the election affects the propensity to vote. β is the immediate effect, while λ and γ estimate the existence and persistence of unemployment scars in participation. The decomposition of a general unemployment measure into distinctively timed components permits an exact analysis of how the turnout effect varies with the timing of joblessness relative to the election. Moreover, it allows for an assessment of the persistence of scar effects in the medium-to-long run. If it does not matter how much time has passed since the experience, then $\lambda = \gamma$. However, existing evidence indicates that unemployment scars diminish over time (Emmenegger et al., 2017; Azzollini, 2023), so that $|\lambda| > |\gamma|$, as per my third hypothesis.

Since the individual's participation outcome when in employment is recognized as the counterfactual, it is assumed that the individual would vote according to this had they not become unemployed and all else is otherwise equal (Ludwig & Brüderl, 2019). With all

time-variant individual heterogeneity controlled for, identification therefore hinges on the ability to account for all relevant time-varying confounders (Imai & Kim, 2019). Following from this, an identifying assumption is that unemployment spells before the observation period do not directly determine turnout in the elections included in the analysis, since variation attributed to earlier labor market experiences will be absorbed by the individual fixed effect and enter into each individual's baseline (Imai & Kim, 2019). I argue that although experience in the remote past may have determined an individual's baseline participation level, it does not confound the relationship between turnout and unemployment experienced between subsequent elections.

The concerns of temporal dependence and unobserved individual heterogeneity in cross-sectional turnout comparisons highlight that panel data is imperative for identification of the effect of unemployment. This is the main methodological consensus of recent empirical work on the relationship. Since unobserved characteristics are likely to be correlated with explanatory variables and unemployment risk is specific to each individual, a fixed effects model is preferred over a random effects model (Woolridge, 2012; Ludwig & Brüderl, 2019). A Hausman test is confirmative that the fixed effects model is a better fit for the data. To account for the correlation between observations of the same individual over time, standard errors are clustered at the individual level (Abadie et al., 2023).

The model is specified as a linear probability model. Estimated coefficients are therefore interpreted as percentage point deviations from the individual's mean turnout level induced by unemployment. Though logit and probit models are custom when the dependent variable is binary, the nature of voting behavior encourages linear probability estimation (Timoneda, 2021; Österman & Brannlund, 2023). Only observations with variation in the outcome variable contribute to estimation in logit regressions (Ludwig & Brüderl, 2019). Since voting habits are relatively persistent, fitting the participation decision in a logit model could result in the discard of a large number of observations. The reduction in sample size could weaken inference and reduce the efficacy of control variables (Timoneda, 2021). Logit regression could also be a source of bias if individuals vulnerable to unemployment are systematically more inclined to "participation switching". The shortcomings of the linear probability model, like predicted outcomes not bound to zero and one (Angrist & Pischke, 2008), appear to constitute a smaller threat to identification. Opting for the linear probability model also makes results comparable to previous empirical work where LPM is used for estimation (Österman & Lindgren, 2021; Österman & Brännlund, 2023; Azzollini, 2023).

4.2   Heterogeneity analyses

To look beyond the average participation effects of unemployment, I investigate how the effects vary with contextual factors of unemployment spells.

4.2.1   Duration of experience

An unemployment experience can be characterized in three dimensions; exposure, timing, and duration. All three deserve assessment for a complete analysis. Controls for observed and unobserved heterogeneity address differential exposure to unemployment in the population, and the timed decomposition of the experience measure accounts for the timing of spells relative to the election. I next turn to the role of the duration of unemployment experience. On the one hand, persistent unemployment could disrupt individuals more than shorter spells; the effect of taking two pills is larger than that of one. Cumulative months out of work could exacerbate the scarring effect, as proposed in my fourth hypothesis. Alternatively, individuals with prolonged experience could be less affected if they have habituated to unemployment; taking many pills results in immunity.

To see how the scarring effect varies with the duration of unemployment experience, I substitute the *RECENTEXPERIENCE* and *DISTANTEXPERIENCE* variables in equation (6) with flexible measures for duration. I define four experience categories using the number of months of unemployment in each of the two years since the previous election, as specified in section 4.1.3. The categories distinguish between short experience (1-4 months), medium experience (5-8 months), and long experience (9-12 months). The reference category is uninterrupted employment and zero months of experience in the specified time interval. Conceptualizing unemployment experience as the total sum of months out of work accounts for the intensity of experience both in terms of the frequency and duration of spells. This aligns with methodological approaches in other research fields where individuals receive treatments of varying intensity (Warren et al., 2007; Neil & Jones, 2015)**.**

4.2.2   Timing of unemployment over the life cycle

A second heterogeneity analysis explores how the effect varies over life cycle phases. In equation (7), I include an interaction term between the unemployment measures and a categorical age group variable which captures individuals above 40 years of age. Doing so allows for an analysis of whether effects differ for the young and potentially more impressionable as opposed to the older and politically habituated. The 40-year age threshold

is in accordance with theoretical age predictions of participation behavior and existing empirical evidence (Emmenegger et al., 2017; Österman & Brännlund, 2023; Azzollini, 2023). The model used in the life cycle analysis is given by:

$$vote_{imt} = a_i + \beta CURRENT_{imt} + \lambda RECENTEXPERIENCE_{imt} + \gamma DISTANTEXPERIENCE_{imt}$$
$$+ (\Psi CURRENT_{imt} + \eta RECENTEXPERIENCE_{imt} + \omega DISTANTEXPERIENCE_{imt})(40plus_{imt}) \quad (7)$$
$$+ \varphi X_{imt} + \delta_t + \mu_m + \varepsilon_{imt}$$

The model is otherwise specified as in equation (6). Since the flexible age control absorbs turnout differences attributable to age, the *40plus* category is redundant and can be omitted.

5   Results

This section presents the results of my analysis. Table 2 reports the immediately observable correlation between unemployment and turnout. In Table 3, I present the regression results from my main analysis. Tables 4 and 5 show how the effect of unemployment varies with unemployment duration and age, respectively. I end with a description of the robustness checks that validate these findings.

5.1   Descriptive results

Table 2. Turnout gap by employment status in election month

	U-E 2015	U-E 2017	U-E 2019
Δ Pr(voted 2015)	-0.148	-0.168	-0.159
Δ Pr(voted 2017)	-0.128	-0.121	-0.144
Δ Pr(voted 2019)	-0.174	-0.159	-0.148

Notes: Estimation sample. Ages 25-62.
U = unemployed in election month; E = employed in election month
Δ Pr(voted) = Pr[vote | U] - Pr[vote | E]

Table 2 demonstrates the negative correlation between unemployment and electoral participation.

The main diagonal corresponds to the electoral snapshot of equation (1) and reports the unadjusted turnout gap between the unemployed and the employed in each of the three elections. U is the group of unemployed individuals, and E is the group of the employed. To see how their relative participation rates have evolved over time, the columns pin down these groups of comparison. The rows give the difference in mean turnout between them in each

election. For example, the middle column, *U-E 2017*, compares the difference in turnout between those who were unemployed and those who were employed in September 2017 in the three elections. For this cohort, 2017 is the base year, when the turnout gap was:

$$\Delta \, Pr(voted \; 2017) \;\; = \;\; Pr[vote_{2017} \, | \, unemployed_{2017}] \;\; - \;\; Pr[vote_{2017} \, | \, employed_{2017}]$$

and two years later, in 2019:

$$\Delta \, Pr(voted \; 2019) \;\; = \;\; Pr[vote_{2019} \, | \, unemployed_{2017}] \;\; - \;\; Pr[vote_{2019} \, | \, employed_{2017}]$$

The cells of the main diagonal demonstrate that the unemployed were 12.1 to 14.8 percentage points less likely to vote come election day. Even so, the notable insight from Table 2 is the consistently negative gaps in participation irrespective of whether one considers the cohorts' base year or the past and future elections. This demonstrates a strong negative correlation between unemployment and participation in elections. Most notably, the above-diagonal cells disclose lower turnout among individuals who eventually will become unemployed. This is a strong indication that individuals vulnerable to unemployment abstain relatively more from voting.

5.2   Regression results

Table 3 reports the estimated relationship between unemployment and electoral turnout from estimations of the empirical models presented in section 4.1. I first estimate the models with only the election month employment status before I account for past unemployment experience, gradually progressing toward the extended model in equation (6).

The models estimated in columns (1), (2), (4), and (6) are pooled OLS regressions relying on between-variation. The model in the leftmost column does not include controls and reports the unadjusted correlation between turnout and unemployment on election day, the other three rely on the selection-on-observables approach presented in equation (5). The specifications estimated in columns (3), (5), and (7) add the individual fixed effect. All models are estimated on the same sample.

Table 3. Unemployment and turnout

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Current	-0.143***	-0.031***	0.025***	-0.004	0.023***	-0.006*	0.021***
	(0.00193)	(0.00211)	(0.00205)	(0.00217)	(0.00209)	(0.00230)	(0.00224)
Experience				-0.035***	0.006***		
				(0.00114)	(0.00117)		
Recent experience						-0.017***	0.007***
						(0.00150)	(0.00147)
Distant experience						-0.032***	0.003*
						(0.00135)	(0.00135)
Adj. R-sq	0,002	0,154	0,503	0,154	0,503	0,154	0,503
N	2819637	2819637	2819637	2819637	2819637	2819637	2819637
n	977404	977404	977404	977404	977404	977404	977404
SES controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Election FEs	No	Yes	No	Yes	No	Yes	No
Muni x Elec FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
Individual FEs	No	No	Yes	No	Yes	No	Yes

Standard errors clustered on individuals in parentheses * p<0.05, ** p<0.01, *** p<0.001
Notes: Ages 25-62. Unemployment indicators (0-1). *Current* indicates unemployed in the election month. *Experience* indicates unemployment spells in the 2 years (23 months) since the previous election. *Recent experience* is defined as spells within a year (12 months) of election; *distant experience* is defined as spells 1-2 years (13-23 months) before election. The outcome variable is turnout in elections (0-1). SES controls include controls for age, income, education. Gender and immigration background control for model (1). Models (1), (2), (4), (6) are pooled OLS regressions. Models (3), (5), and (7) are individual FE estimations.

I show first how turnout varies by employment status in the election month.

The results in columns (1)-(3) demonstrate that the observed difference in participation rates between the unemployed and employed in elections is driven by underlying heterogeneity. Observable confounding socio-economic factors, regional differences, and election-specific trends account for 11.2 percentage points of the turnout gap; controlling for these reduces the negative correlation from -14.3 percentage points in column (1) to -3.1 percentage points in column (2).

Even so, the findings reported in column (3) indicate that any remaining prediction of a withdrawal response to unemployment in the cross-section can be attributed to unobserved individual heterogeneity. When individual fixed effects are controlled for, the estimated effect switches from negative to positive. Individuals are estimated to be 2.5 percentage points more likely to vote in elections if they are unemployed in the election month, compared to elections where they are not unemployed. This negates my first expectation that unemployment causes electoral withdrawal.

I next account for unemployment experience.

I first apply the indicator for experience at any point in the two-year past, as defined in equation (3). The pooled OLS results in column (4) indicate that all of the effect of unemployment is driven by past experience, which decreases individuals' voting propensity by 3.5 percentage points, whereas being unemployed on election day is not estimated to affect the propensity to vote. This attests to a strong correlation between unemployment risk and past unemployment that biases Φ in equation (2).

In contrast, adding the individual fixed effect in column (5) again changes conclusions significantly. There is a small decrease in the estimated effect of unemployment in the election month which reflects an upward bias from omitting past experience. When experience is controlled for, an individual is 2.3 percentage points more likely to vote when they are currently unemployed. Experience with unemployment since the last election increases voting propensity by 0.6 percentage points. This non-negative and significant estimated effect of past unemployment history suggests the existence of unemployment scars in turnout.

Column (7) shows the persistence of these unemployment scars as indicated by the estimated β, λ, and γ coefficients from the extended model in equation (6). Further decomposition of past experience reveals that the scarring effect decreases over time: recent experience within a year of election day boosts turnout by 0.7 percentage points, while the effect has decreased to 0.3 percentage points for unemployment spells one to two years in the past. The coefficients of recent and distant experience are statistically different, and the less precise estimate of distant experience attests to a diminishing scarring effect. Still, the data suggest that current unemployment has the strongest mobilizing effect at 2.1 percentage points. The drop in the coefficient for unemployment on election day in column (3) shows that controlling for experience is relevant since omitting it leads to an overestimation of 0.4 percentage points. Ergo, my second and third hypotheses are corroborated.

As before, the estimation of the model without the individual fixed effect in column (6) predicts the opposite dynamic relationship between turnout and unemployment, driven by the strong state dependence in unemployment over time (Arulampalam et al., 2000).

In summary, the regression results in Table 3 illustrate how the observed, raw participation gap between the unemployed and the employed is closed and eventually reversed when controlling for confounding factors. As such, my analysis amounts to the prediction that

unemployment experiences have a mobilizing effect on individual electoral turnout. The decreasing scarring effect indicates that individuals converge to their baseline turnout level over time. The discrepancy in inferences based on between-variation compared to within-variation underscores the influence of unobserved heterogeneity for identification since the two identification strategies paint two completely different pictures of how unemployment affects electoral participation.

I now shift focus to the role of unemployment duration for turnout and progress to a joint analysis of the temporal and duration dimensions of the relationship. In Table 4, I estimate equation (6) with the categorical experience categories defined in section 4.2.1. Zero months of experience is the reference category.

The results reveal that the extent to which past experience materializes in turnout depends on the cumulative duration of experience, but most importantly on the proximity to the election. Firstly, the estimate for being unemployed on election day is robust to accounting for the duration of experience. Moreover, joblessness in the election month remains the most decisive for participation, with an estimated 2.0 percentage points increase in voting propensity.

We see that the duration of experience only matters for turnout if the unemployment experience is recent. The estimated scarring effects of recent experience vary with duration: short experience of 1-4 months within a year of the election incites the lowest turnout response and raises voting propensity by 0.6 percentage points. Slightly deeper scars in participation are found for experiences of longer duration, after which individuals are 1.1-1.2 percentage points more likely to vote. The effects of 5-8 months and 9-12 months of experience are statistically equivalent, thus the main distinction in responsiveness to experience is between short experience and 5+ months of experience.

The estimated coefficients for unemployment two years before the election are lower in magnitude and lack statistical power. According to the data, unemployment two years back only plays into the decision to vote for short experience between 1-4 months, though the estimated effect is small and less precise. The result that even a full year of unemployment does not induce a scar in participation if experienced more than one year before the election contradicts my expectation that turnout responds more to prolonged experience. On the other hand, it provides further evidence of the diminishing scar effect and corroborates my third hypothesis that unemployment principally has an immediate effect on turnout.

Table 4. Unemployment, duration of experience, and turnout

	(1)
Current	0.020***
	(0.00239)
1-4 months recent	0.006***
	(0.00160)
5-8 months recent	0.012***
	(0.00269)
9-12 months recent	0.011**
	(0.00370)
1-4 months distant	0.003*
	(0.00152)
5-8 months distant	0.002
	(0.00253)
9-12 months distant	0.001
	(0.00358)
Adj. R-sq	0.503
N	2819637
n	977404
SES controls	Yes
Municipality FEs	Yes
Election FEs	No
Muni x Elec FEs	Yes
Individual FEs	Yes

Standard errors clustered on individuals in parentheses. * $p<0.05$, ** $p<0.01$, *** $p<0.001$
Notes: Ages 25-62. Unemployment indicators (0-1). *Current* indicates unemployed in the election month. Experience as cumulative duration of experience. Recent is defined as within a year (12 months) of election; distant is defined as 1-2 years (13-23 months) before election. The outcome variable is turnout in elections (0-1). SES controls include controls for age, income, education. H0: (5-8mo recent=9-12mo recent), $F(1,977403)=0.06$, $p=0.8081$.

Turning next to the impact of the timing of unemployment over the life cycle, I introduce the above 40 age group interaction term as specified in equation (7). In Table 5, I compare participation responses to unemployment between the older and younger age groups.

Table 5. Unemployment, age, and turnout

	(1)
Current	0.024***
	(0.00319)
Current x 40y+	-0.007
	(0.00444)
Recent experience	0.006**
	(0.00202)
Recent x 40y+	0.003
	(0.00292)
Distant experience	0.007***
	(0.00186)
Distant x 40y+	-0.009***
	(0.00266)
Adj. R-sq	0.503
N	2819637
n	977404
SES controls	Yes
Municipality FEs	Yes
Election FEs	No
Muni x Elec FEs	Yes
Individual FEs	Yes

Standard errors clustered on individuals in parentheses * p<0.05, ** p<0.01, *** p<0.001
Notes: Ages 25-62. Unemployment indicators (0-1). *Current* indicates unemployed in the election month. *Recent experience* is defined as spells within a year (12 months) of election; *distant experience* is defined as spells 1-2 years (13-23 months) before election. The outcome variable is turnout in elections (0-1). 40y+ is an age group indicator (0-1) for individuals aged 40 years and older. SES controls include controls for age, income, education. $H0: \hat{\lambda} = \hat{\gamma}$: $F(1,977403)=0.18$. $p=0.6726$. Estimated coefficient (std. err.) of distant experience for the 40y+ group is -0.00199 (0.0019245), $t=1.34$, $p = 0.179$.

There are two points to note from Table 5. We first see by insignificant interaction terms that individuals of all ages respond equally to unemployment in the election month and experience within a year of election day. This finding counters my fifth expectation. Next, the data suggest that unemployment scars in electoral participation disappear at a faster rate for older adults. While the reported estimates indicate that unemployment two years back decreases turnout by -0.2 percentage points for older individuals, the estimated coefficient for the 40-plus age group is not significant. For the young, the estimated effect of recent and distant experience is not statistically significant, which attests to a more persistent effect of unemployment spells in earlier life stages. These results are consistent with the conjecture that voting behavior becomes more stable over the lifetime.

5.3   Robustness checks

I test these findings with a series of robustness checks. The results are provided in the Appendix.

I show that the negative-to-positive switch in the estimated direction of the relationship when accounting for individual fixed effects is consistent across all robustness tests of the main analysis, affirming the shortcomings of selection-on-observables methods in the presence of significant unobserved heterogeneity.

I first estimate equations (5) and (6) with continuous measures for unemployment defined as the cumulative number of months in unemployment in the specified period and report the results in Table A.1 in the Appendix. The estimated effect of unemployment in the election month is robust to the continuous specification. Moreover, the data indicate a curvilinear pattern of unemployment scars in electoral participation over the range of duration. The flexible specification with categories classified by duration of experience estimated in Table 4 is still favored since it allows for heterogeneous effects across experience groups and does not force a quadratic functional form on the data.

Table A.2 shows that the results remain stable in estimations including singleton observations. Singletons are automatically dropped in estimations using the *reghdfe* STATA package to avoid inflated statistical significance due to underestimation of the clustered standard errors in multiway fixed effects estimations (Correia, 2015). These are individuals who are observed once and therefore do not identify the model or have missing values for any of the model regressors (Correia, 2015). Examples of singletons in this balanced panel are individuals that only fall within the restricted age interval for one election, or individuals reported with negative net income which becomes a missing value in the logarithmic transformation. The latter case could have been a source of bias if the unemployed were more likely to be indebted.

In Table A.3, I show that the results are robust to extending the age interval to individuals between 20 and 62 years, which includes more of the "young and impressionable".

6   Discussion

I find positive effects of unemployment on electoral participation for individuals of working age when controlling for individual time-invariant heterogeneity. This finding diverges in

several aspects from results in other panel studies relying on individual fixed effects for identification, where unemployment has been estimated to decrease individual turnout in Sweden (Österman & Brännlund, 2023) and the United Kingdom (Azzollini, 2023). These studies report estimated effects between -1.5 to -2.7 percentage points with further decreases for the young. Moreover, Azzollini (2023) finds negative unemployment scars from past experience but no effect for unemployment in the election year. The results presented in this thesis imply contrarily that unemployment above all affects the propensity to vote if one is currently or has recently been unemployed. This pattern of effects is irrespective of age and length of experience.

These findings raise new questions. First, why is the turnout response to unemployment transient and firmly connected to the proximity to the election? Second, and perhaps above all, why are Norwegian workers mobilized by unemployment? In this section, I look further into the time dimension of the relationship in an attempt to gain insight into what could drive the participation behavior of the unemployed; albeit a precise identification of the causal mechanisms is beyond the scope of this thesis. Additional analyses strengthen the inference from the main analysis that unemployment has an immediate mobilizing effect on electoral participation that is indeed separate from and larger than for past experience. I finish with a discussion of my findings in light of previous evidence.

6.1   Understanding the participation behavior of the unemployed

Moderate estimated effects and evidence of a decreasing scarring effect indicates that people return to their individual baseline level of participation shortly after an unemployment experience. This result is consistent with empirical evidence that voting behavior is habitual and inertial (Plutzer, 2002; Gerber et al., 2003; Denny & Doyle, 2009; Alfaro-Redondo, 2014). These analyses are conducted on individuals above the age of 25. By then, they have been eligible to vote for seven years and at least three elections, and will likely have established a voting habit. My results indicate that these habits are quite resistant to life changes such as labor market transitions, but less so when transitions occur near election day.

6.1.1   Unpacking the time dimension

I now illuminate how unemployment in the election month is more impactful for turnout than the experience from unemployment spells that took place in the past.

An immediate explanation is memory: the near-null and decreasing scar effect suggests that past spells are faded memories by the election. Relatedly, ongoing

unemployment could be more disruptive because it directly pertains to the individual's situation*,* perceptions, and outlook in the moment with immediate implications for the costs and expected benefits of participating in the election. Past unemployment, however, could be less relevant at the time of the election.

Still, the finding that unemployment experience can materialize in electoral participation two years after it was endured, notably for the young, motivates an inquiry into if and when individuals fully readjust after unemployment. It may be that the causal channels from current unemployment and unemployment experience to turnout are different. The effect of being unemployed in the election month could be immediate, direct, and common to all affected, for example reflecting that being jobless frees up time and energy for political participation relative to periods when employment and work constrain these resources. The mechanism relating past unemployment to voting could in contrast be slower, less direct, and only apply to certain subgroups. Unemployment and labor market disadvantage has previously been found to affect ideological convictions (Emmenegger et al., 2015; Marx & Nguyen, 2016; Wiertz & Rodon, 2019) and political interest (Emmenegger et al., 2017). As political preferences are typically sticky, it may take more time for these factors to manifest in turnout. This causal channel may only be open to the young and "impressionable" and explain why older individuals recover their participation habits faster.

When, relative to the election, does the effect of unemployment change? Tables 6 and 7 provide additional insights into the dynamics of the relationship, which underline the distinctive role of the proximity to election day.

6.1.2 Unpacking the time dimension: zooming out

So far, this analysis has considered the relationship within a two-year timeframe. I explore next how unemployment experiences further back in time affect the propensity to vote by extending from the two-year to the four-year labor market history before each election. The model estimated in column (1) of Table 6 includes the lagged values of current unemployment and unemployment experience to equation (6).

While the coefficients of current unemployment and unemployment experience within the two-year past remain significant and unaffected by the inclusion of the lags, none of the lagged variables are significant. Interestingly, being unemployed at the time of the previous election does not affect the propensity that you turn out for the current one.

To determine the persistence of the unemployment scar among the young, I include age group interactions in the model estimated in column (2). The lags remain insignificant. As

in Table 5, the estimated coefficient of distant experience for the above-40 group is not significantly different from zero. With no evidence of unemployment scars in participation from unemployment experience beyond two years, the data indicate that participation is unchanged by unemployment in the long term.

Table 6. Unemployment, time, and turnout: the 2-year versus the 4-year labor market history

	(1)	(2)
Current, t	0.021***	0.024***
	(0.00240)	(0.00338)
Current x 40y+		-0.005
		(0.00467)
Recent experience	0.007***	0.006**
	(0.00148)	(0.00203)
Recent x 40y+		0.003
		(0.00294)
Distant experience	0.004*	0.009***
	(0.00149)	(0.00204)
Distant x 40y+		-0.011***
		(0.00296)
Unemployed, election t-1	-0.000	-0.003
	(0.00240)	(0.00337)
(Unemployed, el. t-1) x 40y+		0.008
		(0.00469)
2-3y before election t	-0.001	-0.001
	(0.00051)	(0.00071)
(2-3y before) x 40y+		-0.000
		(0.00102)
3-4y before election t	-0.001	-0.000
	(0.00044)	(0.00061)
(3-4y before) x 40y+		-0.001
		(0.00087)
Adj. R-sq	0.503	0.503
N	2819637	2819637
n	977404	977404
SES controls	Yes	Yes
Municipality FEs	Yes	Yes
Election FEs	No	No
Muni x Elec FEs	Yes	Yes
Individual FEs	Yes	Yes

Standard errors clustered on individuals in parentheses .* p<0.05, ** p<0.01, *** p<0.001
Notes: Ages 25-62. Unemployment indicators (0-1). *Current* indicates unemployed in the election month. *Recent experience* is defined as spells within a year (12 months) of election; *distant experience* is defined as spells 1-2 years (13-23 months) before election. Lags: Unemployed election t-1, 2-3y (25-36 months) before election t, 3-4y (37-48 months) before election t. 40y+ is an age group indicator (0-1) for individuals aged 40 years and older. The outcome variable is turnout in elections (0-1). SES controls include controls for age, income, education. Estimated coefficient (std. err.) of distant experience for the 40y+ group is −0.0025 (0.0021644), t=-1.16, p=0.245.

6.1.3 Unpacking the time dimension: zooming in

How close up to the election does the effect of unemployment change? Returning to the two-year time frame, I decompose the measure of past unemployment experience further with quarterly indicators for employment status. The estimation result of equation (6) with quarterly time intervals for experience is reported in column (1) of Table 7.

Table 7. Unemployment, time, and turnout: quarterly decomposition

	(1)	(2)
Current	0.018***	0.018***
	(0.00261)	(0.00441)
Current x (1-3mo prior)		-0.001
		(0.00538)
1-3mo prior	0.007**	0.007**
	(0.00234)	(0.00256)
4-6mo prior	0.002	0.002
	(0.00236)	(0.00236)
7-9mo prior	0.004	0.004
	(0.00231)	(0.00231)
10-12mo prior	0.003	0.003
	(0.00231)	(0.00231)
13-15mo prior	0.002	0.002
	(0.00216)	(0.00216)
16-18mo prior	0.003	0.003
	(0.00228)	(0.00228)
19-21mo prior	0.002	0.002
	(0.00229)	(0.00229)
22-23mo prior	-0.003	-0.003
	(0.00226)	(0.00226)
Adj. R-sq	0.503	0.503
N	2819637	2819637
n	977404	977404
SES controls	Yes	Yes
Municipality FEs	Yes	Yes
Election FEs	No	No
Muni x Elec FEs	Yes	Yes
Individual FEs	Yes	Yes

Standard errors clustered on individuals in parentheses. * p<0.05, ** p<0.01, *** p<0.001
Notes: Ages 25-62. Unemployment indicators (0-1). *Current* indicates unemployed in the election month. Experience decomposed to spells within quarterly intervals (3mo) relative to election month. The outcome variable is turnout in elections (0-1). SES controls include controls for age, income, education. Estimated coefficient (std. err.) of linear combination $\sum$(4-6mo - 22-23mo) is 0.014 (0.0030712), t=4.67, p=0.000

We see that only unemployment in the election month and within the three months leading up to the election significantly affect the propensity to vote, though a joint test of the other quarters indicates that unemployment further back does have some influence and would lead to an upward bias if omitted, as found in the main analysis. Moreover, we see that the estimated coefficients of the experience measures increase in magnitude at the three-month mark. Notable are still the distinctive effects of unemployment in the election month and experience within the immediate past: as the unemployment spell becomes concurrent with the election, the mobilizing effect increases by 1.1 percentage points relative to spells within the one to three preceding months.

To account for the presumably high collinearity between unemployment in the election month and the three foregoing months, I include an interaction term between the two measures in the model estimated in column (2). The interaction term is not significant. This indicates that the 1.1 percentage point jump in the effect of unemployment exactly as the spell becomes contemporaneous with the election also applies to individuals who are in an ongoing spell upon entering the election month. This immediate surge in voting propensity lends further support to my hypotheses that months in unemployment affect turnout differently depending on their relative timing and proximity to the election.

This extensive analysis of the temporal scope of effects underscores that exact, high-frequency labor market data is critical for identification. The transient nature of the estimated effects also provides justification for the dynamic decomposition of unemployment experience used in my analysis. This operationalization of unemployment experience has, to my knowledge, not been previously utilized in studies of unemployment scars in electoral participation. Instead, past experience has been captured by absorbing state measures that switch from 0 to 1 for all periods following the first observed transition into unemployment. This specification prevents the separation of the effect of the first observed spell from the effects of the individual's subsequent labor market experiences leading up to and including the election month. Researchers typically conduct separate event studies to assess the persistence of the scarring effect. With a conjecture that the turnout effect will change with time, it seems appropriate to introduce dynamics in the main model specification. In analyses of labor market transitions, which in nature are time-variant, dynamic modeling appears as an intuitive approach. The evidence of transient effects and an on-impact surge in voting propensity in response to unemployment in the election month indicate that the dynamic approach is an appropriate fit for the relationship. It deserves mention that the two modeling methodologies differ in the interpretation of the estimated turnout effects: an absorbing operationalization assumes that unemployment permanently alters participation, while a dynamic approach determines effects only for the period it is experienced (Chen et al., 2024).

6.1.4 The unemployed voter

The results presented in Tables 6 and 7 are consistent with the premise raised at the start of this discussion that past unemployment ceases to affect turnout because they are short-lived in workers' memory. Yet, this explanation would imply a fairly restricted memory span with respect to labor market transitions. Nor can we reject the explanation that unemployment

affects participation because it alters one's current circumstances so that voting becomes more attractive in the moment. The distinctive effect of unemployment on election day can point to several of the mobilizing causal channels discussed in the literature review.

Increased leisure time can free up time and resources for political engagement. This mechanism finds support in electoral survey data from Statistics Norway: about half of the surveyed abstainers in the 2019 election did not participate because they lacked information about the political parties, and 50 percent responded that they did not have the time to vote (Kleven & Bergseteren, 2022). Unemployment could provide individuals who otherwise would have stayed home an opportunity to navigate the political landscape by dedicating more time to follow the electoral campaign and engage in political search.

Unemployment can change individuals' immediate egocentric and sociotropic political concerns and raise the opportunity cost of not defending one's economic interests through voting (Downs, 1957; Lipset, 1960; Fiorina, 1976; Brody & Sniderman, 1977; Schlozman & Verba, 1979; Emmenegger et al., 2015). The unemployed may therefore engage in instrumental voting (Downs, 1957; Fiorina, 1978), blame attribution (Brody & Sniderman, 1977; Fiorina, 1978), and protest voting (Emmenegger et al., 2015) conditional on the current personal financial position and pertaining immediate political interests. Election survey responses from 2019 reveal that 48 percent of the surveyed voters made their party choice within a month of the election, and 8 % stated that they made up their minds on election day (Kleven & Bergseteren, 2022). Naturally, the participation decision and party choice are mutually dependent, and these figures indicate that immediate circumstantial factors could affect voting decisions for a considerable number of Norwegian voters. Moreover, 40 percent of non-participants stayed home because they were "happy with things as they are" (Kleven & Bergseteren, 2022). Unemployment could therefore mobilize through discontentment (Burden & Wichowsky, 2014). Terminated spells are likely less relevant to current interests and do not pose the same incentive for pocketbook voting. Instead, they could incite engagement in retrospective voting. Electoral punishment for past labor market difficulties (Fiorina, 1978) could explain why unemployment has a lasting effect on participation and why individuals with prolonged recent experience are mobilized relatively more.

6.2   Internal validity

Empirical research on unemployment presents numerous pitfalls for selection effects which challenge identification.

A potential pitfall is the discouraged worker effect. Labor market exits by discouraged workers may be recognized as re-employment by the data. This measurement error could lead to attenuation bias and an underestimation of the effect of unemployment (Wooldridge, 2012) because it could appear as though voting propensity does not vary with unemployment if discouraged workers continue to vote as they did when unemployed and still in the labor force. Notably, there could be a selection in the type of job seekers that are susceptible to discouragement in the face of labor market difficulties.

While inertial voting behavior is a viable explanation for the estimated temporal pattern and moderate magnitudes of turnout responses to unemployment, estimates could still reflect the strong correlation between unemployment experience and future unemployment risk (Arulampalam et al., 2000). Scholars have proposed that the effect of unemployment is largest for individuals' first transition while subsequent spells trigger smaller turnout responses (Rosenstone, 1982; Azzollini, 2023). Under the assumption that the response to the first spell is the true causal effect of unemployment, it could be a concern that coefficients are driven by individuals who have already experienced their first spell. Since these are more at risk of becoming unemployed also in the sample period, the coefficients could be diluted and fail to reflect the causal impact of an initial transition. While the research design addresses strong unemployment state dependence by controlling for all observed experience between election months, the validity of estimates could be impaired if unobserved past unemployment has direct carryover effects (Imai & Kim, 2019) on the response to new transitions into unemployment in the sample period for these pre-exposed "unemployment veterans". To verify this identifying assumption of the empirical strategy, further investigation is warranted.

It is not possible to estimate the impact of the first spell for individuals with unemployment history from before the first observed election since we cannot know whether they voted before this. I instead exploit the long timespan of the employment records, which extend beyond the first wave of electoral data, to investigate whether individuals who entered the observation period with experience responded less to a return to unemployment compared to individuals who made their initial transition within the sample period.

Table 8 shows the results of estimations of the model with interactions between the unemployment measures and the veteran group. The presence of unemployment veterans in the sample does not seem to introduce endogeneity, as indicated by the insignificant interaction terms in column (1). The only perceived difference is that unemployment scars in turnout are less persistent for the veteran group, whose propensity to vote is unaltered by unemployment spells two years in the past.

To account for the possibility that the veteran group could predominantly capture older individuals, for whom the probability of previous exposure to unemployment is higher, I include a three-way interaction term between the unemployment measures, the veteran indicator, and the 40-plus age group indicator in the model and report the results in column (2). This ensures an identification of the isolated influence of having had preliminary experience. Both interaction terms are insignificant. Accordingly, the identifying assumption behind the fixed effects model, that unobserved past treatment does not affect current outcomes (Imai & Kim, 2019), appears satisfied. This instills confidence in the causal interpretation of the results of my main analysis. Homogenous effects for the first transition into unemployment as for a subsequent spell also strengthen the generalizability of the estimated effects.

Table 8. Novel transition versus return to unemployment

	(1)	(2)
Current	0.022***	0.022***
	(0.00301)	(0.00301)
Current x veteran	-0.001	0.000
	(0.00450)	(0.00541)
Current x veteran x 40y+		-0.004
		(0.00662)
Recent experience	0.007***	0.007***
	(0.00189)	(0.00189)
Recent x veteran	0.001	0.000
	(0.00301)	(0.00361)
Recent x veteran x 40y+		0.003
		(0.00466)
Distant experience	0.006**	0.006**
	(0.00180)	(0.00180)
Distant x veteran	-0.006*	-0.004
	(0.00272)	(0.00323)
Distant x veteran x 40+y		-0.005
		(0.00402)
Adj. R-sq	0.503	0.503
N	2819637	2819637
n	977404	977404
SES controls	Yes	Yes
Municipality FEs	Yes	Yes
Election FEs	No	No
Muni x Elec FEs	Yes	Yes
Individual FEs	Yes	Yes

Standard errors clustered on individuals in parentheses. * p<0.05, ** p<0.01, *** p<0.001
Notes: Ages 25-62. Unemployment indicators (0-1). *Current* indicates unemployed in the election month. *Recent experience* is defined as spells within a year (12 months) of election; *distant experience* is defined as spells 1-2 years (13-23 months) before election. Veteran indicator (0-1) is an individual with unemployment history before the observation period. 40y+ is an age group indicator (0-1) for individuals aged 40 years and older. The outcome variable is turnout in elections (0-1). SES controls include controls for age, income, education.

Another concern for internal validity is the increasing availability of early voting arrangements in recent years since it challenges the ability to measure unemployment spells around the time of the participation decision. Early voting opens one month before the election in Mainland Norway (Election Act, 2002, §8-1), introducing a period of potential measurement error. However, according to data from past elections, early voting should not pose a major threat to validity. Although many choose to vote ahead of election day, few do so in August; most early votes are cast in early September and the week leading up to the election (Kleven et al., 2023). There is also an agewise selection into early voting, and most early voters are below or above the age restriction imposed on my sample (Kleven et al., 2023).

Digital electoral records were first implemented in Norway in 2013. Since the first wave of data only covered 15 municipalities, my analysis begins with the 2015 election. As of spring 2024, electoral data for three elections were available for my balanced panel. With time, this panel may be extended. Adding observations can strengthen inference by increasing the number of pre-and post-unemployment observations of turnout and improving the precision of the fixed effects controls (Wooldridge, 2012). Additionally, extensions of the panel permit an assessment of potential temporal bias in results and an examination of how the relationship evolves in the coming years. The next wave of data corresponding to the 2021 general election could provide for interesting results, as the 2019 Covid pandemic forced many into unemployment. However, the extraordinary context of the pandemic will likely have implications for the relationship that make inferences less generalizable to other settings. The effect of being unemployed could be different when you share the experience with many of your coworkers, and with society at large, as compared to the effect of unaccompanied job loss. Moreover, many of the registered unemployed were temporarily laid off and expected to return to the workplace when restrictions lifted, which could have different implications for the incentive to vote.

Since digital electoral records were gradually implemented, my estimation sample consists of residents in the 27 municipalities included in the 2015 wave of electoral data. These correspond to the largest municipalities at the time. Consequently, my analysis is conducted on an urban sample. The relationship between unemployment and turnout may differ for individuals in less urban areas.

6.3   External validity

These results may not hold beyond Norwegian borders. The contradiction between my results and previous Swedish and British findings using similar empirical strategies is puzzling. The inconsistency with evidence from Sweden is particularly surprising, as Sweden and Norway share institutional and cultural similarities that instill the expectation that estimates would point in the same direction. This motivates a discussion of potential sources of the discrepancy. In a replication analysis, I adopt the empirical strategy used in Österman and Brännlund (2023) and assess the relationship between unemployment and turnout as portrayed by their empirical model in Norwegian data. I focus the reproduction study to the Swedish results. In addition to comparable institutional contexts, my analysis resembles Österman and Brännlund (2023) in terms of model specifications and the application of administrative data. The evidence from the United Kingdom is in contrast based on less reliable yearly self-reported panel survey data from survey interviews spread over all months of wave years and in some cases were conducted the following calendar year. The inconsistency in data collection and the noise introduced by self-reports (Rosenman et al., 2011; Althubaiti, 2016) make these data difficult to replicate. The key dissimilarity with Österman and Brännlund (2023) is that my research design makes use of monthly labor market data, while their results hinge on annual records with data collected in November, two months after elections. The purpose of this replication is therefore to explore the extent to which yearly observations of unemployment can present a data limitation with important implications for the estimated causal effect of unemployment on election day.

I construct an unemployment measure by the definitions used in the original study and examine how it performs in analyses using Norwegian data. Since actual employment status in election months is unobserved, the researchers define unemployment on election day as having been registered as unemployed by November of an election year, coupled with having zero labor income in November of that year. Students are not classified as unemployed. I refer to the replicated measure as the Ö & B approximation.

Table 9 shows the regression results of the replication analysis. The models in columns (1)-(3) are estimations of the empirical model specified in equation (1) in the original paper. The control variables in this specification are described in the table notes.

The first model is estimated with the Ö & B approximation. Consequently, this is the replication of the Swedish results using Norwegian data. Model (2) is the model that the authors seek to estimate: here, the proxy Ö & B variable is replaced with individuals' actual

employment status in the election month corresponding to the *CURRENT* variable used in the analyses of this thesis. In model (3) I include each of the conditions that determine the proxy variable separately to see how they divert the estimated effect. Lastly, column (4) shows how the Ö & B approximation performs in the empirical model used in this analysis, corresponding to the model estimated in Table 3, column (3) of this thesis. The only specification difference that could be of significance is that model (4) imposes a stricter age restriction of 25-62 years, while models (1)-(3) are estimated on individuals aged 20-64.

Table 9. Reproduction of evidence from Sweden with Norwegian data

	(1)	(2)	(3)	(4)
Ö & B unemployment	0.013***			0.009***
	(0.00153)			(0.00169)
Unemployed election		0.028***	0.028***	
		(0.00187)	(0.00210)	
Unempl. Jan-Aug			0.011***	
			(0.00143)	
New spell, Oct-Nov			-0.004	
			(0.00403)	
No labor income, Nov			-0.019***	
			(0.00076)	
Adj. R-sq	0.506	0.506	0.506	0.503
N	3398219	3398219	3398219	2819637
n	1163315	1163316	1163317	977404
SES controls	Yes	Yes	Yes	Yes
Municipality FEs	Yes	Yes	Yes	Yes
Election FEs	No	No	No	No
Muni x Elec FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes

Standard errors clustered on individuals in parentheses. * p<0.05, ** p<0.01, *** p<0.001
Notes: Models (1)-(3) estimate equation (1) in Österman & Brännlund (2023). Ages 20-64. Controls include family control (household category). Age and education controls as defined in Section 3. Models include the same fixed effects as equation (6) presented in Section 4. Model (4) corresponds to the model estimated in Table 3, column (3) in Section 5. Unemployment indicators (0-1). *Ö & B unemployment* is defined as unemployed at any point between January and November; zero labor income in November; not a student. *Unemployed election* corresponds to the *Current* measure in equation (6). *Unemployed Jan-Aug* captures months in unemployment in the election year ahead of the election month. *New spell, Oct-Nov* captures post-election spells that start after the election. No labor income, Nov (0-1) indicates whether the individual had labor income in November. The outcome variable is turnout in elections (0-1).

I start by showing the estimated effect of unemployment in the replicated model in column (1). While the Norwegian data still suggest that unemployment on election day increases the propensity to vote, the estimated coefficient is 1.5 percentage points lower when compared to the estimate using individuals' actual employment status in the election month in column (2). This indicates that the proxy variable introduces disturbance with ramifications for the ability to estimate the causal effect, resulting in a bias toward zero. We see that the underestimation

is substantial when applied to the model in column (4), likely because of stronger participation inertia among individuals above the age of 25. For the sample of individuals used in my analyses unemployment is estimated to increase turnout by only 0.9 percentage points, against the estimated 2.5 percentage points reported in Table 3, column (3) of the Results section.

There are three potential sources for the underestimation using the Ö & B classification strategy. These are explored in column (3).

First, relying on yearly observations of unemployment can confound unemployment in election months with joblessness in other months. A lower estimate could reflect the scars from these pre-election spells, which diminish at a relatively high rate even within the election year according to my analyses. Second, conditioning on a total absence of earnings could drive down the estimate, given that income is positively correlated with turnout (Kleven, 2019). Third, the inability to exclude post-election spells in the two months between the election and the data collection month could bias results and reflect the negative correlation between unemployment risk and turnout, since these individuals on the brink of unemployment are predisposed to lower participation.

Comparing the inferred employment status to individuals' actual status per the monthly employment records, I find that the Ö & B approximation systematically misidentifies labor market status. As such, some individuals will be recognized as unemployed when they were employed, and vice versa. To give context to the potential scope of measurement error, I find that the Ö & B approximation fails to pick up on 22.2 % of the accounts of election month unemployment and simultaneously overestimates the extent of unemployment; while 1.9 % of the estimation sample experience unemployment in election months, the Ö & B proxy variable has an overall mean of 3.2 %. It appears that out of the inferred unemployed, 36 % were in employment in the election month. With such approximations, estimates are bound to be erroneous. For example, wrongfully measuring an unemployed individual as employed could lead to attenuation bias. If the individual, incentivized by unemployment, votes this year, it would give the impression of higher participation as the individual counterfactual outcome. If the same individual is recognized as unemployed when employed in other elections, one would estimate the opposite relationship if they are less likely to vote in the absence of the mobilizing factor of unemployment. The relationship between unemployment and participation may therefore appear weaker or even inverse as a result of these measurement errors (Wooldridge, 2012).

In column (3), I dissect the components of the Ö & B approximation measure to see what could drive the lower estimate in column (1). I separate out the indicator for zero labor income in November and exploit the monthly data to see how turnout varies with unemployment in election years, occurring in the eight pre-election months, the election month, and the two post-election months respectively.

I identify two viable sources for downward bias in the methodological framework. First, the estimate seems to capture the lower effect of pre-election unemployment experience. In the data, there are more than twice as many occurrences of unemployment in these eight months compared to the one election month. As a result, these will also be given more weight in estimation. Conditioning on zero labor income could also induce downward bias: the results presented in column (3) suggest that income deprivation in November decreases the propensity of turning out.

The estimated coefficient for actual unemployment in the election month remains robust, and post-election unemployment is not found to significantly affect turnout. This last observation is reassuring for the validity of the results using my research design.

This replication analysis highlights the key finding of this thesis that the effect of unemployment depends critically on the exact timing of spells relative to the election. My review indicates that yearly data is insufficient for identification and could be a potential source for underestimation. Monthly data is more suitable for an adequate assessment of the relationship since it allows for precise measurement of unemployment around elections and a proper account of this time dimension without leaning on potentially flawed approximation measures.

Even so, I find that the positive association between individual unemployment and turnout in my data remains with the Ö & B approximation measure. The effect of unemployment could therefore be geographically or geo-temporally dependent, reflecting institutional differences. Despite similarities between the Norwegian and Swedish contexts, Sweden has historically higher unemployment rates (Statistics Sweden, n.d.) and a voluntary, union-funded unemployment insurance system based on the Ghent model (Kjellberg & Lyhne Ibsen, 2016) that has become less generous in recent years (Ferrarini et al., 2012; Kjellberg & Lyhne Ibsen, 2016), which could make the relationship between electoral participation and unemployment fundamentally different. The institutional differences between Norway and the United Kingdom are even larger and could explain the discrepancy in findings; the two countries also practice different voting systems that pose different incentives for participation

(Mueller, 2003). These considerations suggest that the Norwegian results could be less transferable to other contexts and that one should approach cross-country comparisons with caution (Marx & Picot, 2020).

7   Conclusion

In this thesis, I have examined how individuals' electoral participation responds to personal unemployment using administrative micro-level panel data on Norwegian individuals. With rigorous controls of observable and unobservable heterogeneity down to the individual level, I show that being unemployed on election day increases the propensity that an individual votes by 2.1 percentage points. The estimated mobilizing effects of unemployment shed new light on the gap in voter turnout between the unemployed and the employed that arises in aggregated data, and highlight that proper accounts of unobservable individual idiosyncrasies are essential to look beyond mere correlations and descriptive facts.

While previous literature has been less rigid in distinguishing the role of unemployment on election day and past unemployment experience for the causal chain, I exploit high-frequency labor market data to precisely place unemployment spells relative to elections and thoroughly analyze the dynamic relationship. I show that unemployment spells mobilize individuals to vote for up to two years after they were experienced, and that failing to account for experience results in an upward bias of the estimated effects of unemployment around the election. Still, I show that it is predominantly unemployment in the election month and experience in the near past makes individuals more likely to turn out. While experience with prolonged unemployment incentivizes voting relatively more than brief periods out of work, it is predominantly the freshness of the unemployment experience on election day that determines the turnout response.

These findings lend support to instrumental voting (Downs, 1957; Fiorina, 1978) and retrospective voting (Fiorina, 1978) conditional on individual economic position and interests as driving forces of participation in elections, and the evidence of on-impact effects is less in in line with the proposition that the unemployed are mobilized through awakened political interest (Lipset, 1960). Relatively moderate estimates and effects that diminish with time demonstrate that individuals do not deviate greatly from their voting habits in response to unemployment. The decreasing scarring effect suggests that a transition into unemployment does not hold the potential to activate a permanent habit of voting. Participation behavior that

is resistant to labor market fluctuations can indicate that unemployment constitutes less of a shock than the theory predicts, or mainly affects individuals in ways that are irrelevant to the participation decision. It could be that the mechanisms that induce negative effects in other institutional contexts are weaker in Norway. Relatively high income compensation rates in the unemployment insurance system (OECD, 2024) could for example soften the response.

My analysis contributes to the literature on the effect of unemployment on electoral participation in several aspects. First, I expand on knowledge by analyzing the relationship in a new empirical setting. The finding of a mobilizing effect of unemployment contrasts existing evidence using similar empirical strategies. This suggests that the relationship is highly contextually dependent and signals a need for continued research and extension of the analysis to new empirical settings. A second contribution is the application of precise labor market status data, which enables the clear distinction between concurrent employment situation and unemployment experience needed for an adequate analysis of the time dimension of the relationship. My analysis demonstrates that unemployment predominantly has an on-impact switching effect, as individuals are most likely to deviate from their default participation outcome if they are unemployed when the election comes around. Replication of prior results from Sweden with Norwegian data suggests that monthly labor market data present a methodological advantage over data collected annually as it mitigates measurement error in the unemployment variable and alleviates underestimation with approximation measures.

These results are relevant to comprehending the gap in electoral participation between the unemployed and the employed in the population, as they suggest that differential turnout levels are entirely driven by underlying systematic differences. My results imply that active labor market policies are insufficient to promote the democratic integration of the unemployed, as re-employment will not incite the democratic involvement needed to close the participation gap. Policy should instead identify and address the underlying factors that cause abstention in demographic groups vulnerable to unemployment to foster an inclusive democracy and reduce electoral apathy among these individuals. This study is, however, not informative for what these factors are.

Moreover, these findings generate insights that can be used to predict aggregated political consequences of unemployment induced by economic downturns and structural labor market transformations in an evolving world. My results indicate that even short experience

with joblessness such as the common frictional unemployment experienced between jobs has the potential to stimulate individual turnout with immediate effect. In turn, this can affect democratic representation by altering the composition of electors. Yet, the modest effect sizes reported in this thesis suggest that labor market shifts have limited and short-lasting effects on individual and aggregated electoral outcomes. Unemployment is unlikely to sway elections in proportional representation voting systems like that of Norway, but the political consequences could be noticeable in close electoral races and plurality voting systems where percentage points could separate electoral winners and losers. Still, the relationship between unemployment and electoral participation could look different in these institutional contexts, and the results of a micro-level analysis may not immediately aggregable to the macroeconomic level, especially if the individual response to unemployment depends on macroeconomic circumstances as suggested in previous research (Azzollini, 2021; Österman & Lindgren, 2021).

One limitation of this study is the limited observations of turnout per individual. Though the model is identified for any dataset with at least two waves of data, more observations of individual participation are advisable to establish a good measure of individuals' default voting habits. Extending this panel with electoral data from subsequent elections can improve upon identification and reduce potential temporal dependence.

This thesis sought to establish a causal link between unemployment and electoral participation. The unexpected finding that unemployment increases the propensity to vote in Norway raises new research questions into why the unemployed are mobilized to the polls, especially seen in context with opposing evidence from other countries. Since only the final participation outcome is observed in the data, it is out of the scope of this analysis to detect precise causal mechanisms behind the turnout response. Future research could investigate causal channels closely and generate more insight with qualitative approaches or mediation analyses. Qualitative data could supplement the analysis with accounts for the subjective experience of unemployment and economic expectations for the future as recommended by Marx and Picot (2020) since these factors are likely to impact individual turnout.

Moreover, my analysis sheds light on how unemployment affects voting behavior in terms of turnout. Joblessness may affect voting and political behavior in ways that are not recognized in the observed turnout choice. Individuals may for example respond to unemployment by changing party loyalty or allocate more or less time and energy to other forms of political engagement like demonstrations or direct contact with party representatives.

Without data on individual vote choice and political activity outside of the polls, my analysis can only determine the extent to which unemployment changes the outcome of the participation decision. As such, these estimates may provide a lower bound to the implications of unemployment on electoral engagement. An extension of the analysis to other political outcomes such as party choice is an interesting topic for further investigation, though challenging to achieve with administrative data in secret ballot systems. These are suggestions for future empirical investigations that could dive further beneath the immediately observable surface to enhance our knowledge of who votes in elections, and why.

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Appendix

A.1 Data availability

The raw data used in this analysis are from Norwegian administrative records that are subject to legal regulations of controlled access and thus restricted from distribution. Access is granted upon application to Statistics Norway.

A.2 Robustness checks

Table A.1 Unemployment duration (continuous, cumulative months) and turnout

	(1)	(2)
Current	0.0004	0.0192***
	(0.00245)	(0.00240)
Recent exp., months	-0.0071***	0.0025***
	(0.00047)	(0.00046)
Distant exp., months	-0.0097***	0.0012**
	(0.00043)	(0.00043)
Total exp. months, sq	0.0003***	-0.0001**
	(0.00002)	(0.00002)
Adj. R-sq	0.154	0.503
N	2819637	2819637
n	977404	977404
SES controls	Yes	Yes
Municipality FEs	Yes	Yes
Election FEs	Yes	No
Muni x Elec FEs	Yes	Yes
Individual FEs	No	Yes

Standard errors clustered on individuals in parentheses. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Notes: Ages 25-62. *Current* indicates unemployed in the election month. *Recent experience* is defined as cumulative months in unemployment within a year (12 months) of election; *distant experience* is defined as cumulative months in unemployment 1-2 years (13-23 months) before election. The square term is the square of the total cumulative months in unemployment in the 2 years (23 months) before election. The outcome variable is turnout in elections (0-1). SES controls include controls for age, income, education. Gender and immigration background control for model (1). Model (1) is pooled OLS; model (2) individual FE estimation.

Table A.2 Unemployment and turnout. Keeping singletons.

	(1)	(2)
Current	-0.006*	0.021***
	(0.00230)	(0.00224)
Recent experience	-0.017***	0.007***
	(0.00150)	(0.00147)
Distant experience	-0.032***	0.003*
	(0.00135)	(0.00135)
Adj. R-sq	0.154	0.508
N	2921894	2921894
n	1079660	1079660
SES controls	Yes	Yes
Municipality FEs	Yes	Yes
Election FEs	Yes	No
Muni x Elec FEs	Yes	Yes
Individual FEs	No	Yes

Standard errors clustered on individuals in parentheses * p<0.05, ** p<0.01, *** p<0.001
Notes: Ages 25-62. Singletons included. Unemployment indicators (0-1). *Current* indicates unemployed in the election month. *Experience* indicates unemployment spells in the 2 years (23 months) since the previous election. *Recent experience* is defined as spells within a year (12 months) of election; *distant experience* is defined as spells 1-2 years (13-23 months) before election. The outcome variable is turnout in elections (0-1). SES controls include controls for age, income, education. Gender and immigration background control for model (1). Model (1) is pooled OLS; model (2) individual FE estimation.

Table A.4 Unemployment and turnout. Ages 20-65

	(1)	(2)
Current	-0.003	0.022***
	(0.00211)	(0.00209)
Recent experience	-0.021***	0.007***
	(0.00135)	(0.00134)
Distant experience	-0.039***	0.002
	(0.00123)	(0.00125)
Adj. R-sq	0.161	0.495
N	3195171	3195171
n	1065057	1065057
SES controls	Yes	Yes
Municipality FEs	Yes	Yes
Election FEs	Yes	No
Muni x Elec FEs	Yes	Yes
Individual FEs	No	Yes

Standard errors clustered on individuals in parentheses * p<0.05, ** p<0.01, *** p<0.001
Notes: Ages 20-62. Unemployment indicators (0-1). *Current* indicates unemployed in the election month. *Experience* indicates unemployment spells in the 2 years (23 months) since the previous election. *Recent experience* is defined as spells within a year (12 months) of election; *distant experience* is defined as spells 1-2 years (13-23 months) before election. The outcome variable is turnout in elections (0-1). SES controls include controls for age, income, education. Gender and immigration background control for model (1). Model (1) is pooled OLS; model (2) individual FE estimation.