Mobilizing Young Voters with Short Text Messages in Nationwide Field Experiments: Persistence and Partisan Effects

Salomo Hirvonen, Maarit Lassander, Lauri Sääksvuori, Janne Tukiainen*

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Abstract

We examine how randomized get-out-the-vote (GOTV) appeals affect inequalities in voting and voters' turnout decisions transmit in social networks in high salience elections. Moreover, we study the persistence of GOTV mobilization effects across successive elections using a linked dataset covering electronic voting records and randomized treatment assignments across two consecutive elections. We find that receiving a text message reminder before the Finnish 2023 parliamentary elections mainly mobilized young voters with low predicted probability to vote, implying that our intervention reduced existing social inequalities in voting within the target group of young voters. We find that the previously documented remarkably large within household spillovers in voting largely generalize from low salience elections to high salience elections and document that over 100 percent of the direct treatment effect spilled over to untreated household members. We do not find evidence for the persistence of the direct treatment received in the previous elections nor of interaction effects between the two experiments. However, we document a persistence of a within household spillover effect from the first experiment for sub-samples having high original spillover treatment effect. Lastly, we do not find evidence for partisan effects by predicted voting for right-wing or populist party.

Keywords: Field experiments, Dynamic effects, Get-out-the-vote, Inequality in voting, Nudging, Spillover effects, Voter turnout

*Hirvonen: Department of Economics, University of Turku, Rehtorinpellonkatu 3, FI-20014 Turku, Finland. Lassander: Prime Minister's Office, Snellmaninkatu 1, FI-00023 Helsinki, Finland. Sääksvuori: Finnish Institute for Health and Welfare. Mannerheimintie 166, FI-00271, Helsinki, Finland. Tukiainen (corresponding author): Department of Economics, University of Turku, Rehtorinpellonkatu 3, FI-20014 Turku, Finland, janne.tukiainen@utu.fi, +358503083620. The experiment was conducted in collaboration with the Ministry of Justice (Finland), the Prime Minister's Office (Finland) and SITRA. We thank them for funding and support. This RCT was registered in the American Economic Association Registry for randomized control trials (AEARCTR-0011105). The Ethics Committee for Human Sciences at the University of Turku approved this study (8/2023).

1 Introduction

The experimental revolution in political science has led to substantial advancements in the study of voter mobilization and political participation. As a result, a large number of experimental studies suggests that voter mobilization through get-out-the-vote (GOTV) interventions successfully raises voter turnout (Green and Gerber, 2019). However, there is less consensus about the effects of GOTV mobilization on the composition of the electorate. While an assessment of the findings from US-based experiments suggests that GOTV interventions may increase demographic inequalities in political participation (Enos et al., 2014), a more recent stream of European research supports the interpretation that GOTV mobilization may mainly mobilize low-propensity voters and decrease demographic gaps in political participation (Bhatti et al., 2018; Bergh et al., 2020; Bergh and Christensen, 2024; Hirvonen et al., 2024).

This paper evaluates how GOTV appeals affect inequalities in voting and transmission of voting behavior in social networks in high salience multi-party elections. For this purpose, we conduct a large RCT in highly contested Finnish parliamentary elections. Taken together, our study builds on a series of two large RCTs that are conducted in two consecutive elections allowing us to examine the effects of GOTV mobilization using the same electoral rules and the same target population, young adults aged between 18 and 29 years, but with marked differences in the salience of the elections. In this paper, we mainly focus on evidence from the later-held high salience elections and contrast this information with the evidence from the earlier-held low salience elections. Moreover, a linked dataset covering the electronic voting records across multiple elections enables us to study both the persistence of GOTV mobilization efforts across the elections and the dynamic effects of receiving non-partisan text message reminders in two successive elections.

First, using electronic voter turnout records and rich individual-level administrative data on eligible voters, we estimate the effect of a state-led non-partisan text message-based GOTV campaign in social inequalities on voting in high salience elections and contrast these results with the previously observed findings from low salience elections. Second, using unique household IDs, we investigate how election salience modifies the transmission of voting decisions in social networks. Third, using a linked dataset that covers the electronic voter turnout records and randomized treatment assignments in two consecutive elections, we investigate the potential persistence of GOTV intervention effects through multiple electoral cycles both for direct and spill over effects. Finally, we utilize a general population election survey data to build a prediction model for the likelihood of being a right-wing and populist party voter in order to estimate possible partisan treatment effects.

Our results show that the previously observed inequality decreasing compositional effects of GOTV interventions and remarkably large spillovers largely generalize from low salience elections to high salience elections. First, we find that receiving a text message reminder before the Finnish parliamentary elections in 2023 mobilized mainly low-propensity voters and reduced existing social inequalities in voting. Second, we document remarkably large spillover effects in elections with high turnout rate, suggesting that the previously observed magnitudes of spillover effects may generalize to high salience elections and are not just a curiosity related to low salience elections. Third, we do not find evidence that being assigned to a GOTV mobilization treatment in previous elections would affect turnout in the following elections for the youth eligble voters. However, we document a persistence of a within household spillover effect from the first experiment for samples of household members having high school degree and voted in 2021 elections, those being predictors of high treatment effects in the first experiment. Fourth, we do not find evidence that receiving a reminder message in previous elections would either reduce or magnify the effectiveness of text-message based GOTV mobilization in later-held elections. Finally, we do not find evidence for different treatment effects by predicted probability of being a right-wing party or populist party voter. If anything, the point estimate for the second experiment for likely populist party supporters is close to zero.

This paper builds on and relates to several strands of literature. First, our study contributes to the literature on demographic gaps in political participation. There are several largely unresolved questions about the compositional effects of GOTV mobilization as the US-based literature suggest that mobilization strategies may widen existing social disparities in voting (Enos et al., 2014), while the European literature suggest the opposite (Bhatti et al., 2018; Bergh et al., 2020; Hirvonen et al., 2024; Bergh and Christensen, 2024). Our paper complements the existing literature on the compositional effects of GOTV mobilization strategies and solidifies the evidence that GOTV mobilization mainly mobilizes low-propensity voters and their household members with low predicted probability to vote. Through combing evidence from low and high salience elections, we document that the mobilization of low-propensity voters is, in the context of Finnish nationwide elections, independent of the electoral salience.

Second, our paper relates to the increasing number of experimental studies on voter mobilization with an explicit objective to measure how voting decisions transmit in social networks. Prior to this paper, Nickerson (2008); Sinclair et al. (2012); Bhatti et al. (2017); Hirvonen et al. (2024) has investigated how voting decisions transmit after being exposed to different get-out-the-vote appeals. This paper solidifies the evidence that GOTV interventions lead to substantial spillovers that can even exceed the magnitude of the direct effect and reduce inequalities in participation in among population groups who do not belong to the target population. Building on our previous work (Hirvonen et al., 2024), we document that the relative magnitude of spillover effects is independent of the electoral salience. Likewise, we observe that, in the context of high salience elections, the spillover effects mainly occur within low propensity and marginal voter households, reducing the inequality in turnout among the untreated households.

Third, and more generally, our paper contributes to the literature about the persistence and long-term effectiveness of nudge interventions across policy areas and topics (Brandon et al., 2017; Robitaille et al., 2021; DellaVigna and Linos, 2022; Byrne et al., 2023). Our paper complements this emerging literature and by finding that the direct effects of GOTV mobilization interventions are unlikely to persist in the following elections. Simultaneously, our findings suggest that that experiencing the same nudge several times in a similar situation is unlikely to either magnify or reduce its effectiveness. However, given our setting and data we contribute to the literature by being are able to detect the persistence of within household spillover effects. This furthermore underscores the importance of accounting for spillovers when assessing overall impacts of a policy intervention.

Lastly, this paper contributes to the understanding of possible partisan effects from GOTV interventions. Previous studies in the context of two-party system from the US have found GOTV experiments having differential treatment effects by whether individual was a registered voter for the Democratic or the Republican party (Green et al., 2013). For example Gerber and Green (2000) finds that non-partisan GOTV appeal increases turnout only among voters being unaffiliated to the two major parties, but not for registered Democrats or Republicans. However, it is unclear how these results would translate outside of the majoritarian two-party setting of the US. By predicting party preferences for our sample individuals with external survey data, we do not find evidence for heterogeneous treatment effects by predicted partisan support.

The paper proceeds as follows. Section 2 describes the relevant electoral system and how the electoral context varies between the two experiments. In section 3, we describe our data, experimental design and the sample. Section 4 presents our empirical methods. Section 5 presents the results. Section 6 concludes.

2 Background and Context

The experiment conducted for the purpose of this paper complements the experiment conducted in the Finnish nationwide county elections held on January 23, 2022, and summarized in a paper by Hirvonen et al. (2024). The second experiment in this series of two RCTs and the main focus of this paper was conducted in the Finnish Parliamentary elections held in April 2023. Given the connection between these papers, some of the material follows fairly closely Hirvonen et al. (2024).

The allocation of seats in both elections, like in all Finnish nationwide elections, was proportional to the votes following d'Hondt system of open party list proportional representation (PR). Notably, Finland uses a very pure form of open-lists in the sense that personal vote is obligatory: each voter gives exactly one vote to one candidate. Parties are assigned seats based on the sum of its candidates' personal votes and the seats within the party are assigned purely based on the personal votes. Moreover, candidates are presented in alphabetical order in the ballot lists limiting parties ability to signal their preferences over the candidates. Overall, the open list electoral system in Finland is highly personalised, which may increase incentives for individual campaigning compared to several democracies with closed list PR or mixed electoral systems (von Schoultz and Strandberg, 2024).

Voters are automatically registered in all elections in Finland. An electronic register of all eligible voters (voting register) is established based on the Population Information System on the 46th day before the election day (Jääskeläinen, 2020). All voters listed in the voting register receive a notice of their right to vote (polling card) no later than 24 days before the election day. The polling card indicates the date of the election, the period for advance voting, the locations of advance polling stations within the voter's electoral district, the address of the voter's election day polling station, and contact information of the electoral authorities. The polling stations have only an administrative role as the elections are held at-large in the whole county. A typical characteristic of the Finnish elections is that a relatively large share of voters cast their ballots at polling stations during the period for advance voting that begins 11 days before the election day and ends five days before the actual election day. In the 2023 parliamentary elections, 40.5% of eligible votes used the advance period to cast their vote. Overall turnout was 71.9%.

Prior to our studies, text message-based mobilization experiments have been conducted in the US, Denmark and Norway. The Finnish electoral system and voter mobilization environment closely resembles the other Nordic countries. There are notable demographic inequalities in voting. Young adults aged from 18 to 29 years are markedly less likely to vote than the older age cohorts. The previous parliamentary elections were held in year 2019. The age gap between the young voters (aged from 18 to 29) and older voters (over 29 years of age) in these these elections was around 11 percentage points, and the gender gap within the group of young voters was around 10 percentage points.

3 Experimental Design and Data

3.1 Sample

To carry out the experiment, we utilized the Finnish Digital and Population Data Services Agency register, which contains information on eligible voters such as their names, personal identity code, electoral district, and municipality of residence from the Population Information System. The electronic voting register records the individual-level turnout and allows us to link this to the above data together with treatment status, also from our previous experiment (Hirvonen et al., 2024). Our target sample is restricted to municipalities, where the electronic voting register is available in voting districts covering at least 80% of eligible voting population in the municipality. After this restriction we are left with 128 municipalities. As it can be seen from Table 1 around 43% of eligible voters who are younger than 31 years-old live in these municipalities.

Following the retrieval of relevant information for all eligible voters aged between 18 and 30 years-old and residing in our sample municipalities in voting districts covered by the electronic voting registry, we tasked an IT-company to find a cell phone number for these individuals. The company was able to find a phone number for 16.4% of individuals in the above mentioned target group. This led to a total sample of 49864 individuals aged from 18 to 30 years of age.

Table 1 compares the descriptive statistics of the analysis sample to various populations. Column (1) shows covariates for the analysis sample that was used to randomly allocate individuals into a treatment and a control group. Column (2) is otherwise same as Column (1), but it does not include 18 year-old individuals in order to make the sample comparable for Columns (3) and (4), where we are not able to identify individuals who were 18 and eligible to vote in the 2023 parliamentary elections as our data only includes the birth year but not the date of birth. Column (3) contains all the 19-30-year-old individuals living in the municipalities with the electronic voting registry coverage and Column (4) shows descriptive statistics for all eligible voters in the same age bracket. Comparing columns (3) and (4), we can observe that analysis sample municipalities seem to be representative of the population of all municipalities in Finland. Moreover, when comparing Columns (2) and (3), it can be seen that loss of individuals due to not finding phone numbers does not make the analysis sample observed characteristics vastly differ from all same-aged individuals living in our sample municipalities. We only find some differences in terms of income and share of females, where the analysis sample individuals have slightly higher income and have 8%-points less females.

Table 1: Summary statistics: Sample compared to population

	Analysis sample Full Sample (1)	Analysis sample Aged 19 to 30 (2)	Analysis Municipalities Aged 19 to 30 (3)	Full population Aged 19 to 30 (4)
Female	0.40	0.40	0.48	0.49
	(0.49)	(0.49)	(0.50)	(0.50)
Age	25.16	25.26	24.57	24.63
	(3.49)	(3.41)	(3.46)	(3.49)
High School Degree	0.45	0.45	0.46	0.48
	(0.50)	(0.50)	(0.50)	(0.50)
Taxable Income	19980.73	20161.30	17366.25	17725.93
	(14970.78)	(14935.75)	(14411.31)	(14821.19)
Immigrant	0.02	0.02	0.03	0.04
	(0.15)	(0.15)	(0.18)	(0.20)
Observations	49.864	49.090	304.536	710.516

Notes: Standard deviations in parentheses. Covariates are measured in year 2021 with the exception of age which is for year 2023. Number of observations for taxable income are 47.408 (Column 1), 46.932 (Column 2), 288.613 (Column 3) and 670.596 (Column 4).

3.2 Experimental design

The experiment was conducted in collaboration with the Ministry of Justice (Finland) and the Prime Minister's Office (Finland), which also funded the experiment. The study was approved by Ethics Committee for Human Sciences at the University of Turku, Finland (decision number: 8/2023). The trial was pre-registred with objectives of our RCT and a study protocol detailed in the American Economic Association Registry for randomized controlled trials with an RCT id AEARCTR-0011105.

To estimate both the direct causal effect and potential spillover effects of SMS reminders on turnout, we randomized all individuals in our analysis sample into a control and a treatment group. We allocated 40 percent of individuals into a control group and 60 percent of individuals into a treatment groups (Figure 1). This departs from a traditional 50/50-split as we wanted to also study so called dynamic effects exploiting data from our previous experiment from 2022 County elections, where we used 60/40 overall split with three different treatment-arms. Retaining the same assignment ratio allows us to increase the statistical power in order to detect possible dynamic effects. We implemented stratified randomization at the municipality level to ensure that 60% of all eligible voters aged 18 to 30 received a reminder in each municipality. The stratification aims to increase the precision of estimated treatment effects (Duflo et al., 2007).

Aligned with the timing of polling opportunities in Finnish elections, we sent two text messages to individuals belonging to the treatment group. The first message was sent a day prior to the commencement of the advance voting period, followed by a second message sent a day before the election day. The timing of these messages remained constant, with all messages sent simultaneously at 4 pm through a mass text messaging service. In our previous experiment conducted during 2022 County elections (Hirvonen et al., 2024), we found a neutral formulation of the message being the most effective one. Thus, in order to maximize effectiveness in this experiment we used only a neutral message type. Following the previous experiment the message content was developed by the authors in collaboration with the electoral authority (Ministry of Justice, Finland) ensuring alignment with the prevailing electoral code of conduct. Each message contained a hyperlink directing recipients to the official electoral authority homepage, www.vaalit.fi [www.elections.fi], which offers reliable and unbiased information on organization of elections in Finland. Acting as the sender of the messages, the electoral authority likely bolstered message credibility, distinguishing them from typical promotional messages individuals receive on their phones. Messages were sent in Finnish and Swedish both being the official languages of Finland. Table 2 shows English translations of the messages.

Table 2: Message content

Group	Message #	Message text
Treatment	#1	”Hi, a reminder for you that the parliamentary elections are held on the 2nd of April. The domestic advance voting period is from 22nd of March until 28th of March. Read more vaalit.fi. Best Regards, the Ministry of Justice”
Treatment	#2	”Hi, a reminder for you that the parliamentary elections are held on the 2nd of April. Read more vaalit.fi. Best Regards, the Ministry of Justice”
Control	-	[None]

We assess the effect of SMS reminders on voter turnout by utilizing individual-level voting data sourced from the electronic voting register. This register contains a unique identifier for individual and includes a variable indicating whether they cast their vote in each respective elections. Employing unique personal identifiers we merge treatment status of individuals to personal-level turnout data from all nationwide elections since 2015, and to a rich administrative based individual level socio-economic data and household IDs. Importantly, for the analysis of treatment heterogeneity this administrative socio-economic data contains for example information on individual’s taxable income by different sources (including social transfers), employment histories, education and immigration background. In addition to that we are able to merge the treatment status also from the experiment conducted in 2022 in order to study persistence and dynamic effects. The resulting unique dataset is protected from improper disclosure and its access is restricted to Statistics Finland’s remote access system, precluding sharing of the data. However, replication of findings is possible by using the code from us and purchasing of the specified datasets alongside with access to Statistics Finland’s remote access system.

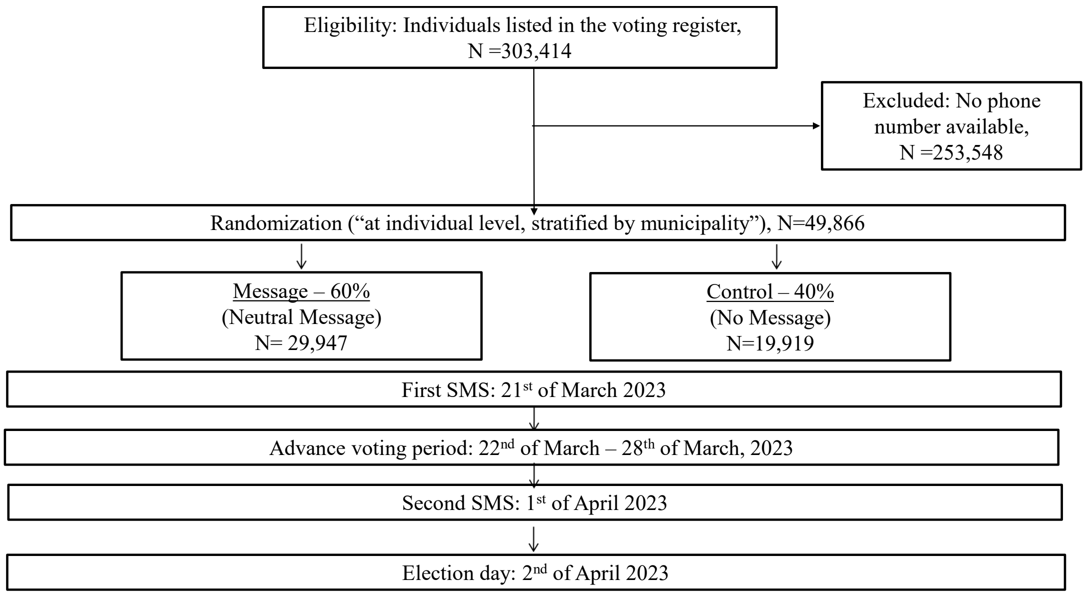

Figure 1: Eligibility, Randomization and Treatment.

4 Estimation methods

Given the randomization procedure, and access to electronic voting register and administrative data containing unique personal and household IDs, we estimate direct effects, spillover effects and treatment effect heterogeneity.

4.1 Direct effects

To assess the direct effect of SMS reminders on vote mobilization, we estimate the average treatment effect of receiving a SMS reminder in contrast to the counterfactual of receiving no reminder. Following the pre-registeration, we estimate the direct treatment effect with a linear probability model and progressively add control variables to the model:

$$Y_i = \beta_0 + \beta_1 Treatment_i + \boldsymbol{X'}_i \boldsymbol{\beta} + \epsilon_i,$$

where $Treatment_i$ is an indicator for treatment assignment and $X_i' \boldsymbol{\beta}$ includes individual level demographic controls. Our demographic controls are educational background, which is defined as individual's mother having a high school degree or using individual's own high school degree status if we are not able to identify the mother of the individual (47.4% of our sample) based on the household data going back to year 2011.

In addition to the education, we use logarithm of individual's mother's taxable income and single-digit occupation code as controls for the socio-economic background. Given that the sample consists of young voters, we believe that mothers' characteristics are more relevant in describing individuals circumstances and predictive of voting than their own characteristics. In addition to socio-economic characteristics, we include individuals' immigration background, defined as person's both parents born outside of Finland. We use age, gender and an indicator variable documenting if the individual was eligible to vote in the 2023 elections for the first time, as additional controls. Using control variables in the estimations of average treatment effects in a randomized experiment is not expected to have an effect on the point estimates, but it can reduce residual variance increasing the precision of the estimates. We use clustered standard errors at the municipal level.[1]

4.2 Spillover effects

Our data includes unique household IDs, which enables us to estimate spillover effects of our voter mobilization intervention within the households.[2] To investigate treatment spillovers within households following an SMS reminder, we narrow our focus to households containing either precisely one young voter from the treatment group or exactly one young voter from the control group. This restriction results in a sample of 52.3% of the total sample, reflecting a significant proportion of individuals living independently. Consequently, households with more than one potentially treated young voter are excluded from the spillover estimation sample. Thus, for spillover effect analysis, the treatment group comprises all individuals residing within the same household as of the end of the year 2022 (the most recent data available to us) with a member who received an SMS reminder. The control group encompasses individuals cohabiting with a young voter assigned to the control group. On average, these households contain 1.52 eligible voters in addition to the SMS recipient or control group member.

4.3 Effect heterogeneity analysis

Estimating both direct and spillover effects allows us to evaluate the impact of SMS reminders as a voter mobilization tool. However, these effects might vary across different segments of the electorate,

[1]From a design-based perspective, clustering may not be necessary as our randomized treatment is assigned at the individual level (Abadie et al., 2022). However, as we observe only a subset of Finnish municipalities clustering accounts for municipality-level sampling variance, and therefore is used to generalize our results to the whole population of young voters.

[2]Given the number of treated individuals living in the same household with control group individuals is small (around 5% of the sample) even very large potential spillovers of over 100% would not affect our direct effect estimates at any relevant decimal level. Therefore, we do not study potential spillovers from treatment group individuals to control group individuals, but we analyse the intra-household spillovers from our target sample (voters aged 18 to 30 years) to eligible voters residing in the same household.

potentially increasing or decreasing current inequalities in turnout. Drawing on the studies by Arceneaux and Nickerson (2009) and Enos et al. (2014), we explore how text message mobilization influences the composition of the electorate. Our pre-registered estimation method includes several steps, beginning with fitting a following logistic regression model in order to predict a propensity to vote for every individual using the available administrative data:

$$Pr(Y_i = 1|\boldsymbol{X}_i) = \frac{exp(\boldsymbol{X}_b)}{1 + exp(\boldsymbol{X}_b)},$$

where $Pr(Y_i = 1|\boldsymbol{X}_i)$ is the predicted probability of voting in 2023 parliamentary elections with predictors being individuals' gender, age, logarithm of (mother's) taxable income, ethnicity, education, SES background, eligibility to vote for the first time and municipality fixed effects.

To estimate individual voting probabilities without intervention, we restrict our predicted voting estimation to the control group members. The random allocation into treatment and control groups ensures that the propensity estimates calculated for the control group are representative of those in the treatment group. Therefore, we predict the probability that each person in the sample would vote in the 2023 Finnish parliamentary elections without the influence of the receiving SMS voting reminders. Next, we group the individual predicted voting propensities by 25th, 25-75th, and top 25th percentiles. This categorization helps identify potential non-linear effects associated with varying levels of voting propensity (Arceneaux and Nickerson, 2009; Fowler, 2015). Dividing the sample into three groups offers more flexibility than imposing a functional form of voting propensity into an OLS model and maintains more statistical power for comparing groups than would be possible with more finer groupings. Finally, we assess the impact of the SMS reminders on these groups using a linear probability model to determine if the intervention disproportionately affects voters based on their initial likelihood to vote, examining interactions with existing disparities among high-propensity voters, marginal voters, and low-propensity voters.

We recognize the risk of overfitting the data in estimating voting propensities via logistic regression, by itting random variation and using outlier observations in demographic variables that could lead to biased comparison of treatment heterogeneities different voting groups. To mitigate this, we supplement our initial analysis by using the Elastic Net (Zou and Hastie, 2005; Hastie et al., 2015). The Elastic Net combines optimally two penalty terms: one from LASSO (based on absolute value of the estimated coefficient, enabling elimination of predictors) and one from ridge regression methods (based on the square of the estimated coefficient, not enabling elimination of predictors). This dual approach allows the Elastic

Net to overcome the tendency of LASSO to select only one predictor among highly correlated covariates, and allows dropping out predictors, which is not done by ridge regression alone. The method includes sample folding to optimize the penalty parameters separately from model fitting, thereby trading bias and variance to reduce the likelihood of overfitting.

Additionally, we explore potential heterogeneity in treatment effects using predefined sub-samples based on single observed characteristics such as age, geographical location, past voting behavior, education, and income. These sub-groups were pre-registered in our analysis plan with the American Economic Association Registry for RCTs. Notably, we have not posited specific hypotheses regarding the direction or magnitude of these potential effects. This methodological approach helps us examine how different segments of the population respond to the SMS reminder intervention and it is also easily applicable for policy makers.

Lastly, in order to estimate partisan effects by predicted voting probability separately for a right-wing party and populist party we utilize the Finnish National Election Study survey data. This general population survey conducted after the 2023 parliamentary election, asked eligible voters which party they voted for if they voted in the election. After restricting ages of the respondents to match our target population of youth voters we are left with 961 individuals from the survey. We pooled parties with seats in the parliament to either right-wing parties or left-wing parties and separately created an indicator for reported being voted for the populist the Finns party. Next using the survey data we estimated a logit prediction model including self reported independent variables which could be matched to the administrative data covariates of our sample individuals. These were gender, age, having a higher education degree, household income (quartile) and geographical area. By obtaining the coefficient estimates from the logit model, we predicted the probability of being right-wing party voter and populist party voter for our sample individuals. Then we split the sample by median predicted right-wing and populist party voting propensities, and estimate treatment effects separately for these samples.

5 Results

5.1 Direct and Spillover effects

We begin by estimating the average treatment effect (ATE) of SMS reminders on turnout in Table 3. Column (1) shows that receiving an SMS reminder leads to a 0.6 percentage point (p.p.) increase in turnout in 2023 Parliamentary elections. However, this effect is not statistically significant at the conventional level error levels. As expected, the ATE estimate remains stable after progressively adding demographic control variables. Overall, the size of our point estimate is largely consistent with the findings from existing studies that have examined the effectiveness of text message reminders in high salience elections in the Nordic countries.

Table 3: Average Treatment Effect

	Voted			
	(1)	(2)	(3)	(4)
Treatment	0.006 (0.004)	0.005 (0.004)	0.004 (0.004)	0.005 (0.004)
Controls				
Gender	✗	✓	✓	✓
Age	✗	✓	✓	✓
Ethnicity	✗	✓	✓	✓
Ln income	✗	✓	✓	✓
SES	✗	✗	✓	✓
Education	✗	✗	✓	✓
First-time voter	✗	✗	✓	✓
Municipality FE	✗	✗	✗	✓
Control group $\bar{Y}$	0.625	0.627	0.627	0.627
Observations	49.852	49.327	49.327	49.327

Notes: *** p < 0.01, ** p < 0.05 ,* p < 0.1, standard errors clustered at the municipal level in parentheses. This table follows a pre-analysis plan.

Next we turn to look at the spillover effects. Column (1) from Table 4 shows that the ATE for the intra-household spillovers is around 0.8 p.p. and statistically significant at 10% error level without controls or with controls and municipality fixed effects. This suggests that over 100 percent of the direct treatment effect spilled over to untreated household members similarly as in the 2022 Finnish county elections SMS reminder experiment (Hirvonen et al., 2024).

The presence of sizeable spillover effects has a couple of important implications. Firstly, if impact evaluations fail to account for spillovers among social networks, there's a risk of severely underestimating the true net causal impact. This also has impact on cost-effectiveness calculations of voter mobilization policies. Secondly, when spillovers extend from targeted groups to other groups, the disparity in turnout rates between these groups might not diminish as much as simple direct effect comparisons would suggest; in fact, the gap could potentially widen. Additionally, large spillovers may affect social inequalities in voting behavior within the groups affected by the spillovers.

Table 4: Spillovers - Average Treatment Effect

	Voted			
	(1)	(2)	(3)	(4)
Treated in HH	0.008*	0.007	0.007	0.008*
	(0.005)	(0.005)	(0.005)	(0.005)
Controls				
Gender	✗	✓	✓	✓
Age	✗	✓	✓	✓
Ethnicity	✗	✓	✓	✓
Ln income	✗	✓	✓	✓
SES	✗	✗	✓	✓
Education	✗	✗	✓	✓
First-time voter	✗	✗	✓	✓
Municipality FE	✗	✗	✗	✓
Control group $\bar{Y}$	0.771	0.772	0.772	0.772
Observations	36,135	35,873	35,873	35,873

Notes: *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$, standard errors clustered at the municipal level in parentheses. This table follows a pre-analysis plan.

5.2 Heterogeneous effects by voting propensities

In this section, we evaluate the effect of our intervention on voting inequality by estimating heterogeneous treatment effects by voting propensity groups. Using a logit model, we categorize voters into three groups: Low Propensity Voters, Marginal Voters, and High Propensity Voters. Table 5 (Panel A) displays the direct treatment effects for these groups. Additionally, Table 5 (Panel B) presents estimates of within-household spillovers, following the same approach. We find that the direct effect estimate for the low propensity voters is 2.1 p.p., and is statistically significant at 5% level. The point estimate for the marginal voters is 0.4 p.p. and for the high propensity voters -1.2 p.p, neither being statistically different from zero. The coefficient for the low propensity group is statistically different from the high propensity group estimate at 1% significance level. This suggest that, similarly to the previous experiment Hirvonen et al. (2024) from a much more lower salience elections in the same country, the intervention reduced voting inequality among the targeted youth population.

Table 5 (Panel B) presents the heterogeneous treatment effects by voting propensities within the spillover sample. For this analysis, we predict the voting propensity of each individual living in the same household with a treated or untreated youth voter, and classify these cohabitants into three groups based on their prediction. The results show that the low propensity group has a point estimate of 3.5 p.p. (statistically significant at 1% significance level), while the marginal voters group sees a null estimate

Table 5: Heterogeneity by Vote Propensity

	Voted			
	All	Low Propensity {Bottom 25%}	Marginal Voters {25-75%}	High Propensity {Top 25%}
	(1)	(2)	(3)	(4)
Panel A: Direct Effects				
Treated	0.004	0.021**	0.004	-0.012
	(0.004)	(0.009)	(0.005)	(0.008)
Controls	✓	✓	✓	✓
Control group $\bar{Y}$	0.628	0.442	0.635	0.801
Observations	49.190	12.297	24.595	12.298
Differences		Marginal - Low	Marginal - High	High - Low
		-0.017	0.016	-0.033***
		(0.011)	(0.010)	(0.012)
Panel B: Spillover Effects by HH Members' Voting Propensity				
Treated in HH	0.007	0.035***	0.001	-0.013*
	(0.004)	(0.013)	(0.006)	(0.006)
Controls	✓	✓	✓	✓
Control group $\bar{Y}$	0.773	0.585	0.792	0.926
Observations	35.723	8.930	17.862	8.931
Differences		Marginal - Low	Marginal - High	High - Low
		-0.034**	0.014	-0.048***
		(0.014)	(0.009)	(0.014)

Notes: *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$, standard errors clustered at the municipal level in parentheses. Controls include gender, age, ethnicity, ln taxable income, SES background groups, educational background (high school completion) and an indicator variable whether individual was eligible to vote for the first time. TThis table follows a pre-analysis plan.

of 0.1 p.p.. Conversely, the point estimate for the high propensity group is -1.3 p.p. and statistically significant at 10% level. Similar to the direct treatment effects, these spillover effects appear to diminish socio-economic turnout inequality among untreated individuals. Again this finding corresponds to what we found in the earlier youth voter mobilization experiment in Finland.

Table 6: Heterogeneity by Vote Propensity - Elastic Net

	Voted			
	All	Low Propensity {Bottom 25%}	Marginal Voters {25-75%}	High Propensity {Top 25%}
	(1)	(2)	(3)	(4)
Panel A: Direct Effects				
Treated	0.004	0.015*	0.003	-0.003
	(0.004)	(0.009)	(0.006)	(0.007)
Controls	✓	✓	✓	✓
Control group $\bar{Y}$	0.627	0.449	0.635	0.793
Observations	49.327	12.324	24.671	12.332
Differences		Marginal - Low	Marginal - High	High - Low
		-0.012	0.006	-0.018
		(0.011)	(0.009)	(0.011)
Panel B: Spillover Effects by HH Members' Voting Propensity				
Treated in HH	0.007	0.033**	0.003	-0.012*
	(0.005)	(0.013)	(0.006)	(0.006)
Controls	✓	✓	✓	✓
Control group $\bar{Y}$	0.772	0.583	0.790	0.927
Observations	35.873	8.968	17.936	8.969
Differences		Marginal - Low	Marginal - High	High - Low
		-0.030**	0.015*	-0.045***
		(0.014)	(0.009)	(0.014)

Notes: *** p < 0.01, ** p < 0.05 ,* p < 0.1, standard errors clustered at the municipal level in parentheses. Controls include gender, age, ethnicity, ln taxable income, SES background groups, educational background (high school completion) and an indicator variable whether individual was eligible to vote for the first time. This table follows a pre-analysis plan.

To address concerns about overfitting data when estimating predicted voting probabilities, in Table 6 we have replicated the analysis presented in Table 5 using predictions from the Elastic Net method (Zou and Hastie, 2005; Hastie et al., 2015). We observe in Table 6 that results are very similar to what we obtained with the logit prediction model in Table 5 with slight differences in point estimates and precision in some cases. As with direct effects the low propensity voters have the highest point estimates 1.5 p.p. (statistically significant at 10% error level) and 3.3 p.p (significant at 5% error level), for direct effects (Panel A) and spillover effects (Panel B) respectively. For the case of direct effects there are no statistically significant group differences at conventional significance levels, whereas with the spillover effects t-tests reveal that the low propensity group differs statistically from the marginal voters (at 5% error level) and from the high propensity voters (at 1% error level). In sum, these findings suggest that the spillover effects from our SMS-based GOTV intervention lowers socio-economic participation disparities also in a Finnish high salience election environment.

5.3 Heterogeneous effects by various subsamples

This section provides estimates for treatment effect heterogeneity by splitting data to various subsamples along different covariates one variable at the time. This kind of analysis could be helpful particularly for policy makers as when designing targeted GOTV policies they might lack access to data needed for the more data driven methods presented in the earlier sections. Lastly, in the end of this section we combine the voting propensity group analysis with the univariate subsample analysis by splitting the voting propensity groups by voting status in 2022 county elections.

Table 7 presents estimates, both for the direct (Panel A) and spillover effects (Panel B), where the sample is divided by educational background, ethnical background, voting in 2022 county elections and type of residential municipality (urban vs. rural). By comparing Columns (1) and (2) in Table 7, we observe that the point estimates for both the direct (Panel A) and the spillover effects (Panel B) are higher for lower educational background individuals. However, these estimates are not statistically significantly different from each other. Next looking at direct effects (Panel A) split by ethnicity, we observe that individuals born in Finland to Finnish parents have a positive point estimate (0.5 p.p.), whereas immigrants have a negative estimate (-0.9 p.p.). However, these estimates are not statistically different from zero or each other. When comparing the spillover effects, the estimate for non-natives (1.1 p.p.) is statistically different from zero and from the coefficient for natives (0.1 p.p.) at the 1% error level. This is a contradictory finding compared to results from our previous experiment Hirvonen et al. (2024), but could be explained by that the composition of the non-native sample is different in parliamentary elections compared to county elections due to different voting eligibility requirements.

Columns (5) and (6) of Table 7 presents results by voting in 2022 county elections. When examining those who voted in 2022, point estimates are 0.2 p.p. and -0.2 p.p. for the direct effects (Panel A) and the spillover effects (Panel B) respectively. Turning to individuals who did not vote in the previous elections, we observe that the estimated coefficient is 0.5 p.p. for the direct effect (Panel A) and 1.3 p.p. for the

Table 7: Heterogeneous Effects by Subsamples

	Educational background		Ethnicity		Voting in 2022		Urbanity	
	High School	No High S.	Native	Non-native	Voted	Not voted	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Direct Effects								
Treated	0.002	0.006	0.005	-0.007	0.002	0.005	0.007	0.004
	(0.005)	(0.006)	(0.004)	(0.018)	(0.005)	(0.004)	(0.011)	(0.005)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Control group $\bar{Y}$	0.734	0.543	0.635	0.336	0.924	0.498	0.625	0.628
Observations	21.962	27.365	48.203	1.124	13.350	31.802	6.289	43.038
Differences	-0.004		0.012		-0.003		0.003	
	(0.008)		(0.018)		(0.007)		(0.012)	
Panel B: Spillover Effects								
Treated in HH	0.002	0.011	0.005	0.105***	-0.002	0.013	0.029***	0.003
	(0.005)	(0.007)	(0.005)	(0.031)	(0.003)	(0.008)	(0.011)	(0.005)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Control group $\bar{Y}$	0.861	0.410	0.779	0.416	0.960	0.591	0.755	0.775
Observations	14.969	20.904	35.177	696	17.500	17.642	6.184	29.689
Differences	-0.009		-0.099***		-0.014		0.026**	
	(0.008)		(0.019)		(0.009)		(0.012)	

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the municipal level in parentheses. Controls include gender, age, ethnicity, ln taxable income, SES background groups, educational background (high school completion), and an indicator variable whether individual was eligible to vote for the first time. This table follows a pre-analysis plan.

spillover effect (Panel B). None of these coefficients are statistically different from zero or when tested against each other.

Columns (7) and (8) in Table 7 split the sample into individuals living in rural and urban municipalities. In Panel A, the coefficient for young voters in urban areas is greater (0.7 p.p.) than for individuals residing in rural areas (0.4 p.p.). These estimates are not statistically different from zero or each other. In Panel B, which examines spillovers, the situation is reversed; individuals residing in rural municipalities display a higher point estimate (2.9 p.p.) in contrast to those in urban municipalities (0.3 p.p.). The former estimate is statistically different from zero and from the latter coefficient at 1% error level.

Additionally, in Table 8, we split the three voting propensity groups by the past voting variable resulting in total of six groups, where we assess the treatment effect heterogeneity. Similarly to our earlier findings in Hirvonen et al. (2024) we find that the highest point estimate (1.9 p.p.) belongs to the predicted low propensity voters who had voted in the last elections. This gives support for a theory that the reminders are most effective for individuals who have personal intentions for voting but live in socio-economic environments with few social cues about the elections. However, for this high salience 2023 parliamentary elections none of the six group estimates nor their differences are statistically significant at the conventional error levels.

5.4 Persistence and dynamic effects

In this subsection, we examine the persistence of the treatment effect from 2022 county elections experiment on voting in 2023 parliamentary elections, and the dynamic effects regards having being treated in both elections. For the persistence we look separately direct and spillover effects, where the spillover sample and treatment status is defined from the 2022 elections. We define dynamic effects as difference in the treatment effect if youth voter was treated both in 2022 and 2023 elections versus being in a control group in the 2022 elections and receiving an SMS reminder before 2023 elections.

The original ATE for the 2022 county elections experiment was 0.9 p.p., it being statistically significant at 1% error level. From Table 9 we observe that the point estimate for the direct effect persistence is -0.4 p.p. and it is not statistically different from zero. Thus we cannot rule out that there would be no persistence from the voter mobilization intervention even within the short time interval of a bit over one year. This indicates that at least one-off successful SMS reminder mobilization in low salience county elections is not enough for formation of longer term voting habits for higher salience parliamentary elections for treated youth voters.

Table 8: Heterogeneity by Vote Propensity

	Voted			
	All	Low Propensity {Bottom 25%}	Marginal Voters {25-75%}	High Propensity {Top 25%}
	(1)	(2)	(3)	(4)
Panel A: Voted in 2022				
Treated	0.002	0.019	0.006	-0.007
	(0.005)	(0.017)	(0.008)	(0.006)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	0.923	0.861	0.914	0.957
Observations	13,324	1,863	6,526	4,935
		Marginal - Low	Marginal - High	High - Low
Differences		-0.013	0.013	-0.025
		(0.019)	(0.010)	(0.018)
Panel B: Did Not Vote in 2022				
Treated	0.005	0.013	0.001	-0.003
	(0.004)	(0.011)	(0.006)	(0.012)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	0.498	0.364	0.523	0.659
Observations	31,698	9,707	16,062	5,929
		Marginal - Low	Marginal - High	High - Low
Differences		-0.012	0.004	-0.016
		(0.012)	(0.014)	(0.016)

Notes: *** p $<$ 0.01, ** p $<$ 0.05 ,* p $<$ 0.1, standard errors clustered at the municipal level in parentheses. Controls include gender, age, ethnicity, ln taxable income, SES background groups, educational background (high school completion) and an indicator variable whether individual was eligible to vote for the first time. The analyses reported in the table were not pre-registered and are added ex post.

Turning to the spillover effects, where the point estimate for the 2022 elections was 1.3 p,p, Table 10 shows positive estimates, but these are not statistically different from zero. The point estimate for (3), which shows our baseline specification, is around one third from the original spillover effect. Additionally, we estimate persistence, both for the direct and spillover effect, by various subsamples. These estimates are presented in Table 11. We find positive, and statistically significant, estimates for within household spillover effect persistence for subsamples with high school degree and having voted in 2021 municipality elections. These two characteristics were also predictive of high spillover treatment effects for voting in 2022. The point estimates for spillover persistence are around 75% from the original spillover treatment effect for subsamples having high school degree and past voting in 2021 elections. In contrast we do not find positive estimates which are statistically different from zero for direct effects in any of the subsamples. These results underscore the importance of considering spillover effects when assessing the longer-term impacts of GOTV interventions and policy interventions more broadly.

Table 9: Persistence

	Voted 2023			
	(1)	(2)	(3)	(4)
Treated in 2022	-0.004	-0.004	-0.004	-0.004
	(0.005)	(0.005)	(0.004)	(0.004)
Controls				
Gender	✗	✓	✓	✓
Age	✗	✓	✓	✓
Ethnicity	✗	✓	✓	✓
Ln income	✗	✓	✓	✓
SES	✗	✗	✓	✓
Education	✗	✗	✓	✓
First-time voter	✗	✗	✓	✓
Municipality FE	✗	✗	✗	✓
Control group $\bar{Y}$	0.613	0.614	0.615	0.614
Observations	50.099	49.618	49.618	49.618

Notes: *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$, standard errors clustered at the municipal level in parentheses. This table follows a pre-analysis plan.

Table 10: Spillover Persistence

	Voted 2023			
	(1)	(2)	(3)	(4)
Treated in HH 2022	0.006	0.006	0.004	0.002
	(0.005)	(0.005)	(0.005)	(0.005)
Controls				
Gender	✗	✓	✓	✓
Age	✗	✓	✓	✓
Ethnicity	✗	✓	✓	✓
Ln income	✗	✓	✓	✓
SES	✗	✗	✓	✓
Education	✗	✗	✓	✓
First-time voter	✗	✗	✓	✓
Municipality FE	✗	✗	✗	✓
Control group $\bar{Y}$	0.757	0.758	0.758	0.758
Observations	37.797	37.556	37.556	37.556

Notes: *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$, standard errors clustered at the municipal level in parentheses. This table follows a pre-analysis plan.

As for the direct dynamic treatment effects we don't observe any statistically significant difference between having received an SMS reminder before both 2022 county and 2023 parliamentary elections versus being only treated before 2023 parliamentary elections. The estimated coefficient is around -0.7 p.p. and not statistically different from zero. It can be interpreted that having being treated before does not increase the effectiveness of the subsequent treatment, at least when the initial treatment was in a low salience and the latter treatment in a high salience elections. Absence of both direct persistence and

Table 11: Heterogeneous Persistence Effects by Subsamples

	Educational background		Ethnicity		Voting in 2022		Urbanity	
	High School	No High S.	Native	Non-native	Voted	Not voted	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Direct Effects								
Treated 2022	-0.009	0.000	-0.004	0.021	0.006	-0.005	0.014	-0.006
	(0.006)	(0.005)	(0.004)	(0.021)	(0.005)	(0.005)	(0.014)	(0.004)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Control group $\bar{Y}$	0.713	0.533	0.622	0.308	0.861	0.462	0.591	0.618
Observations	22.477	27.030	48.342	1.165	18.094	29.332	5.628	43.879
Differences	0.009		-0.025		0.012		0.020	
	(0.008)		(0.022)		(0.007)		(0.015)	
Panel B: Spillover Effects								
Treated in HH 2022	0.016**	-0.004	0.006	-0.061*	0.011***	-0.001	-0.015	0.008
	(0.007)	(0.007)	(0.005)	(0.032)	(0.004)	(0.007)	(0.014)	(0.006)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Control group $\bar{Y}$	0.836	0.701	0.779	0.765	0.931	0.540	0.772	0.756
Observations	21.588	15.968	36.771	785	20.466	15.389	5.971	31.585
Differences	0.020***		0.066**		0.012		-0.023	
	(0.010)		(0.032)		(0.008)		(0.015)	

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the municipal level in parentheses. Controls include gender, age, ethnicity, ln taxable income, SES background groups, educational background (high school completion), and an indicator variable whether individual was eligible to vote for the first time. This table follows a pre-analysis plan.

dynamic effects would suggest that an effective policy to mobilize youth voters would require reminders to repeated before every election.

Table 12: Dynamic Effects

	Voted 2023			
	(1)	(2)	(3)	(4)
Treated Twice vs Once	-0.005	-0.006	-0.007	-0.007
	(0.007)	(0.007)	(0.007)	(0.007)
Controls				
Gender	✗	✓	✓	✓
Age	✗	✓	✓	✓
Ethnicity	✗	✓	✓	✓
Ln income	✗	✓	✓	✓
SES	✗	✗	✓	✓
Education	✗	✗	✓	✓
First-time voter	✗	✗	✓	✓
Municipality FE	✗	✗	✗	✓
Control group $\bar{Y}$	0.631	0.633	0.633	0.633
Observations	18.702	18.513	18.513	18.513

Notes: *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$, standard errors clustered at the municipal level in parentheses. This table follows a pre-analysis plan.

5.5 Partisan Effects

The upper panel of Table 13 presents estimates of heterogeneous treatment effects by predicted partisanship for the 2022 elections. The sample is divided in half based on the median predicted propensity to vote for right-wing parties (columns (1) and (2)) and the median predicted propensity to vote for the populist party (columns (3) and (4))[3]. As shown, we do not observe any statistically significant differences between predicted right-wing and populist voters. The lower panel replicates this analysis for the 2023 elections, and again, no statistically significant differences are detected. For likely populist voters, the point estimate is -0.3 percentage points, but it is not statistically different from zero.

While more likely populist voters have a lower control group mean, we do not find higher treatment effects for these groups. This is in comparison with earlier analyses in this paper, which found that low-propensity voters were the most responsive to the treatment. These findings suggest that, although youth voters are a key demographic for the Finns populist party, they do not disproportionately benefit from voter mobilization interventions targeting youth voters.

[3]We find qualitatively similar results when splitting the sample according to the mean predicted voting proportions for right-wing and populist parties.

Table 13: Heterogeneous Effects by Predicted Right-wing and Populist Party Voting

	Outcome: Voted			
	Predicted Right Voter		Predicted Populist Voter	
	Below Median	Over Median	Below Median	Over Median
	(1)	(2)	(3)	(4)
Panel A: 2022 Elections				
Treated	0.007	0.009	0.006	0.009
	(0.007)	(0.005)	(0.005)	(0.005)
Controls	✓	✓	✓	✓
Control group $\bar{Y}$	0.339	0.295	0.385	0.249
Observations	20.299	21.286	20.857	20.728
Differences	-0.003		-0.003	
	(0.010)		(0.008)	
Panel B: 2023 Elections				
Treated	0.002	0.004	0.010	-0.003
	(0.008)	(0.008)	(0.007)	(0.006)
Controls	✓	✓	✓	✓
Control group $\bar{Y}$	0.667	0.641	0.715	0.593
Observations	19.015	19.112	18.939	19.188
Differences	-0.002		0.013	
	(0.011)		(0.009)	

Notes: *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$, standard errors clustered at the municipal level in parentheses. This table follows a pre-analysis plan.

6 Conclusions

First, we find that the direct main effect (ATE) is positive at 0.6 p.p., but not statistically significant. Second, we find that more than 100% of direct effect spills over to the untreated household members with spillover ATE point estimate being 0.8 p.p. and statistically significant at 10% level. The direct effect result differs from Hirvonen et al. (2024) who found also that being significant, but the large spillover result is similar.

Third, with respect to inequality, we show that for both the direct effect and the spillover effect the effects are statistically significant only for the voters that based on their socio-economic and demographic characteristics have a low predicted propensity to vote. The direct effect for this group is 2.1 p.p. and is significant at 5% level and the spillover effect is 3.5 p.p. and significant at 1% level. Therefore, similar to Hirvonen et al. (2024) we find that the intervention reduced voting inequality among the targeted youth population. This means that SMS reminder can reduce inequalities in voting in both high and low salience elections and in both contexts spillover amplify this result. Studying the generalizability of the effects across various types of elections is one main novel contributions of this study.

Fourth, again similarly to our earlier findings in Hirvonen et al. (2024) we find that the highest point estimate (1.9 p.p.) belongs to such low propensity voters who had still voted in the last elections. This seemingly paradoxical result is consistent with an argument that the reminders are most effective for individuals who have personal intentions for voting but live in socio-economic environments with few social cues about the elections. This is consistent with the Noticeable Reminder Theory (Dale and Strauss, 2009) that the effect comes from citizens who have an intention to vote, but may fail to do so, because of lack of attention, in which case only a simple nudge is enough to remind them of their intention. It is also consistent with the The Receive-Accept-Sample Theory (Zaller, 1992) as citizens belonging to this social group may not receive too many messages, and thus, accepting the message is particularly likely for the voters who have high individual interest in voting, but live in low interest environment. Taken together, this combination of high interest but low exposure could make nudging more efficient.

Fifth, we do not observe statistically significant dynamic effects either from the perspective of persistence nor from having interaction effects between the two experiments in terms of direct effects. That is, voters who received the SMS in county elections are no longer more likely to vote also in the parliamentary elections, and voters who received the SMS reminder in both cases are no more likely to vote than those who received it only in the parliamentary elections. Documenting these dynamic effects or their absence is another main novel contribution of our study. Absence of both persistence and dynamic direct effects suggests that an effective policy to mobilize youth voters requires SMS reminders to be sent repeatedly before every election. However, we do observe persistence in spillover effects within certain subsamples that exhibited strong spillover treatment effects in the first experiment. This finding underscores the importance of considering spillover effects when assessing the lasting impacts of policy interventions.

Finally, we did not detect partisan differences in direct treatment effects when estimating heterogeneous treatment effects by predicted voting for right-wing and populist parties. In the 2023 experiment the point estimate for likely populist voters is close to zero. This is notable given that likely populist voters tend to have low turnout rates, and our other results indicated higher treatment effects among low-turnout voters. These findings suggest that GOTV interventions, at least in the Finnish context, do not disproportionately benefit populist parties, despite youth voters being one of their key demographics.

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