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Policy Feedback and Civic Engagement: Evidence from the Finnish Basic Income Experiment

Aboa Centre for Economics

Discussion paper No. 155

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May 2023 (previous version November 2022*)

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ABSTRACT

In many democracies, unemployed and low-income citizens are less willing to vote. Can social policies weaken the link between income and turnout? We study policy feedback leveraging a unique experiment in Finland, which randomly assigned a sizable group of unemployed to receiving an unconditional basic income for two years (2017-19). Combining individual-level registry and survey data, we show that the intervention has large positive effects on actual voter turnout and subjective levels of political efficacy. Basic income increases turnout in municipal elections by about 3 p.p., on average, an effect that is concentrated among marginal voters (+ 6-8 p.p.) and persists in national elections after the end of the experiment. Exploring possible mechanisms, our analysis highlights the role of interpretive effects, which boost political efficacy through various channels including trust in government. We discuss implications for theories of voter turnout and policy feedback, and the design of basic income policies.

JEL Classification: C93, D72

Keywords: Basic income, Field experiment, Turnout

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1 Introduction

In many democracies, socio-economic status (SES) correlates with political participation (e.g., Brady, Verba and Schlozman 1995). Most notably, voter turnout is higher among richer and more educated citizens, a pattern that is well-documented in advanced industrial economies including the U.S. (Leighley and Nagler 2013), Italy (Schafer et al. 2021), and Finland (Lahtinen et al. 2019). This is troublesome because unequal political participation usually leads to unequal descriptive (e.g., Lijphart 1997) and substantive (e.g., Harjunen, Saarimaa and Tukiainen Ftcm.) representation. The income-turnout relationship seems particularly problematic because income is the main factor that determines individual levels of taxation and benefits (Leighley and Nagler 2013). Simply put, the income bias in voter turnout matters because it creates a link between economic inequality and political inequality. This study asks whether social policies that benefit the poor, such as Basic Income (BI), can help to weaken this link.

To address this question, we study an exceptionally large policy experiment in Finland and examine its feedback effects on mass public behavior. Targeting the unemployed, this intervention randomly assigned an unconditional BI over a period of two years (2017-19). At the beginning of the program, both treatment (N=2,000) and control participants (N=173,200) were unemployed and the level of monthly BI payments (€560) for unemployed treated individuals was roughly the same as the unemployment benefits for control group individuals (Kangas et al. 2021). Unlike the control group, treated individuals continued receiving BI payments even if they found a new job or stopped looking for employment. Note that, although the policy made conditional cash transfers unconditional for all treated participants, its effect on income was contingent on finding a new job. In addition, a majority of treated (and control) participants received means-tested support such as housing benefits (Verho, Hämäläinen and Kanninen 2022).

Combining theories of political behavior and policy feedback, we discuss reasons to expect that this intervention should stimulate civic engagement among the unemployed. The literature suggests two alternate pathways: a resource channel (Brady, Verba and Schlozman 1995; Schafer et al. 2021) and an interpretive channel (Soss 1999; Jacobs, Mettler and Zhu 2022). On the one hand, increasing income, especially among relatively poor voters, may in itself have a positive effect on turnout. On the other hand, the unconditionality of BI payments may increase the feeling of political efficacy through various channels including trust in government. In addition, we theorize that the impact of BI should be heterogeneous among different voting propensity groups (Arceneaux and Nickerson 2009; Enos, Fowler and Vavreck 2014). It should be stronger among the "marginal" voters who sometimes vote, compared to those who usually or always vote, and those who rarely or never vote.

In the empirical analysis, we leverage individual-level registry data on voter turnout and personal characteristics linked to the experimental treatment status. We find support for our theoretical prediction that receiving BI boosts voter turnout, namely by about 3 percentage points (p.p.), on average, from a baseline of 36% among the control group.[1] Consistent with our expectations, this effect is concentrated among marginal voters (+ 6-8 p.p.), who, as the experiment targets the unemployed, typically have some secondary school but no university education. Although the intervention has no turnout effect on low/high propensity voters, the large effect on marginal voters narrows the turnout gap between unemployed and employed citizens, thus reducing turnout inequality, on average.

Our rich administrative records, together with additional survey evidence, shed light on possible mechanisms. The large effect on turnout at the local level seems inconsistent with vote buying (e.g., Gonzalez Ocantos, De Jonge and Nickerson 2014) given that the BI program was administered at the national level. In addition, the turnout effect persists in national parliamentary elections held after the end of the experiment, which indicates a long-term impact on political behavior. Interestingly, our registry data suggest that turnout does not increase among treated participants who find a new job, and thus receive additional income from the program. Instead, our results seem to be driven by those who remain unemployed over the period of study. Yet, this comports with our survey data indicating that the unconditionality of BI payments increases political efficacy through various channels. While the subjective feeling of efficacy also increases among low propensity voters, the actual

[1]For comparison the average turnout of the whole voting eligible population in our sample voting areas was 57.5% (Statistics Finland 2017).

turnout effect is concentrated among the marginals, which seems consistent with a "threshold" model of political mobilization (Arceneaux and Nickerson 2009).

The extraordinary scale of this nation-wide field experiment and the high internal validity of our individual-level data advance the literature studying the turnout effects of social policies that benefit the poor (e.g., De La O 2013; Clinton and Sances 2018; Akee et al. 2020). For example, two recent studies document the positive effects of unconditional cash transfers on turnout in Alaska (Loeffler 2022) and Brazil (Araújo 2022) using aggregate-level difference-in-difference designs. However, these findings may be limited by time-varying confounds and the risk of ecological fallacy (Arceneaux 2003). We overcome these problems providing individual-level causal estimates from a randomized control trial. Combining an exceptionally well-funded experiment with high quality administrative and survey data, our research design not only avoids possible measurement error from self-reported voting (e.g., Lahtinen et al. 2019), but also allows us to explore the heterogeneous causal effects of the policy using machine learning techniques. In addition, we are able to look at the persistence of the effect as our data contain multiple elections, and to explore possible mechanisms by benchmarking our estimates between administrative and survey data.

While studying this unique experiment is important on its own, we argue that our findings can guide scholars and policymakers interested in the design and implementation of BI policies in developed (Hoynes and Rothstein 2019) and in developing (Hanna and Olken 2018) countries. To this end, we discuss what aspects may be specific to our setting and then highlight what general lessons may travel to other contexts. In particular, our suggestive evidence that additional income does not affect turnout is conditional on participants finding a new job and should be interpreted with caution. However, our finding that the feedback effects of social policy are concentrated among marginals speaks to a large body of theory and has important implications for the study of turnout inequality. Moreover, our results show that the interpretive effects of BI boost multiple measures of social capital among marginalized citizens, allowing us to make a nuanced impact evaluation of the Finnish BI program. Broadly speaking, our results corroborate the view that targeted BI policies would more efficient than universal ones (e.g., Hanna and Olken 2018).

2 Theoretical Background

To formulate hypotheses about how receiving BI may affect voting participation, we start by building on a growing literature that blends theories of political behavior and policy feedback to study how government assistance affects political attitudes and behaviors. We then extend this work theorizing about individual-level heterogeneity and its consequences on turnout inequality.

2.1 Resource and Interpretive Effects of Social Policies

While the extant literature documents that, in most western democracies, the rich vote more than the poor (e.g., Kasara and Suryanarayan 2015), it also provides some evidence that social policies may help to reduce this gap. In developing countries, government assistance to the poor is often associated with "vote buying" (De La O 2013; Gonzalez Ocantos, De Jonge and Nickerson 2014). This practice is less common in advanced democracies with programmatic political parties, though, in this context, policies may affect electoral politics through "policy feedback". Focusing on the Affordable Care Act in the U.S., for example, Clinton and Sances (2018) demonstrate that a policy expanding government transfers to low-income voters led to a large increase in county-level turnout. Leveraging individual-level vote records, Markovich and White (2022) provide evidence that increasing the minimum wage leads to higher turnout. Yet, despite this recent work, the policy feedback literature has been limited by a dearth of causal evidence and we know little about mechanisms (Campbell 2012).

Theoretically, the literature suggests two different channels through which redistributive policies like BI may influence turnout. First, the influential "resource model" posits that citizens need money, time, and civic skills to participate in politics (Brady, Verba and Schlozman 1995; Solt 2008). This suggests that the poor vote less than the rich vote because they have fewer participatory resources. Yet, while the experience of poverty should depress turnout at the bottom of the distribution, income may not have much effect beyond that. Thus, we should expect a curvilinear relationship between income and turnout (Rosenstone 1982).

This pattern is well-documented in the "advanced democracies" of Western Europe and North America (e.g., Kasara and Suryanarayan 2015). It also holds in countries like Finland despite comparatively low levels of income inequality and high levels of voter turnout, on average (Lahtinen et al. 2019). Recent empirical work examines whether this macro-correlation also holds at the individual-level, and whether the effect of income is clearly distinct from other correlated factors like education. Schafer et al. (2021) leverage individual-level administrative data from Italy confirming that the impact of income on voter turnout has diminishing returns: additional income has a large effect among the poor, some effect among the lower-middle class, but no effect among the middle and upper class. Using large-scale survey data from Germany, Schaub (2021) documents that financial hardship, which often worsens at the end of the month, reduces voting participation among the poor.

Although existing research clearly shows that negative changes in income depress turnout among the poor, it is less clear whether interventions that lead to positive changes in income will stimulate participation. Analyzing state-level differences-in-differences, Loeffler (2022) finds that yearly payments made to all Alaskan residents from Oil Fund Dividends increase turnout. Using a similar design at the municipality-level, Araújo (2022) finds that monthly unconditional cash transfers targeting the poor boost electoral participation in Brazil. Yet, these findings may be limited by time-varying confounders and the risk of ecological fallacy when analyzing individual behavior with aggregate data – i.e., changes in income may coincide with other community-level changes that affect turnout, also among non-treated individuals (Arceneaux 2003). Examining the impact of unconditional cash transfers related to Native American casinos at the individual-level, Akee et al. (2020) find no effect on turnout among low-income voters in the short-run, though there appears to be an effect on children's participation in the long-run. Thus, the prior evidence is mixed and calls for more empirical studies studying effect heterogeneity and persistence at the individual-level.

The second mechanism pertains to the interpretive effects of social policies. Whereas the voter behavior literature discussed above usually focuses on the amount of money that individuals receive from government transfers (Akee et al. 2020; Schafer et al. 2021; Araújo 2022; Loeffler 2022), the policy feedback literature suggests that *how* these payments are made also affects civic engagement (e.g., Soss 1999; Jacobs, Mettler and Zhu 2022). Specifically, prior work provides reasons to expect that the (un)conditionality of cash transfers impacts political efficacy – i.e., "the feeling that individual political action does have, or can have, an impact upon the political process" (Campbell, Gurin and Miller 1954) – in various ways.

A large literature documents the links between social capital, political efficacy, and civic engagement (Putnam, Leonardi and Nanetti 1994; Putnam et al. 2000; Atkinson and Fowler 2014; Poertner 2023). In fact, the Finnish BI experiment explicitly aimed to improve the social inclusion of low-income citizens (Kangas et al. 2021). As suggested by social exchange theory (e.g., Cropanzano and Mitchell 2005), unconditional cash transfers may trigger a sequence of giving and counter-giving, in which BI recipients feel obliged to reciprocate by adopting more pro-social attitudes. Thus, we should expect that receiving BI increases trust towards governmental institutions, as well as broader levels of interpersonal trust.

In addition, prior work on the turnout effects of welfare state provision shows that program participation may diminish the feeling that one can influence societal outcomes, particularly so if it involves negative experiences with the bureaucracy (Soss 1999). Conversely, by reducing the bureaucracy involved in receiving payments from the government, BI may increase the feeling of political efficacy, and thus raise electoral participation.

2.2 Policy Feedback and Marginality

An important limitation of prior work on policy feedback is the paucity of individual-level evidence on who is mobilized to vote, despite strong theoretical reasons to expect heterogeneity. In particular, the Get-Out-the-Vote (GOTV) literature, studying the impact of electoral campaigns, holds that there are three types of voters: "always-voters", "marginal voters", and "never-voters" (Arceneaux and Nickerson 2009; Enos, Fowler and Vavreck 2014; Fowler 2015). This body of research suggests that mid-propensity voters only participate in elections they consider important. Thus, we may expect that marginals should be most likely to be mobilized by BI raising election salience. This heterogeneity may have major consequences on turnout inequality, possibly reducing the turnout gap between marginal voters and high propensity voters, but increasing the gap to low propensity voters. To our knowledge, though, no prior work has studied policy feedback in terms of who is marginal.

Marginality is likely a function of demographic predictors of turnout like education. Although highly educated individuals often tend be "always voters", education may, conditional on unemployment (i.e., in our data), be a good predictor of being marginal. For example, BI recipients who finished high school may be more responsive to treatment, compared to those who didn't, because they have better civic skills and higher levels of interest in politics (Sondheimer and Green 2010). Yet, it is unclear whether this prediction holds at the bottom of the income distribution. In addition, voting behavior may be more malleable among young voters who have not yet completed their formative years (e.g., Akee et al. 2020), though the moderating influence of age may not be critical in our case as all study participants were at least 25 years old (see Experimental Design section). Given that the Finnish BI experiment targeted the unemployed, we may expect that, compared to the entire voting population, the proportion of citizens who always vote should be smaller, if anything. However, who exactly is marginal with respect to BI is an empirical question.[2]

We note that the effect of BI on political efficacy and its consequences on voter turnout will likely be heterogeneous, as they depend on baseline levels of social capital. Whereas receiving BI may have large effects on trust in institutions among low propensity and marginal voters, it may not have much effect among high propensity voters who are already used to participating in elections. By the same token, an increase in political efficacy may lift the salience of an election above the "indifference threshold" (Arceneaux and Nickerson 2009) among marginal voters without achieving the same result among low propensity voters.

2.3 Summary Hypotheses

To summarize, we expect that BI should increase voter turnout, though the magnitude and persistence of this effect are less clear a priori. Prior theory suggests two alternate mechanisms. First, by increasing participatory resources among the poor, income itself may

[2]We discuss ways to empirically explore this heterogeneity without "over-fitting" the data in the Data and Methods section.

boost turnout. Second, receiving an unconditional BI may have interpretive effects increasing political efficacy through various channels like trust in institutions and the feeling that one can influence politics. We argue that policy feedback effects should be heterogeneous. Specifically, our theory predicts that the turnout effects should be concentrated among the "marginal voters" who sometimes vote and have intermediate levels of political efficacy at baseline. There should be no effect among the "high propensity voters" who usually vote and already have high levels of political efficacy, whereas among the "low propensity voters" who rarely vote, low levels of political efficacy may increase without leading to higher turnout.

3 Experimental Design and Setting

To empirically study the effect of BI on voting participation, we leverage a unique policy experiment in Finland. This intervention randomly assigned about 1% of its target population – i.e., individuals aged 25-58 claiming unemployment insurance as of November 2016 – to receiving unconditional BI transfers in lieu of conditional unemployment benefits over a period of two years (January 2017 to December 2018). Compared to other BI programs conducted to date (reviewed in Hanna and Olken (2018) and Hoynes and Rothstein (2019)), the Finnish experiment stands out in that it was nation-wide rather than local, included a very large number of participants (2,000 in treatment and 173,200 in control), and involved high monthly payments of €560 (US$615) or about 30% of national median income.[3] The

[3]For comparison, BI payments in the municipality of Maricá, Brazil, are R$170 (US$35) per month (Araújo 2022), and can only be spent locally; the dividend payments from the

total budget was €20 million, though the net cost was lower (€5.5 million) as fewer treated participants than expected took up new jobs (see below). Note that the Finnish program differed from the type of policy that many BI proponents would prefer on normative grounds in that it was not "universal" but targeted the unemployed (Bidadanure 2019).

3.1 Overview Policy Benefits

At the beginning of the experiment, both treatment and control participants were unemployed. The level of monthly BI payments was set at €560 – i.e., about the same level as after-tax unemployment insurance. Yet, unlike the control group, treated participants continued receiving BI payments even if they found a new job or stopped looking for employment (Kangas et al. 2021). In practice, replacing unemployment insurance with BI did not eliminate all conditions associated with cash transfers to the unemployed. For example, many BI recipients applied for additional benefits such as the child supplement of unemployment benefits or sick leave that involved the same job search requirements as unemployment insurance (Verho, Hämäläinen and Kanninen 2022). Nevertheless, the Finnish experiment aimed to reduce the conditionality of government transfers (Kangas et al. 2021).

Importantly, the income effect of this intervention varied depending on both treatment assignment and post-treatment occupation (Verho, Hämäläinen and Kanninen 2022). First, among those who remained unemployed, treated and control participants received roughly

Alaska Oil Fund fluctuate between US$300 and US$2,000 per year (Loeffler 2022); the annual payouts from Native American casinos are about US$4,700 (Akee et al. 2020).

the same basic level of transfers and were equally eligible to apply for additional means-tested government programs such as housing and social assistance. Second, among those who found a new job, treated participants received BI payments on top of their salary without BI payments being taxed out completely, whereas control participants only received their salary. Third, among those who stopped actively looking for a job, treated participants continued receiving BI payments unless they claimed benefits from other targeted government programs – such as pensions and subsidies for childcare at home – or moved abroad,[4] whereas control participants faced possible sanctions. More specifically, control group participants who failed to meet the goals specified in their "employment plan" by the public employment services – such as submitting job applications, arranging a health check-up, or participating in active labor market programs – would risk disqualification from the main type of unemployment insurance – which usually lasts for up to 400 weekdays – and would instead receive less generous benefits – which are paid indefinitely but are wealth-tested (Verho, Hämäläinen and Kanninen 2022).

3.2 Political Context

The political circumstances in which this policy was introduced deserve close attention. Finland has an open list proportional representation electoral system. Each voter gives one vote to a single candidate. Voting for a party in isolation of selecting a candidate is not possible. Candidates are presented in alphabetical order, sorted by party, leaving voters

[4]BI payments were discontinued for 137 out of 2,000 treated individuals.

without obvious signals on the parties' preferences over their candidates. Moreover, there are typically hundreds of candidates to choose from. Thus, the information environment is challenging to voters (Cunow et al. 2021).

The national parliament regularly includes eight political parties, which also dominate municipal politics (Lyytikäinen and Tukiainen 2019). The BI program was administered at the national level. Note, however, that local government spending accounts for about 18% of Gross Domestic Product (GDP) and municipal politics have an important impact on citizens' lives (Hyytinen et al. 2018). There are no major differences in the composition of the electorate between municipal and national elections in our sample (i.e., among the unemployed – see Table 2 and Appendix Table A15), which suggests that the effects of treatment may be comparable across election types. Voter registration is automatic and, although vote-by-mail is restricted to citizens living abroad, advance voting is possible and fairly common.[5]

Importantly, Finland has a generous "Nordic" system of social protection (Esping-Andersen 1990). Finland's social spending is near 30% of GDP, second only to France among Organisation for Economic Co-operation and Development (OECD) countries. Two thirds of that spending goes to cash benefits and the remainder to providing social and health services (Kangas et al. 2021). Public support for redistributive policies (Rueda 2018) and average levels of interpersonal (Butler, Giuliano and Guiso 2016) and institutional (Kangas et al.

[5]https://dvv.fi/en/right-to-vote

2021) trust are high compared to other advanced economies. Yet, there is also a concern that, for bureaucratic reasons, people can become trapped in the Finnish welfare system. For example, there may be disincentives to seek taxable low-wage or part-time work.

While there is a consensus among political parties about preserving and modernizing the Finnish welfare state, a major debate revolves around the means to achieve these ends. The Finnish BI experiment was introduced by a center-right government (Kangas et al. 2021), which may seem surprising given that BI is usually associated with left-wing politics in many democracies (Bidadanure 2019). In Finland, however, the idea of giving BI a try gathered broad support in parliament and was backed by all major parties (Kangas et al. 2021). Although the BI experiment spurred some public debate in Finland and beyond, it was a mostly elite-driven project (Kangas et al. 2021). Yet, as political support for BI within the ruling coalition diminished shortly after the beginning of the experiment, it became clear that the program would not be extended beyond its initial two years (Kangas et al. 2021).

In this context, the Finnish BI experiment was designed to achieve two primary goals: to reduce bureaucracy and to encourage labor force participation (Kangas et al. 2021). Accordingly, the program was restricted to the unemployed and the level of monthly payments (€ 560) was too low to replace other means-tested transfers such as housing or child support. Nevertheless, the Finnish experiment was quite expensive: € 20 million budget for 2,000 treated participants over two years, though the net cost was lower (€ 5.5 million) as fewer unemployed than expected found new jobs. Thus, it likely provides an upper bound, if anything, for the BI policies that are practically feasible in advanced industrial economies.

3.3 Implementation and Prior Program Evaluations

Several aspects of policy implementation should be carefully considered. First, treated participants received a letter informing them about the BI program (Kangas et al. 2021). Moreover, unemployment benefits were paid out at the end of the month but BI transfers were paid out at the beginning, resulting in two payments to the treatment group around January 1, 2017 (Verho, Hämäläinen and Kanninen 2022). Thus, it is reasonable to assume that all treated participants were aware of the treatment, whereas control participants may not necessarily have known about the program. This raises the possibility that treated individuals may have changed their behavior in response to their awareness of being observed. Note, however, that such an effect would likely affect all treated participants. Therefore, observing heterogeneity as predicted by our theory would mitigate concerns about bias.

A second concern pertains to how the policy was discontinued. Knowing that the program would not be extended after two years may have affected participants' attitudes and behaviors. Yet, we can test whether the effects persist after the end of experiment, which would further allay concerns about bias.

Prior program evaluations show that the impact on economic and social outcomes was mixed. In registry data, BI has no effect on labor force participation in the first year and perhaps a small positive effect in the second year (Verho, Hämäläinen and Kanninen 2022). Interestingly, treated participants who remain unemployed show about the same effort seeking jobs as control participants even though they have less bureaucratic pressure to do so, thus suggesting that they often don't find employment because of insufficient fit with the job market – rather than lack of trying (Verho, Hämäläinen and Kanninen 2022). Using survey data, another study finds positive effects on institutional and interpersonal trust, the feeling that one can influence social outcomes, and subjective well-being (Kangas et al. 2021). Therefore, an important debate revolves around how to reconcile findings from registry and survey data. Here we examine effects on political behavior employing both.

4 Data and Methods

In this section, we describe the data and methods we use in our main analysis focusing on administrative records. We then also describe our additional survey data.

4.1 Registry Data

Our registry data come from three different sources: treatment status of the UBI experiment provided by KELA (the Social Insurance Institution of Finland), individual-level turnout data from the electronic voting registries, and finally administrative demographic covariate data provided by Statistics Finland (SF). We asked SF to merge these data, using social security codes as identifiers, into a final unique pseudonymous data set that can only be used through the SF remote access system.

Note that not all municipalities and within municipalities not all precincts have digitized voter rolls into an electronic voting registry. For the April 2017 municipal elections, out of

Table 1: Municipality Demographics by Electronic Voting Registry

	All	100% in Election Registry	>0% in Election Registry
Voter turnout	.616	.614	.610
	(.0535)	(.0528)	(.0504)
Population	18702	14194	29513
	(49633)	(31025)	(72033)
Share of tertiary educated	.230	.222	.245
	(.0643)	(.0654)	(.0691)
Employment rate	.699	.694	.702
	(.0556)	(.0530)	(.0546)
Unemployment rate	.114	.115	.112
	(.0356)	(.0342)	(.0340)
Observations	293	59	116

Note: Standard deviation in parentheses.

293 municipalities, 59 have a complete and 116 have a partial coverage, leaving us with 655 (out of 2,000) treated and 53,583 (out of 173,200) control group individuals.[6] Table 1 presents demographics of municipalities by their electronic voting register status, showing that our sample municipalities are fairly representative of the whole country. For example, there is only .2 (.6) p.p. difference in turnout between all Finnish municipalities and those that have full (partial) coverage of the electronic voting register. The only noteworthy difference is that municipalities where all precincts are covered tend to be smaller in terms of population than the national average. Moreover, as we show in Appendix Figure A4, the municipalities in our sample are geographically representative.

We also observe individual-level vote records for two additional post-treatment elections: the April 2019 national parliamentary elections and the June 2021 municipal elections. As

[6]The Appendix Table A13 shows balance tests for pre-treatment covariates and past voting in 2015.

the number of municipalities included in the electronic voting registry increases over time, we are able to match a larger number of BI experiment participants to the vote registry in 2019 (N=63,841) and 2021 (N=75,924) compared to 2017. In addition, we observe pre-treatment turnout in the 2015 parliamentary election (N=29,643). To test our hypotheses, we begin the analysis focusing on the 2017 municipal elections, and then examine whether the effect of treatment persists over time.

4.2 Estimation

The main outcome of interest is whether the experimental participants voted in subsequent municipality and parliamentary elections. We estimate our main effects using the following linear regression model:

$$Y_i = \beta_0 + \beta_1 Treatment_i + \boldsymbol{X}_i'\boldsymbol{\beta} + \epsilon_i \tag{1}$$

where Y_i indicates whether an individual voted or not; β_1 is the estimated treatment effect; $\boldsymbol{X}_i$ is a vector of controls (described in greater detail below, see Table 2), including age, gender, education, log of taxable income and socio-economic status; ϵ_i is the error term, errors are clustered at the municipal level.[7] As $Treatment_i$ is randomly assigned, β_1 identifies the causal effect of interest. The purpose of the controls is to reduce residual variation.

[7]From a design-based perspective, clustering may not be necessary as our treatment is assigned at the individual level (Abadie et al. 2022). However, clustering accounts for municipal-level sampling variance as we observe only a subset of Finnish municipalities.

4.3 Heterogeneity

To examine theoretically-important heterogeneity, we also estimate equation (1) separately among different types of voters ("low propensity", "marginal voters", "high propensity"). To this end, we sort voters into different groups based on their baseline vote propensity, which we predict using the following logistic regression model:

$$Pr(Y_i = 1|\boldsymbol{X_i}) = \frac{exp(\boldsymbol{X}_b)}{1 + exp(\boldsymbol{X}_b)} \tag{2}$$

where $Pr(Y_i = 1|X_i)$ is the predicted probability voting, given the control group individuals' age, gender, education, log of income and socio-economic status (profession) measured before the intervention and residential municipality fixed effect. [8]

Table 2 shows descriptive statistics for the different groups in our sample, which were all unemployed at the beginning of the experiment. In our main analysis, we split the sample into bottom 25th, 25-75th, and top 25th percentiles. Therefore, we split the sample into half between the group where we expect the effect and the rest of the sample. That is, we maximize statistical power to test the hypothesis that treatment effects are concentrated among the "marginal voters". We use the same split in multiple election-years and across registry and survey data. However, our key results are robust when we instead split the

[8] As a robustness check to take into account the uncertainty induced by the "first stage" of predicting the voting propensities: we compute standard errors by bootstrapping (1000 replications) the whole procedure of first predicting the voting propensities and then dividing observations into three groups according to the voting propensity percentiles. We show results in Appendix Tables A22, A23, A24 and A25.

Table 2: Demographic Covariates by Vote Propensity

	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%	All
Pre-Tax Income	9831.54	10778.14	11556.32	10736.05
	(3457.35)	(3881.37)	(5036.79)	(4151.65)
Female	0.41	0.47	0.56	0.48
	(0.49)	(0.50)	(0.50)	(0.50)
Age	33.01	41.22	45.82	40.32
	(6.78)	(9.62)	(9.43)	(10.07)
High School	0.05	0.17	0.44	0.21
	(0.23)	(0.37)	(0.50)	(0.40)
Observations	13,576	27,153	13,577	54,306

Note: Standard deviation in parentheses.

sample into three equal groups – i.e., in bottom 33th, 33th-67th, and top 33th percentiles (see Appendix Table A12).

The main difference between the three subsamples is education: Whereas only 5% of "low propensity" voters graduated from generalist high school, 17% of "marginal voters" and 44% of "high propensity" voters have at least a generalist high school degree.[9] There is also an age and gender gradient among the voting propensity groups: the higher the predicted voting, more likely the individual is going to be older and female. In contrast, the pre-tax income differences between the three groups are relatively small and negligible because all study participants were unemployed at the beginning of the experiment.

This method is consistent with the standard approach to studying marginality in the

[9]In the Finnish education system, a generalist high school degree provides access to university, but many students choose to pursue a vocational high school degree instead.

GOTV literature, which estimates baseline vote propensities using pre-treatment covariates (e.g., Arceneaux and Nickerson 2009; Enos, Fowler and Vavreck 2014). We use sample splits instead of an interaction model because sample splitting is more flexible by allowing also the estimates relating to the control variables to differ across the samples. However, we note that, in our data, this approach may pose the risk of "over-fitting" – i.e., some of the differences between voter "types" may be driven by random variation in a few observations.

To address this concern, we also analyze this heterogeneity using an alternative approach, namely the Elastic Net (Zou and Hastie (2005) and Hastie, Tibshirani and Wainwright (2015)). Instead of using the full set of standard predictors of turnout to estimate baseline vote propensities, the Elastic Net chooses optimal predictors combining two penalty terms: one from LASSO (based on absolute value of the estimated coefficient, enabling elimination of predictors) and another from ridge methods (based on the square of the estimated coefficient, not enabling elimination of predictors). Therefore, the method overcomes, first, the problem of LASSO selecting only one predictor among highly correlated covariates. Second, the method allows dropping out predictors in general, which is not done by ridge regression alone. The procedure employs sample splitting to separate the choice of parameters for penalty terms and fitting the model. Compared to regular logit, the Elastic Net trades some bias for less variance by using penalty terms, thus reducing the risk of over-fitting the data.[10][11]

4.4 Survey Data

In addition to our administrative records, we leverage survey data provided by the Finnish Social Science Archive (KELA 2018). These data were collected between October and December 2018, that is, during the last three months of the BI program. Researchers solicited phone interviews with the 2,000 treated BI recipients and with 5,000 randomly selected control participants. The response rate was 28% (Nt=569) in the treatment group and 20% (Nc=1,028) in the control group. The total number of observations was 1,597.

Note that, despite being both drawn from the same population, our survey and registry data are not matched. Consequently, the subsets of BI experimental participants included in each data set intersect but are not identical. To benchmark the results across data sets, we estimate the effect of treatment on self-reported vote intentions in the survey data (Table A16), excluding 42 non-respondents to the vote intention question, and compare the results with our main estimates using registry data (Table 4). Furthermore, in the

[10]The Appendix Figure A3 shows the distribution of the predicted voting propensity both for logit and the Elastic Net logit models

[11]In the Appendix, we also explore treatment effect heterogeneity across three groups computed with the honest causal forest machine learning algorithm by Wager and Athey (2018). This approach repeatedly partitions the sample according to splits by covariates in order to find which splittings lead to consistently larger differences in the treatment effect. The advantage of this method is that we do not need to assume at which dimensions the treatment effect heterogeneity takes place, which may be difficult to theorize ex-ante. Although somewhat underpowered (half of the sample is used for the partition and other half for the estimation), the results (Figure A1) are consistent with our main findings.

Appendix we replicate the survey data analysis with a weighted sample, where weights are constructed by entropy balancing (Hainmueller 2012) in order to match the demographics of the administrative data population. Entropy balancing addresses the concerns related to different response rates in the survey between the treated and the control group, and the different composition of individuals between the survey and the administrative data.

Building on prior work (Kangas et al. 2021), we then estimate the effects of BI treatment on outcome variables gauging potential mediators: trust in parliament (11 point scale); interpersonal trust towards "most people" (11 point scale); feeling of having had the opportunity to influence social issues over the past two years (5 point scale); whether respondents currently experience stress and anxiety (5 point scale). We exclude non-responses when analyzing these survey data. We also examine heterogeneity by baseline vote propensity using the methods discussed above.

5 Main Effects

Table 3 reports the Average Treatment Effect (ATE) of the experiment. The first column indicates that receiving BI increases turnout by 2.9 p.p. in 2017 municipal elections, on average, when no controls are used. As the average turnout for the control group is 36%, this means that receiving the BI treatment results in an 8% increase in voting participation. The following three columns show that the estimate is stable in terms of size and statistical significance ($p<.10$) in order to test the hypothesis that the BI treatment increased turnout, when progressively adding controls. The last column also includes municipality fixed effects. Given that treatment status is randomized, adding controls should result in stable point estimate and somewhat increased precision. This is what we see. Table A10 in the Appendix shows the same specification for 2019 parliamentary elections turnout as an outcome. There, estimated coefficients for the ATE are still positive and non-negligible in magnitude, but smaller and less precise.

Table 3: Average Treatment Effect

	Outcome: Voted in 2017			
	(1)	(2)	(3)	(4)
BI Treatment	0.029^{*}	0.027^{*}	0.028^{*}	0.027^{*}
	(0.017)	(0.016)	(0.015)	(0.015)
Controls	No	Female, Age, Ln Income	Female, Age, Ln Income, SES, Education	Female, Age, Ln Income SES, Education
Municipality FE	No	No	No	Yes
Untreated $\bar{Y}$	0.359	0.359	0.359	0.359
Observations	54,522	54,516	54,516	54,516

Note: *** p<0.01, ** p<0.01 and * p<0.1. Municipality level clustered standard errors in parentheses.

Next in our main analysis, we investigate treatment effect heterogeneity across the three vote propensity groups described in the last section. Table 4 shows important heterogeneity corroborating our theoretical expectations. The first three columns show results for voting propensities estimated by the logit model. While the effect of treatment is large and statistically significant (6.5 p.p., p<.05) among study participants with intermediate baseline vote propensities, we find no statistically significant effect among low and high propensity voters, which comports with our theoretical expectations. When we compare the coefficient for "marginal voters" with the coefficients for "low propensity" and "high propensity" voters, the p-values of the differences are just above the conventional significance level of $p<.05$.[12]

The average levels of turnout among untreated voters in different groups in Table 2 provide a useful benchmark to evaluate the magnitude our treatment effects: Our models indicate that the effect of receiving BI among "marginal voters" (6.5 p.p.) is more than third of the average difference between untreated "marginal voters" and "high propensity" voters (19.1 p.p.). Another way of illustrating the effect size is that the estimated coefficient (6.5 p.p.) is around 24% of the turnout gap between all unemployed (average turnout 36%) and all employed (average turnout 63%[13]) voters in our sample voting areas.[14] This provides strong evidence that BI reduces turnout inequality, on average.

Columns 4 to 6 in Table 4 show results using voting propensities constructed by the Elastic Net logit model described in the previous section. Again the treatment effects for "low propensity" and "high propensity" voters are not statistically different from zero, whereas for "marginal voters" the estimated coefficient (8.4 p.p.) is statistically significant at $p<.001$

[12] In the Appendix Table A12 we repeat our main analysis with bottom 33th, 33th-67th and top 33th percentile splits. The point estimate for the "marginal voters" (7.0 p.p.) is very close to our main result (6.5 p.p.) in Table 4 both in term of size of the coefficient and statistically significance ($p<.05$).

[13] Turnout by employment is computed using Figure 2 and the Appendix Table 2 from Statistics Finland (2017).

[14] These effects also seem large compared to studies conducted in other contexts: Schafer et al. (2021) estimate that the impact of unemployment on voter turnout in Italy is -3 p.p., on average. Studying GOTV in the U.S., Gerber and Green (2000) find that the effect of sending reminders to vote by mail is .5 p.p. and the effect of canvassing in person is 8 p.p.

Table 4: Heterogeneity by Vote Propensity

	Outcome: Voted in 2017					
	Logit			Elastic Net Logit		
	"Low Propensity" Bottom 25% (1)	"Marginal Voters" 25-75% (2)	"High Propensity" Top 25% (3)	"Low Propensity" Bottom 25% (4)	"Marginal Voters" 25-75% (5)	"High Propensity" Top 25% (6)
BI Treatment	-0.011	0.065^{**}	-0.010	-0.026	0.084^{***}	-0.019
	(0.033)	(0.027)	(0.029)	(0.030)	(0.027)	(0.031)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	0.202	0.349	0.540	0.202	0.347	0.539
Observations	13,576	27,153	13,577	13,629	27,258	13,629
	Marginal - Low	Marginal - High	High - Low	Marginal - Low	Marginal - High	High - Low
Differences	0.075^{*}	0.075^{*}	0.000	0.110^{***}	0.102^{**}	0.007
	(0.043)	(0.040)	(0.044)	(0.040)	(0.041)	(0.043)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Municipality level clustered standard errors in parentheses. Controls comprise gender, age, ln of pre-tax income, education groups and SES (profession) groups. Baseline vote propensities are computed using logit model for columns (1) to (3) and an elastic net model for columns 4 to 6. Treatment effects for vote propensity groups are estimated by OLS. See Appendix Table A12 for robustness check splitting sample into three groups of equal size.

level. Also, when estimates of "low propensity" and "high propensity" voters are compared respectively to the coefficient for "marginal voters", the differences are statistically significant at $p<.001$ level for comparison between "marginal voters" and "low propensity" groups and significant at $p<.05$ level for comparison between "marginal voters" and "high propensity" groups. The estimate for "marginal voters" is 1.9 p.p. higher compared to the corresponding estimate from column 2, where ordinary logit was used to create the voting propensity groups. This alternative way of constructing voting propensities thus supports our findings about the heterogeneity of the treatment effect with respect to the marginality of individual voters.

To study effect longevity, we repeat the analysis from Table 4 examining turnout in the 2019 parliamentary elections.[15] In the second column of Table 5, which uses ordinary logit to construct voting propensity groups, we find a 4.9 p.p. effect on turnout among "marginal voters". This effect is statistically significantly different from zero ($p<.05$), and is also discernible from the point estimate among "low propensity voters" ($p<.05$). Using Elastic Net logit to predict voter types, column 5 shows an estimate of 5.2 p.p. ($p<.05$) for "marginal voters", and the difference between "marginal voters" and "high propensity voters" is statistically different from zero ($p<.05$), whereas the p-value for difference between "marginal voters" and "low propensity voters" is just above the $p<.05$ boundary. When

[15] Although average levels of turnout are higher in parliamentary than in municipal elections, the demographics of vote propensity groups are similar – see Table 2 and Appendix Table A15 – which facilitates comparisons across election types.

Table 5: Heterogeneity by Vote Propensity 2019 Parliamentary Elections

	Outcome: Voted in 2019					
	Logit			Elastic Net Logit		
	"Low Propensity" Bottom 25% (1)	"Marginal Voters" 25-75% (2)	"High Propensity" Top 25% (3)	"Low Propensity" Bottom 25% (4)	"Marginal Voters" 25-75% (5)	"High Propensity" Top 25% (6)
BI Treatment	-0.042	0.049**	-0.010	-0.029	0.052**	-0.023
	(0.033)	(0.022)	(0.026)	(0.036)	(0.022)	(0.026)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	0.376	0.512	0.690	0.368	0.510	0.696
Observations	15,605	31,210	15,605	15,989	31,979	15,989
	Marginal	Marginal	High	Marginal	Marginal	High
	- Low	- High	- Low	- Low	- High	- Low
Differences	0.091**	0.059*	0.032	0.081*	0.076**	0.006
	(0.040)	(0.034)	(0.042)	(0.042)	(0.034)	(0.045)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Municipality level clustered standard errors in parentheses. Controls comprise gender, age, ln of pre-tax income, education groups and SES (profession) groups. Baseline vote propensities are computed using logit model for columns 1 to 3 and an elastic net model for columns 4 to 6. Treatment effects for vote propensity groups are estimated by OLS.

looking at 2021 municipality elections from Table A11 in the Appendix, the estimate (3.3 p.p.) using logit is smaller in magnitude and not anymore statistically discernible from zero, although the point estimate is higher than for "low propensity voters" and "high propensity voters" groups. Taken together, our results demonstrate that the experimental effects are persistent, although somewhat diminishing over time.[16]

6 Mechanisms

We now turn to our analysis of possible mechanisms. We begin by exploring whether the effects of BI on voter turnout are driven by more money – e.g., if participants find a new job while continuing to receive BI – or less conditionality – e.g., if participants remain unemployed but receive BI without having to prove that they are actively looking for a job. To examine these different pathways, we compare the effects of BI among individuals with different employment status. Although conditioning treatment effects by post-treatment employment status bears the risk of "post-treatment" bias (Bullock, Green and Ha 2010), it is not likely to happen in our case as Verho, Hämäläinen and Kanninen (2022) find zero effect of BI on employment for 2017, which is our outcome period.

Table 6 shows the effects of BI on turnout among study participants with different post-

[16]The persistence of the effect in the April 2019 national parliamentary election and the June 2021 municipal election also mitigates the concern that our results in the April 2017 municipal election may be driven by treated participants receiving two transfers around January 1, 2017, due to the different timing of unemployment and BI payments (see Experimental Design section).

Table 6: Heterogeneity by Post-Intervention Employment

	Outcome: Voted in 2017			
	>9 Months		12 Months	
	Unemployed	Employed	Unemployed	Employed
	(1)	(2)	(3)	(4)
BI Treatment	0.054**	-0.023	0.049*	-0.046
	(0.026)	(0.051)	(0.029)	(0.074)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	0.344	0.412	0.349	0.417
Observations	16,811	8,388	12,211	5,816
Differences	0.077		0.095	
	(0.058)		(0.079)	

Note: *** p<0.01, ** p<0.01 and * p<0.1. Municipality level clustered standard errors in parentheses. Controls comprise gender, age, ln of pre-tax income and education groups.

treatment employment status. Columns 2 and 4 show that, among those who remained unemployed for more than 9 months or for the whole year in 2017,[17] the treatment effect is 5.5 p.p. and statistically significantly different from zero (p<.05) for the group of who were unemployed more than 9 months. In contrast, columns 1 and 3 show that, among individuals who were employed for over 9 months or for the whole year, the point estimates are negative and not statistically significantly different from zero. When testing the difference between the point estimates among the unemployed and the employed, we find that these differences are not statistically significant at the conventional levels. Yet, it should be noted

[17]Due to data availability issues, we focus on unemployement status after 9 and 12 months even though the 2017 municipal elections were held in April (i.e., after 4 months).

that, compared our main findings, we are underpowered here due to missing data on post-treatment employment status leading to smaller sample sizes. Overall, these results provide suggestive evidence for the hypothesis that the unconditionality of BI benefits – rather than an income effect – is one of the mechanisms behind our results.[18]

Next, we report our models estimating the effects of treatment on possible mediators of voter turnout employing additional survey data. In the Appendix, we benchmark these survey-based results comparing the effect of BI on self-reported vote intentions (Table A16) with our main results based on validated 2017 municipality election turnout (Table 4). Our findings are consistent across both types of data, giving us confidence about using survey data to explore the mechanisms behind our main results using registry data.

Column 1 in Table 7 shows that receiving BI increases trust in parliament by about .5 points ($p<.001$) on an 11-point scale, on average. This comports with the theory that raising trust in political institutions may contribute to increasing voter turnout. We also find evidence of important heterogeneity: while this effect is large and statistically significant among "low propensity" and "marginal voters", there is no significant effect among "high propensity" voters, who have higher levels of social capital at baseline.

Comparing the effects shown in Table 7 with our main effects in Table 4 reveals that BI increases trust in parliament but not actual turnout among "low propensity" voters.

[18]Verho, Hämäläinen and Kanninen (2022) find that treated BI recipients who remained unemployed put about the same effort in seeking employment than those who found a new job, thus suggesting that it is not an increase in leisure time that is driving the turnout effects.

Table 7: Effect of BI Treatment on Trust in Parliament

	Outcome: Trust in Parliament (11 pt. scale)			
	All	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)	(4)
BI Treatment	0.488***	0.801**	0.596***	-0.160
	(0.144)	(0.330)	(0.202)	(0.256)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	4.429	3.485	4.274	5.584
Observations	1,562	361	806	395
		Marginal - Low	Marginal - High	High - Low
Differences		-0.205	0.756**	-0.961**
		(0.387)	(0.326)	(0.417)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Huber-White standard errors in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups.

Table 8: Effect of BI Treatment on Interpersonal Trust

	Outcome: Trust in Other People (11 pt. scale)			
	All	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)	(4)
BI Treatment	0.427***	0.663**	0.461***	0.146
	(0.122)	(0.275)	(0.169)	(0.229)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	6.303	5.648	6.289	6.922
Observations	1,587	366	822	399
		Marginal - Low	Marginal - High	High - Low
Differences		-0.202	0.315	-0.516
		(0.323)	(0.285)	(0.358)

Note: *** $p<0.01$, ** $p<0.01$ and * $p<0.1$. Huber-White standard errors in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups.

However, this discrepancy is consistent with our theoretical framework, which builds on a "threshold" model of voter mobilization (Arceneaux and Nickerson 2009), as illustrated by the large differences in baseline levels of trust in parliament between different types of untreated voters in Table 7. Our results suggest that, after receiving treatment, "never voters" still have relatively low levels of trust in institutions, whereas among "marginal voters" BI increases trust in parliament close to the level of "high propensity" voters.

Table 9: Effect of BI Treatment on Opportunity to Influence Social Issues

	Outcome: Opportunity to Influence Social Issues (5 pt. scale)			
	All	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)	(4)
BI Treatment	0.284***	0.327**	0.320***	0.148
	(0.065)	(0.138)	(0.090)	(0.131)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	2.556	2.291	2.580	2.743
Observations	1,538	359	797	382
		Marginal - Low	Marginal - High	High - Low
Differences		-0.007	0.172	-0.178
		(0.165)	(0.159)	(0.190)

Note: *** $p<0.01$, ** $p<0.01$ and * $p<0.1$. Huber-White standard errors in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups.

Tables 8 and 9 show the effects of treatment on interpersonal trust and the feeling that one can influence social issues, respectively. Again, we find that BI has a positive effect, on average, and that this effect is concentrated among "low propensity" and "marginal voters".

Note, however, that the average differences between types of voters are not statistically discernible from 0, and relatively smaller for interpersonal trust and opportunity to influence social issues compared to trust in parliament. This suggests that the latter likely plays a greater role in driving turnout effects.

We report several additional analyses in the Online Appendix. In Appendix Table A17, we examine evidence about stress as a possible mediator, which has recently been linked with low voting participation among the poor (e.g., Schaub 2021). We find that BI reduces stress, on average. Yet, there are no major differences in baseline levels of stress between "low propensity", "marginal", and "high propensity" voters, and differences in effect sizes are not statistically significant across the three groups. Thus, this mechanism is unlikely to drive the large heterogeneity we find in our main effects.

In Appendix Tables A19, A20 and A21, we replicate the analysis using weights from entropy balancing (Hainmueller 2012) to match the survey data sample to the administrative data sample. This also addresses concerns about different response rates in the survey between treated and control. Among "marginal voters" the coefficient on trust in parliament is statistically significant, whereas for interpersonal trust and influence on social issues it is not. This provides further evidence for the importance of trust in institutions as a proposed mechanism for the positive effect of BI on voter turnout.

7 Conclusion

Whether social policy can increase civic engagement among the poor has important implications for political science and policymaking. We advance the literature leveraging an exceptionally large-scale randomized policy intervention in Finland with individual-level data. Using administrative records, our analysis shows, first, that receiving BI increases turnout among the unemployed. Yet, this effect is heterogeneous. While the impact of BI is large among the "marginal" unemployed who sometimes vote, there is no effect among the unemployed who rarely vote and among those who usually vote. Moreover, we find that the effect is persistent but somewhat diminishing over time after the end of the experiment. Second, utilizing the covariates in our rich individual-level registry data, we find suggestive evidence that it is the unconditionality of BI transfers rather than additional income that is driving our results. Third, using survey data, we find evidence, consistent with our main results, that the turnout effect may be driven by an increase in political efficacy through various mediators including trust in government.

Although the generalizability of our findings is difficult to assess precisely, there are reasons to believe that many of our insights would likely travel to other established democracies. Indeed, the heterogeneity in our main results speaks to a large body of prior theory and empirical findings. Combining registry and survey data, we provide the best available evidence that the interpretive effects of social policy can be consequential. Although concentrated among the marginals, the turnout effect of BI narrows the participation gap between unemployed and employed citizens. In addition, BI somewhat improves the low levels of political efficacy among treated participants who remain unlikely to vote, which also comports with a "threshold model" of political participation (Arceneaux and Nickerson 2009). Yet, when it comes to additional income not affecting turnout, our results could be more context-dependent, and possibly reflect the high levels of social protection and/or taxation in Nordic welfare states. Moreover, one should keep in mind that Finnish politics tend to be comparatively consensual regarding income redistribution, and that voters' reaction to government assistance like BI might be more polarized in other countries such as the U.S. (Anzia, Jares and Malhotra 2021).

Paying close attention to the specific context of the Finnish BI experiment will also be of paramount importance for further work focusing on other outcomes of interest to social scientists. In particular, Verho, Hämäläinen and Kanninen (2022) show that, contrary to some prior expectations, the Finnish BI program had negligible effects on labor supply. To be sure, the primary objective of this policy was increasing employment – rather than increasing turnout. If BI was designed as a GOTV-campaign, it would be a very expensive one. Nevertheless, fostering social inclusion was an explicit goal of the Finnish BI program. Moreover, studying voter turnout often yields insights that are relevant to other important social behaviors (Green, McGrath and Aronow 2013), and lessons from this experiment may also provide guidance to researchers and policymakers interested in BI design in other contexts. First, our results suggest that, even in a high trust society like Finland, an unconditional BI can benefit marginalized low income citizens. Beyond voter turnout, this might positively affect of wide range of social outcomes related to trust, such as saving money and vaccine acceptance (Butler, Giuliano and Guiso 2016). Second, the important heterogeneity in our findings suggests that some aspects of BI policies may be less costly and easier to implement than others – e.g., reducing bureaucratic pressure on the unemployed without necessarily continuing payments after they find a new job – though further work is needed to better distinguish the effects of additional income vs. unconditionality. More generally, our findings support the notion that BI policies may be more efficient when they are targeted rather than "universal" (Hanna and Olken 2018).

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Online Appendix:

Policy Feedback and Civic Engagement: Evidence from the Finnish Basic Income Experiment

Not intended for publication in printed versions

Table of Contents

A Heterogeneity: Honest Causal Random Forest

Figure A1: CATE within Groups - Honest Causal Random Forest

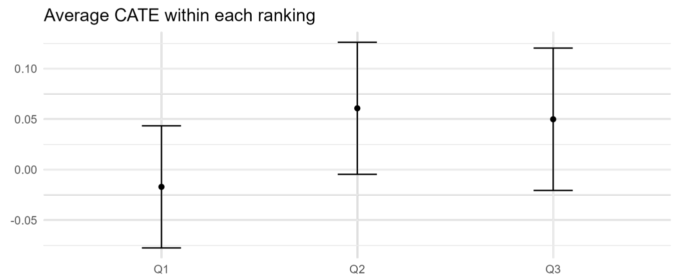


Figure A2: Covariates within Groups - Honest Causal Random Forest

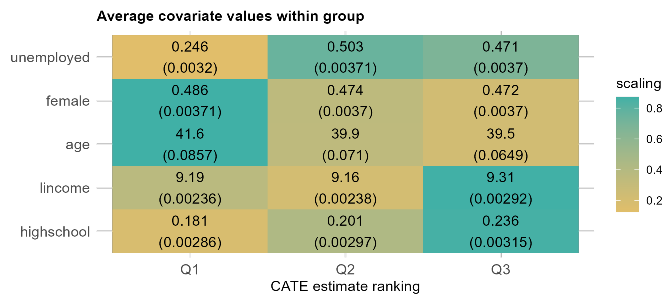


Figure A1 shows the heterogeneity of the treatment effect in three groups computed with honest causal forest machine learning algorithm by (Wager and Athey 2018). This approach explores the heterogeneity of the treatment effect with multi-step procedure in order to avoid over-fitting the data. A random causal forest method partitions sample according to splits by covariates into leafs and estimates conditional average treatment effect in each of these leafs. This splitting procedure is repeated many times in order to find which splittings leads to consistently larger differences in the treatment effect. Honest causal forest separates the splitting and estimation of the conditional average treatment effect by using part of the sample for the former task and another part for the latter. Advantage of this method is that we don't need to assume at which dimensions the treatment effect heterogeneity takes place, which could be difficult to do based on the theory ex-ante. Figure A2, which shows the mean of covariates (scaled as 0.5 being the mean) by conditional average treatment effect groups, illustrates that treatment effect grows in education and unemployment making the these results consistent with our main results, even though honest causal forest results are a little underpowered as they use only half of the sample for the estimation. This provides further evidence for the unconditionality hypothesis.

B Additional Tables and Figures Main Analysis

B.1 Distribution of Predicted Voting

Figure A3: Distribution of Predicted Voting

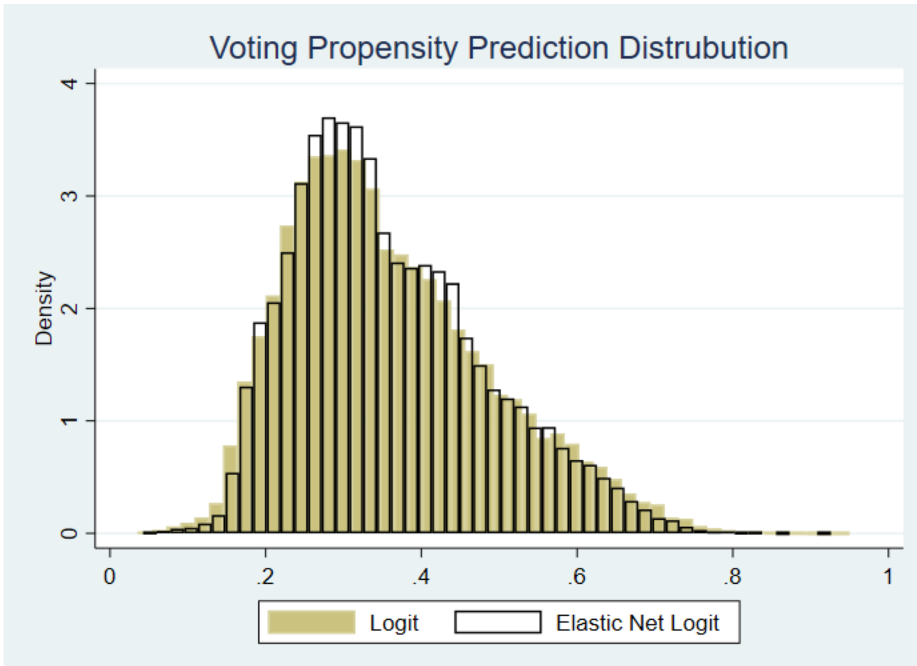


Figure A3 shows the distribution of predicted voting in the 2017 elections computed with the logit and the Elastic Net models using pre-treatment covariates. Notice the smaller variance in estimates using the Elastic Net, which reduces risk of "over-fitting" that data.

B.2 Additional Longevity Analysis and Alternative Vote Propensity Split

Tables A10 and A11 provide additional results supporting our main analysis. Table A10 shows that the average treatment effect in the 2019 national parliamentary elections, though not statistically discernible at conventional levels, was substantively meaningful and robust across specifications. Table A11 shows heterogeneity results for the 2021 municipal elections. While the average treatment effect and the marginal voters coefficients are not statistically significant, coefficient sizes are substantively meaningful and the point estimate is higher compared to "low propensity voters" and "high propensity voters" groups.

Table A12 shows the robustness of our main result for different way of splitting the three voting propensity groups. The estimates for three different voting groups are very similar

Table A10: Average Treatment Effect 2019 Parliamentary Elections

	Outcome: Voted in 2019			
	(1)	(2)	(3)	(4)
BI Treatment	0.020	0.017	0.011	0.011
	(0.017)	(0.017)	(0.016)	(0.016)
Controls	No	Female, Age, Ln Income	Female, Age, Ln Income, SES, Education	Female, Age, Ln Income SES, Education
Municipality FE	No	No	No	Yes
Untreated $\bar{Y}$	0.521	0.521	0.521	0.521
Observations	63,974	63,958	63,958	63,958

Note: *** p<0.01, ** p<0.01 and * p<0.1. Municipality level clustered standard errors in parentheses.

to what we have in our main results in Table 4 both in terms of the coefficients sizes and statistical significance.

B.3 Covariate Balance and Covariates by Vote Propensity 2019 Elections

Table A13 lists balance tests for pre-treatment covariates and voting in 2015 elections. With the exception of annual pre-tax income (372 euros, which is a substantively small difference, significant at the p<.05 level) there are no statistically significant differences at conventional levels. Importantly, there is no evidence that turnout differed between treatment and control in the 2015 election, which was held prior to the BI intervention.

In Tables A14 and A15 we show the demographic covariates by voting propensity groups, where the dependent variable of the logit prediction model is voting in 2019 parliamentary elections rather than 2017 municipality elections as for main analysis. For the latter table, sample is restricted to the same individuals who were in the sample in 2017 municipality elections. As it can be seen the demographics of the parliamentary vote propensity groups are very similar compared to those of the municipality elections in Table 2.

B.4 Benchmarking Survey and Registry Results

We benchmark the survey based results by comparing voting intention ATE and effect among voting propensity groups to our main administrative data results on 2017 municipality election turnout. As it can be seen from column 1 in Table A16, the ATE of BI on voting intention is 2.5 p.p., which is very similar to our comparable main result ATE from Table 3 using administrative data. This is despite the self-reported voting intention untreated group mean is over twice that of the actual comparison group turnout from the administrative data. Similar picture arises when looking the treatment effect on "marginal voters": column 2 in

Table A11: Heterogeneity by Vote Propensity 2021 Municipality Elections

	Outcome: Voted in 2021					
	Logit			Elastic Net Logit		
	"Low Propensity" Bottom 25% (1)	"Marginal Voters" 25-75% (2)	"High Propensity" Top 25% (3)	"Low Propensity" Bottom 25% (4)	"Marginal Voters" 25-75% (5)	"High Propensity" Top 25% (6)
BI Treatment	-0.002	0.033	0.006	-0.008	0.031	0.013
	(0.039)	(0.026)	(0.026)	(0.030)	(0.025)	(0.026)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	0.236	0.332	0.508	0.234	0.330	0.510
Observations	18,346	36,692	18,348	18,993	37,982	18,999
	Marginal - Low	Marginal - High	High - Low	Marginal - Low	Marginal - High	High - Low
Differences	0.034	0.027	0.008	0.039	0.018	0.021
	(0.047)	(0.037)	(0.047)	(0.039)	(0.036)	(0.040)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Municipality level clustered standard errors in parentheses. Controls comprise gender, age, ln of pre-tax income, education groups and SES (profession) groups. Baseline vote propensities are computed using logit model for columns 1 to 3 and an elastic net model for columns 4 to 6. Treatment effects for vote propensity groups are estimated by OLS.

Table A12: Heterogeneity by Vote Propensity - Split By Thirds

	Outcome: Voted in 2017					
	Logit			Elastic Net Logit		
	"Low Propensity" Bottom 33% (1)	"Marginal Voters" 33-67% (2)	"High Propensity" Top 33% (3)	"Low Propensity" Bottom 33% (4)	"Marginal Voters" 33-67% (5)	"High Propensity" Top 33% (6)
BI Treatment	0.009	0.070**	0.000	0.026	0.062	-0.003
	(0.028)	(0.034)	(0.023)	(0.029)	(0.038)	(0.024)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	0.223	0.348	0.513	0.221	0.348	0.511
Observations	17,828	18,369	17,829	17,897	18,441	17,898
	Marginal - Low	Marginal - High	High - Low	Marginal - Low	Marginal - High	High - Low
Differences	0.061	0.070*	-0.009	0.036	0.065	-0.029
	(0.044)	(0.041)	(0.036)	(0.048)	(0.045)	(0.038)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Municipality level clustered standard errors in parentheses. Controls comprise gender, age, ln of pre-tax income, education groups and SES (profession) groups. Baseline vote propensities are computed using logit model for columns 1 to 3 and an elastic net model for columns 4 to 6. Treatment effects for vote propensity groups are estimated by OLS.

Table A13: Pre-treatment Covariate Balance

	(1) All	(2) Control	(3) Treated	(4) Difference
Pre-tax Income	10735.989	10740.460	10368.397	372.063*
	(4150.579)	(4156.782)	(3587.925)	(163.153)
Female	0.477	0.477	0.487	-0.010
	(0.499)	(0.499)	(0.500)	(0.020)
Age	40.306	40.300	40.794	-0.494
	(10.064)	(10.066)	(9.965)	(0.396)
High school	0.206	0.206	0.203	0.003
	(0.404)	(0.405)	(0.403)	(0.016)
Employed < 9 Months	0.091	0.091	0.078	0.013
	(0.288)	(0.288)	(0.268)	(0.011)
Unemployed < 9 Months	0.407	0.406	0.423	-0.016
	(0.491)	(0.491)	(0.494)	(0.019)
Voted in 2015†	0.463	0.463	0.476	-0.013
	(0.499)	(0.499)	(0.500)	(0.027)
Observations	54,522	53,867	655	54,522

Note: *** p<0.01, ** p<0.01 and * p<0.1. Standard deviations in parenthesis except for column 4, where standard errors in parenthesis.†Note that for "Voted in 2015" observations are 29,303 and 340, for control group and treated group respectively.

Table A14: Demographic Covariates by Vote Propensity - 2019 Parliamentary Elections

	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%	All
Pre-Tax Income	9882.02	10775.88	11629.04	10765.70
	(3483.49)	(3900.08)	(5082.06)	(4180.53)
Female	0.38	0.45	0.55	0.46
	(0.49)	(0.50)	(0.50)	(0.50)
Age	33.34	40.71	45.93	40.17
	(7.35)	(9.81)	(9.39)	(10.18)
High school	0.10	0.20	0.48	0.25
	(0.30)	(0.40)	(0.50)	(0.43)
Observations	15,605	31,210	15,605	62,420

Note: Standard deviation in parentheses.

Table A15: Demographic Covariates by Vote Propensity - 2019 Parliamentary Elections - Conditional on Being in 2017 Sample

	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%	All
Pre-Tax Income	9678.97	10783.62	11628.71	10789.43
	(3415.97)	(3877.44)	(5106.22)	(4212.37)
Female	0.37	0.45	0.56	0.46
	(0.498	(0.50)	(0.50)	(0.50)
Age	32.38	40.94	46.19	40.64
	(6.46)	(9.73)	(9.35)	(10.20)
High school	0.06	0.17	0.47	0.23
	(0.23)	(0.38)	(0.50)	(0.42)
Observations	8,496	22,810	11,399	42,705

Note: Standard deviation in parentheses.

Table A16 shows a 7.3 p.p. increase where as column 2 in Table 4 in our main results has a coefficient corresponding to a 6.5 p.p. increase. As these results are so closely matched, it gives us a confidence to trust our survey based results when exploring possible mechanisms.

B.5 Effect of BI on Stress

In Table A17 we examine additional evidence about stress as a possible mediator. We find that BI reduced levels of stress, on average. While this effect might be larger among "low propensity" and "marginal voters" compared to "high propensity" voters, the differences between coefficients are not statistically significant. Moreover, there are no major differences in baseline levels of stress among the three groups. Thus, this mechanism is unlikely to drive the large heterogeneity that we find in our main effects.

B.6 Weighted Survey Sample

In order to address the possible issue of non-response bias for the analysis done with the survey sample, we repeat the analysis with weighting. We employ entropy balancing by Hainmueller (2012). It gives weights for survey observations in order to match the (weighted) means of the covariates to the means of the same covariates from the administrative data. Moreover, the method gives weights which are as close as possible to the original weights (i.e. to 1) and thus avoids discarding observations (i.e. giving weight of 0) if possible. For the balancing we use covariates which are commonly available both in the survey and

Table A16: Effect of BI Treatment on Vote Intentions

	Outcome: Intention to Vote			
	All	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)	(4)
BI Treatment	0.025	-0.041	0.073**	-0.013
	(0.022)	(0.053)	(0.030)	(0.034)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	0.757	0.647	0.737	0.900
Observations	1,552	364	802	386
		Marginal - Low	Marginal - High	High - Low
Differences		0.113*	0.086*	0.027
		(0.061)	(0.045)	(0.063)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Huber-White standard errors in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pre-treatment occupation.

Table A17: Effect of BI on Stress

	Outcome: Currently Feeling Stressed (5 pt. scale)			
	All	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)	(4)
BI Treatment	-0.256***	-0.410***	-0.234***	-0.127
	(0.060)	(0.131)	(0.085)	(0.113)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	2.685	2.786	2.650	2.663
Observations	1,592	368	825	399
		Marginal - Low	Marginal - High	High - Low
Differences		0.176	-0.107	0.284
		(0.156)	(0.141)	(0.173)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Huber-White standard errors in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pre-treatment occupation.

administrative data sets and match the group definitions across the data sets. Namely, covariates for the weighting are: gender, age groups, income groups and education groups.

Table A18 shows that the treatment effects for voting intention of the weighted sample (8.5 p.p. for "marginal voters") is close to what we have for our main results (6.5 p.p. for "marginal voters") with the administrative data sample in Table 4 and similar to unweighted survey sample results (7.3 p.p. for "marginal voters") in Table A16. Also, Table A19 shows that effect on trust in parliament is very similar, both in terms of the point estimates and statistical significance, compared to the unweighted sample in Table 7. Tables A20 and A21 show the treatment effect with weighted sample for trust in other people and influence on social issues respectively. For both outcomes the treatment effect for "marginal voters" is not statistically significant and somewhat smaller compared to the unweighted sample in Tables 8 and 9, albeit the estimates are also more imprecise.

Table A18: Effect of BI Treatment on Vote Intentions - Weighted Sample

	Outcome: Intention to Vote			
	All	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)	(4)
treated	0.041	-0.031	0.085**	0.017
	(0.028)	(0.065)	(0.040)	(0.043)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	0.757	0.635	0.745	0.881
Observations	1,552	340	802	410
		Marginal - Low	Marginal - High	High - Low
Differences		0.116	0.067	0.049
		(0.076)	(0.058)	(0.077)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Huber-White standard errors in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pre-treatment occupation. Weights attained by entropy balancing.

Table A19: Effect of BI Treatment on Trust in Parliament - Weighted Sample

	Outcome: Trust in Parliament (11 pt. scale)			
	All	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)	(4)
treated	0.588***	0.951***	0.525**	0.143
	(0.177)	(0.365)	(0.260)	(0.325)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	4.429	3.456	4.381	5.293
Observations	1,562	338	808	416
		Marginal - Low	Marginal - High	High - Low
Differences		-0.425	0.383	-0.808*
		(0.448)	(0.416)	(0.489)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Huber-White standard errors in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pre-treatment occupation. Weights attained by entropy balancing.

Table A20: Effect of BI Treatment on Interpersonal Trust - Weighted Sample

	Outcome: Trust in Other People (11 pt. scale)			
	All	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)	(4)
treated	0.526***	0.891***	0.345	0.405
	(0.157)	(0.300)	(0.231)	(0.286)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	6.303	5.665	6.299	6.822
Observations	1,587	343	823	421
		Marginal - Low	Marginal - High	High - Low
Differences		-0.546	-0.060	-0.486
		(0.378)	(0.368)	(0.414)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Huber-White standard errors in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pre-treatment occupation. Weights attained by entropy balancing.

Table A21: Effect of BI Treatment on Opportunity to Influence Social Issues - Weighted Sample

	Outcome: Opportunity to Influence Social Issues (5 pt. scale)			
	All	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)	(4)
treated	0.189**	0.232	0.186	0.109
	(0.079)	(0.153)	(0.113)	(0.162)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	2.556	2.332	2.583	2.683
Observations	1,538	339	795	404
		Marginal - Low	Marginal - High	High - Low
Differences		-0.046	0.077	-0.123
		(0.191)	(0.197)	(0.223)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Huber-White standard errors in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pre-treatment occupation. Weights attained by entropy balancing.

B.7 Bootstrapping

Tables A22, A23, A24 and A25 show an alternative way to compute standard errors, namely bootstrapping, for our main text tables with the voting propensity groups with an exception of the elastic net logit models, where for computational reasons calculating bootstrapped standard errors is not feasible. Bootstrapping is done in order to take into account the additional variance caused by the estimation of the voting propensity groups, before estimating the coefficients for those groups. The bootstrapping samples are drawn with replacement and replicated 1000 times. For each sample we first estimate the logit model for voting group classification (as explained in the main text), assign voting groups according to this and then estimate the standard errors for the coefficients of different voting groups. These results show that our main results are robust for this alternative way of estimating standard errors: for the "marginal voters" from the main text Table 4 standard error is 0.027 and corresponding bootstrapped standard error from Table A22 is 0.029 both being statistically significant ($p<0.05$).

Table A22: Heterogeneity by Vote Propensity - Bootstrapped SEs

	Logit Model					
	Outcome: Voted in 2017			Outcome: Voted in 2019		
	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)	(4)	(5)	(6)
BI Treatment	-0.011	0.065^{**}	-0.010	-0.042	0.049^{*}	-0.010
	(0.033)	(0.029)	(0.041)	(0.040)	(0.028)	(0.036)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	0.202	0.349	0.540	0.368	0.510	0.696
Observations	13,576	27,153	13,577 15,605	15,989	31,979	15,989
	Marginal	Marginal	High	Marginal	Marginal	High
	- Low	- High	- Low	- Low	- High	- Low
Differences	0.075^{*}	0.075	0.000 0.091^{*}	0.076	0.006	
	(0.044)	(0.050)	(0.053)	(0.049)	(0.045)	(0.054)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Bootstrapped (1000 replications) standard errors in parentheses. Controls comprise gender, age, ln of pre-tax income, education groups and SES (profession) groups. Baseline vote propensities are computed using logit model. Treatment effects for vote propensity groups are estimated by OLS.

Table A23: Effect of BI Treatment on Trust in Parliament - Bootstrapped SEs

	Outcome: Trust in Parliament (11 pt. scale)		
	"Low Propensity" Bottom 25% (1)	"Marginal Voters" 25-75% (2)	"High Propensity" Top 25% (3)
BI Treatment	0.801^{**}	0.596^{**}	-0.160
	(0.381)	(0.236)	(0.277)
Controls	Yes	Yes	Yes
Untreated $\bar{Y}$	3.485	4.274	5.584
Observations	361	806	395
	Marginal - Low	Marginal - High	High - Low
Differences	-0.205	0.756^{**}	-0.961^{**}
	(0.448)	(0.364)	(0.471)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Bootstrapped standard errors with 1000 replications in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups.

Table A24: Effect of BI Treatment on Interpersonal Trust - Bootstrapped SEs

	Outcome: Trust in Other People (11 pt. scale)		
	"Low Propensity" Bottom 25%	"Marginal Voters" 25-75%	"High Propensity" Top 25%
	(1)	(2)	(3)
BI Treatment	0.663**	0.461**	0.146
	(0.289)	(0.191)	(0.263)
Controls	Yes	Yes	Yes
Untreated $\bar{Y}$	5.648	6.289	6.922
Observations	366	822	399
	Marginal - Low	Marginal - High	High - Low
Differences	-0.202	0.315	-0.516
	(0.347)	(0.325)	(0.391)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Bootstrapped standard errors with 1000 replications in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups.

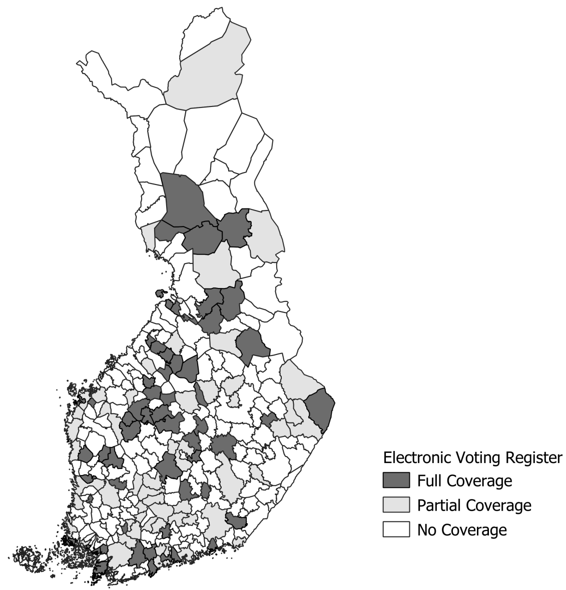
Table A25: Effect of BI Treatment on Opportunity to Influence Social Issues - Bootstrapped SEs

	Outcome: Opportunity to Influence Social Issues (5 pt. scale)		
	"Low Propensity" Bottom 25% (1)	"Marginal Voters" 25-75% (2)	"High Propensity" Top 25% (3)
BI Treatment	0.327**	0.320***	0.148
	(0.144)	(0.101)	(0.139)
Controls	Yes	Yes	Yes
Untreated $\bar{Y}$	2.291	2.580	2.743
Observations	359	797	382
	Marginal - Low	Marginal - High	High - Low
Differences	-0.007	0.172	-0.178
	(0.176)	(0.172)	(0.200)

Note: *** p<0.01, ** p<0.01 and * p<0.1. Bootstrapped standard errors with 1000 replications in parentheses. Controls comprise gender, age groups, education groups and SES (profession) groups.

Geographical Coverage of Electronic Voting Registry

Figure A4: Coverage of Electronic Voting Registry by Municipalities



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