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ARTICLE

Policy feedback and voter turnout: Evidence from the Finnish basic income experiment

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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 (BI) for 2 years (2017–19). Combining individual-level registry and survey data, we show that the intervention has large positive effects on voter turnout. Unconditional BI increases turnout in municipal elections by about 3 percentage points (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 the interpretive effects that follow from unconditionality in the bureaucratic process, including higher levels of political trust and efficacy. We discuss implications for theories of voter turnout and policy feedback, and the design of BI policies.

In Western democracies, citizens with higher socioeconomic status (SES) are more willing to participate in elections (e.g., Schafer et al., 2021). Voter turnout correlates with income and education, a pattern that is well documented in advanced industrial economies including the United States (Leighley & Nagler, 2013), Italy (Schafer et al., 2021), and Finland (Lahtinen et al., 2019). Prior work documents that unequal political participation has important consequences for descriptive (e.g., Lijphart, 1997) and substantive (e.g., Harjunen et al., 2023) representation. In this study, we ask whether social policies can increase voter turnout among the poor.

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 basic income (BI) between January 2017 and December 2018. In both the treatment ($N = 2000$) and the control group ($N = 173{,}222$), participants were initially unemployed and the monthly BI transfers (€560) for the treated were about the same as in control (Kangas et al., 2021). Whereas control participants received unemployment benefits only until they found a new job, and faced possible sanctions if they stopped looking for employment, treated individuals received unconditional BI payments for 2 years. In addition, a majority of treated (and control) participants received means-tested support such as housing benefits (Verho et al., 2022). Note that, while the policy manipulated the bureaucratic process, making conditional cash transfers unconditional for all treated participants, it only increased income among those who found a new job and thus received BI on top of their salary.

Combining theories of political behavior and policy feedback, we discuss reasons to expect that this intervention should stimulate voter turnout among the unemployed. The literature suggests two alternate pathways: a resource channel (e.g., Brady et al., 1995; Schafer et al., 2022) and an interpretive channel (e.g.,

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Jacobs et al., 2022; Soss, 1999). On the one hand, income may in itself boost turnout, especially among relatively poor voters. On the other hand, the unconditionality of BI payments may trigger a bundle of social and psychological effects (Kangas et al., 2021) related to political participation. We discuss how various mediators, including political and interpersonal trust, external and internal efficacy, and physical and mental health may lead to higher turnout. In addition, we theorize that the turnout effect should be heterogeneous among different voting propensity groups. 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 (Arceneaux & Nickerson, 2009; Enos et al., 2014).

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.

The extraordinary scale of this nation-wide field experiment and the high internal validity of our individual-level registry data advance the policy feedback literature (Baicker & Finkelstein, 2019; Campbell, 2002; Clinton & Sances, 2018; Mettler, 2002) as well as related work studying the turnout effects of (un)conditional cash transfers (e.g., Akee et al., 2020; De La O, 2013). Our research design not only avoids measurement error from self-reported voting (e.g., Lahtinen et al., 2019), but also provides unique causal leverage, and allows us to explore the heterogeneous 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 the turnout registry and additional survey data.

The large effect on turnout at the local level seems inconsistent with vote buying (e.g., Gonzalez Ocantos et al., 2014) given that the Finnish BI program was administered at the national level. Furthermore, we find that the turnout effect persists in national parliamentary elections held after the end of the experiment, thus indicating a long-term impact on political behavior. This differs from the ephemeral turnout effect of the Affordable Care Act (ACA) in the United States, which was likely driven by partisan mobilization (Baicker & Finkelstein, 2019; Clinton & Sances, 2018). However, the inverted U-shaped heterogeneity by vote propensity aligns with the policy feedback literature studying major US social programs adopted with bipartisan support after World War 2, such as the Ground Infantry (GI) bill (Mettler, 2002). In the Finnish context, redistributive politics is relatively consensual and being a welfare recipient carries little stigma (Larsen, 2008; Schofield et al., 2022), which facilitates the turnout effect of BI among marginal voters.

Surprisingly, we find that, in our registry data, turnout does not increase among treated participants who find a new job, and thus receive additional income. Instead, our results seem to be driven by those who remain unemployed over the period of study. These results are conditional on finding a new job post-treatment and should be interpreted with caution. Yet, they comport with our survey data indicating that unconditionality in the bureaucratic process boosts turnout through an interpretive channel. While various mediators such as political trust and efficacy also increase among low propensity voters, the actual turnout effect is concentrated among the marginals, which seems consistent with a "threshold" model of political mobilization (Arceneaux & Nickerson, 2009). We discuss the generalizability of our findings and implications for the design and implementation of BI policies.

THEORETICAL BACKGROUND

While the extant literature documents that, in most Western democracies, the rich vote more than the poor (e.g., Kasara & Suryanarayan, 2015), it also provides some evidence that social policies can reduce this gap. In developing countries, government assistance to the poor is often associated with clientelism and "vote buying" (De La O, 2013; Gonzalez Ocantos et al., 2014). This practice is less common in advanced democracies with programmatic parties, though policies may affect electoral politics through "policy feedback" (Campbell, 2002; Clinton & Sances, 2018; Mettler, 2002; Pierson, 1993) and political mobilization (Clinton & Sances, 2018; Rosenstone & Hansen, 1993). To formulate hypotheses about how receiving BI may impact voting participation, we build on a growing literature that blends theories of political behavior and policy feedback, and extend this work theorizing about individual-level heterogeneity and its consequences on turnout inequality.

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

Resource and interpretive effects of social policies

Theoretically, the policy feedback literature suggests two different channels through which redistributive policies like BI may influence turnout. The first mechanism draws on the influential "resource model" of political participation (Brady et al., 1995; Solt, 2008). This approach suggests that affluent citizens vote more because they can simply afford to do more of everything than citizens with little money, including political participation (Campbell, 2002; Rosenstone & Hansen, 1993).

Targeting the unemployed, the Finnish BI program may plausibly affect two important participatory resources: money and time (Brady et al., 1995). Recent work provides evidence that additional income from unconditional cash transfers increases voter turnout in Alaska (Loeffler, 2023) and Brazil (Araújo, 2022). In the Finnish context, BI payments were relatively generous but limited to 2 years from the onset, which may attenuate the income effect. However, if income itself is driving the turnout effect of BI in Finland, then this effect should be concentrated among participants who find a new job.

The effect of BI on time available to participate in politics is unclear. As we discuss below, treated participants who remained unemployed provided about the same effort seeking a new job as untreated participants even though they were not required to report to the unemployment services (Verho et al., 2022), thus suggesting that receiving an unconditional BI did not increase their leisure time. Moreover, the prior literature indicates that the total amount of free time should be less important for voting participation than other time-based inputs such as daily schedules (Brady et al., 1995; Schafer & Holbein, 2020).

The second mechanism pertains to the interpretive effects of social policies. Whereas the voter behavior literature usually focuses on the amount of money that individuals receive from government transfers (Akee et al., 2020; Araújo, 2022; Loeffler, 2023), the policy feedback literature suggests that *how* these payments are made also affects political participation (e.g., Jacobs et al., 2022; Soss, 1999). Yet, the democratic consequences of (un)conditionality are contested in the literature. On the one hand, "paternalistic" theories argue that supervisory welfare programs, in which the government provides directive forms of social support—for example, by requiring that recipients seek employment—should help to integrate citizens into the political sphere (e.g., Mead, 1997). On the other hand, theories focusing on welfare recipients' experiences with the bureaucracy warn that supervisory programs are likely to depress levels of democratic participation because they tend to involve a subordinate role of the client toward the government (e.g., Soss, 1999).

Our study provides a unique opportunity to test how manipulating the bureaucratic process affects voter turnout among low-income citizens receiving government assistance. Prior comparative work suggests that, in advanced economies, welfare recipients' voting participation tends to be higher when transfers are unconditional (Watson, 2015). Using Swedish data, Kumlin and Rothstein (2005) suggest that contacts with unconditional welfare-state programs tend to increase broader levels of civic engagement and social trust, whereas experiences with needs-testing social policies undermine them. Yet, there is a dearth of causal evidence and we know little about mechanisms.

The Finnish BI experiment explicitly aimed to improve the social inclusion of low-income citizens. A prior impact evaluation documents the positive effects of the program on interpersonal and political trust, in particular (Kangas et al., 2021). The increase in interpersonal trust comports with the notion that interpretive effects may operate through a dynamic of reciprocity (Mettler, 2002), which suggests that unconditionality may encourage welfare recipients to adopt more prosocial attitudes and behaviors. Yet, while the relationship between social capital and voter turnout is well documented at the aggregate level (e.g., Putnam et al., 1994), the prior individual-level evidence is more ambiguous. Whereas interpersonal trust does not predict turnout in conditional-on-observable models (Smets & Van Ham, 2013), quasi-experimental work finds a positive relationship (Atkinson & Fowler, 2014). By the same token, there are important theoretical reasons to think that trust in government and voting participation should be linked (e.g., Norris, 2011), but the prior empirical evidence is mixed (Rosenstone & Hansen, 1993; Smets & Van Ham, 2013).

In addition, the (un)conditionality of government assistance may affect levels of external and internal political efficacy (Watson, 2015). Prior work in the US context emphasizes the role of external efficacy—that is, citizens' feeling that the political system listens to people like them—as a driver of political participation (e.g., Campbell, 2002), whereas the role of internal efficacy—that is, the feeling that an individual can impact the political process—depends on whether receiving welfare payments is politically stigmatized. Soss (1999), for example, documents that the negative effects of the income-targeted Aid to Families with Dependent Children (AFDC) program on recipient turnout operate through lowered external efficacy, given that the internal efficacy of AFDC recipients is greater than other citizens, due to their pride over figuring out how to navigate a difficult system, but this heightened internal efficacy does not translate into higher participation. However, studying the consequences of increasing conditionality in various UK welfare programs, Watson (2015) suggests that internal efficacy may also affect political participation. In the Finnish setting, we would expect that both external and internal efficacy impact voter turnout, given that being a welfare recipient carries little stigma (Larsen, 2008; Schofield et al., 2022).[2]

Another plausible pathway through which receiving BI may increase voter turnout is improved physical and mental health (Kangas et al., 2021), which have been recently linked with political participation, particularly so among low-income citizens (e.g., Lahtinen et al., 2017; Schafer & Holbein, 2020; Schaub, 2021). Note that our treatment is bundled and may simultaneously impact multiple intervening variables that affect voting. Moreover, distinguishing between mediators is inherently difficult (Bullock et al., 2010). However, studying the Finnish BI experiment allows us to suggestively explore causal links.

How consequential is policy feedback?

Regardless of the precise mechanisms, the feedback effects of the Finnish BI program will be politically important if they reduce turnout inequality. Yet, the consequences on the composition of the electorate will depend on effect magnitude, heterogeneity, and persistence. Examining the impact of the ACA on the 2014 midterms and the 2016 presidential election, Clinton and Sances (2018) find a large initial turnout effect, which was likely driven by partisan mobilization and voter registration efforts, but did not persist. In our context, voter registration is automatic and the Finnish BI experiment was initially supported by all major parties. As a result, there was no significant partisan mobilization supporting or opposing the program. Moreover, only the treated individuals were informed that they were participating in a BI experiment. The control group participants were not told (Hämäläinen & Verho, 2022), and thus, demobilizing or other behavioral effects among the control group seem unlikely.

However, the feedback effects of BI may vary depending on recipients' socioeconomic background, as suggested by prior work studying major US social programs adopted with bipartisan support after World War 2. Studying policy feedback in the context of the G.I. bill, for example, Mettler (2002), shows that the persistent effect of funding education for veterans is concentrated among individuals with intermediate baseline SES. Among low SES individuals, the effect is not powerful enough to make up for having had a childhood impoverished of factors that lead to civic activity, and high SES individuals would participate either way. Although Mettler (2002) focuses on nonvoting participation, we may expect a similar heterogeneity on voter turnout.

The Get-Out-the-Vote (GOTV) literature suggests that the effects of mobilizing interventions will vary across three types of voters: "always-voters," "marginal voters," and "never-voters" (Arceneaux & Nickerson, 2009; Enos et al., 2014; Fowler, 2015). Mid-propensity or marginal voters tend to be most responsive to GOTV interventions and may thus be most likely to be mobilized by our treatment as well. Note that while this may reduce the turnout gap between marginal and high propensity voters, this may also increase the gap between marginal and low propensity voters. Thus, this heterogeneity may have important consequences on turnout inequality.

Voters' types will likely depend on demographic predictors of turnout like education. Specifically, we theorize that among the unemployed (i.e., in our data), highly educated individuals will tend to be always voters, whereas those who did not finish high school will be never-voters, and those who finished high school but have no university education will be the marginals who are most responsive to treatment. Given that all experimental participants were initially unemployed, we may expect that the proportion of "always-voters" should be smaller than in the entire voting population. However, the exact composition of voters' types is an empirical question.[3]

The GOTV literature indicates that a "threshold" model of mobilization (Arceneaux & Nickerson, 2009) may explain how voters respond to various mediators of turnout. This suggests that mobilizing mechanisms may be most effective among marginal voters who are on the fence about voting. For example, an increase in political efficacy may lift the salience of an election above the "indifference threshold" (Arceneaux & Nickerson, 2009) among marginal voters without achieving the same result among low propensity voters. Note also that the effect of BI on intervening variables will likely be heterogeneous across different vote propensity groups. For instance, receiving BI may have large effects on trust in institutions among low propensity and marginal voters, but it may not have much effect among high propensity voters who are already used to participating in elections. Yet, whereas an increase in trust may mobilize marginal voters, it may not have the same effect on turnout among low propensity voters, who likely have very low levels of trust at baseline.

[2] Watson (2015) suggests that unconditionality also increases political interest, which may lead to higher voter turnout (Brady et al., 1995). However, we do not observe political interest and information in the available data.

[3] We discuss ways to empirically explore this heterogeneity without "overfitting" the data in the Data and Methods section.

Empirically, there is limited causal evidence about the mobilizing effects of policy feedback (Campbell, 2012), despite recent work leveraging quasi-experimental designs. Using a geographic regression discontinuity design, Clinton and Sances (2018) find that Medicaid expansion following the ACA raised county-level turnout in the short term. Focusing on the Oregon Medicaid lottery, Baicker and Finkelstein (2019) find consistent results. Leveraging vote registry data in a difference-in-differences framework, Markovich and White (2022) provide evidence that increasing the minimum wage leads to higher turnout. Our design advances this literature by providing individual-level impact estimates from a randomized experiment accounting for all observed and unobserved confounds, by examining heterogeneity across vote propensities, and by studying two subsequent elections.

Similar caveats apply to prior work on the turnout effects of unconditional cash transfers. 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—that is, changes in income may coincide with other community-level changes that affect turnout, also among non-treated individuals (Arceneaux, 2003). Using individual-level data from Native American communities, Akee et al. (2020) find no effect of unconditional cash transfers 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—a gap that we seek to fill.

Summary hypotheses

To summarize, we expect that BI should increase voter turnout. Prior theory suggests two alternate mechanisms. First, by increasing participatory resources among the poor, income itself may boost turnout. Second, receiving an unconditional BI may have interpretive effects through various channels like political and interpersonal trust, internal and external efficacy, and physical and mental health. In addition, we argue that policy feedback effects should be heterogeneous. Specifically, our theory predicts that the turnout effects should be concentrated among marginal voters, and may persist over time.

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—that is, individuals aged 25–58 claiming unemployment insurance—to receiving unconditional BI transfers in lieu of conditional unemployment benefits over a period of 2 years (January 2017 to December 2018). Compared to other BI programs conducted to date (reviewed in Hanna & Olken, 2018, and Hoynes & Rothstein, 2019), the Finnish experiment stands out in that it was nationwide rather than local, included a very large number of participants (2000 in treatment and 173,222 in control), and involved high monthly payments of €560 (US$615) or about 30% of national median income.[4] Thus, it likely provides an upper bound, if anything, for the BI policies that are practically feasible in advanced industrial economies. The total budget was €20 million, though the net cost (€5.5 million) was lower (Hämäläinen & Verho, 2022; Verho et al., 2022). The difference is due to government incurring net costs from the experiment only if the treated individuals became employed, and the result that 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).

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—that is, about the same level as after-tax unemployment insurance. 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 et al., 2022). Nevertheless, the Finnish experiment reduced the conditionality of government transfers (Kangas et al., 2021).

[4] 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 Alaska Oil Fund fluctuate between US$300 and US$2000 per year (Loeffler, 2023); the annual payouts from Native American casinos are about US$4700 (Akee et al., 2020).

Importantly, the income effect of this intervention varied depending on both treatment assignment and post-treatment occupation (Verho et al., 2022). First, among those who remained unemployed, treated and control participants received roughly 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,[5] 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 unemployment insurance, but would still be eligible for other (means-tested) social benefits such as housing or social assistance (Verho et al., 2022).

Political context

The political circumstances in which this policy was introduced deserve close attention. The Finnish national parliament regularly includes eight political parties, which also dominate municipal politics (Lyytikäinen & 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 has 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 Online Appendix Table A23), 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.[6]

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 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).

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 et al., 2016) and institutional (Kangas et al., 2021) trust are high compared to other advanced economies. Yet, there is also a concern that 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 2 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 program was quite expensive: €20 million budget for 2000 treated participants over 2 years.

Implementation and prior program evaluations

Several aspects of policy implementation should be carefully considered. First, the target population of the experiment ($N = 175{,}222$) consisted of individuals who received minimum unemployment benefits but did

[5] BI payments were discontinued for 137 out of 2000 treated individuals.
[6] https://dvv.fi/en/right-to-vote.

not qualify for higher earnings-related insurance payments.[7] As the experiment was implemented by law, participation was mandatory, leaving aside any problems with noncompliance (Verho et al., 2022). Two thousand eligible individuals were assigned to the BI treatment group using simple randomization, which could raise concerns about spillover effects. However, as the treatment group is much smaller relative to the control group, we can estimate that spillover effects are negligible (Hirvonen et al., 2023).[8]

Second, 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 et al., 2022). Thus, it is reasonable to assume that all treated participants were aware of the treatment. This raises the possibility that treated individuals may have changed their behavior in response to their awareness of being observed. However, such an effect would likely affect all treated participants and observing heterogeneity as predicted by our theory would mitigate concerns about bias. We also note that most control participants were likely uninformed about the experiment, which alleviates concerns about (de)mobilization in reaction to control assignment.

A third concern pertains to how the policy was discontinued. Knowing that the program would not be extended after 2 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 et al., 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. During the first year of the program, 65% of the treated (70% in control) voluntarily prepared an employment plan in a joint meeting with a caseworker. In addition, treated participants spent an average of 97 days (110 in control) participating in active labor market programs, possibly encouraged by the modest additional cash incentives of doing so. Note that the unemployment services were uninformed about treatment status and issued sanction statements to 10% of treated participants, which were however inconsequential, compared to 8% in control, who consequently faced the risk of losing unemployment insurance, though they would still qualify for other (means-tested) social benefits. Overall, Verho et al. (2022) suggest that many treated individuals did not find employment because of insufficient fit with the job market—rather than lack of trying.

Using survey data, another study finds positive effects on institutional and interpersonal trust, the feeling that one can influence social outcomes, physical and mental health, 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.

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.

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,[9] 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 293 municipalities, 59 have a complete and 116 have a partial coverage, leaving us with 655 (out of 2000) treated and 53,867 (out of 173,200) control group individuals.[10] Table 1 presents demographics of municipalities by their electronic voting register status, showing that our sample municipalities are fairly representative of the whole country.[11] 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

[7] Benefits based on prior employment last for up to 400 working days. Afterwards, minimum unemployment benefits can be paid indefinitely but are wealth tested (Verho et al., 2022).

[8] Even with a very high hypothesized spillover effect of 100% (i.e., treatment has an effect for the same number of control group individuals as it has for treated individuals) with our range of point estimates, control group estimates would be affected by a number that is not included in our reported digits (from around .02 p.p. to .06 p.p.).

[9] Oikeusrekisterikeskus (2017), Oikeusrekisterikeskus (2019), and Oikeusrekisterikeskus (2021).

[10] Online Appendix Table A17 shows balance tests for pretreatment covariates and past voting in 2015.

[11] Online Appendix Table A21 shows this for the 2019 election sample.

TABLE 1 Municipality demographics, by electronic voting registry.

	All	100% in election registry	>0% in election registry
Voter turnout	0.616	0.614	0.610
	(0.053)	(0.053)	(0.050)
Population	18,702.102	14,193.661	29,512.457
	(49,632.966)	(31,024.564)	(72,032.928)
Share of tertiary educated	0.230	0.222	0.245
	(0.064)	(0.065)	(0.069)
Employment rate	0.699	0.694	0.702
	(0.056)	(0.053)	(0.055)
Unemployment rate	0.114	0.115	0.112
	(0.036)	(0.034)	(0.034)
Observations	293	59	116

Note: Standard deviation in parentheses. Comparing all municipalities to municipalities with all voting areas and with some voting areas included in 2017 electronic voting registry.

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 Online 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 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.[12] In addition, we observe pretreatment 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.

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 SES; ϵ_i is the error term, errors are clustered at the municipal level.[13] As $Treatment_i$ is randomly assigned, β_1 identifies the causal effect of interest. The purpose of the controls is to reduce residual variation.

Heterogeneity

To examine theoretically important heterogeneity (Hirvonen et al., 2022), 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 SES (profession) measured before the intervention and residential municipality fixed effect.[14]

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, 25th–75th, and

[12] In order to address a concern that results would differ due to change in the sample between 2017 and 2019 elections, we repeat the analysis for 2019 elections using only individuals who were in the 2017 election sample in Online Appendix Table A14. The point estimates correspond to those in Table 5 meaning that results are not explained by changes in the sample composition.

[13] 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.

[14] Errors are clustered at the municipal level. As a robustness check to take into account the uncertainty induced by the "first stage" of predicting the voting propensities, we also compute bootstrapped standard errors. Our results are robust to bootstrapping standard errors and these tables are available from authors upon a request.

TABLE 2 Demographic covariates, by vote propensity.

	Low propensity (bottom 25%)	Marginal voters (25%–75%)	High propensity (top 25%)	All
Pretax income	9,831.541	10,778.135	11,556.323	10,736.049
	(3,457.352)	(3,881.368)	(5,036.786)	(4,151.655)
Female	0.408	0.468	0.565	0.477
	(0.491)	(0.499)	(0.496)	(0.499)
Age	33.013	41.223	45.816	40.319
	(6.781)	(9.622)	(9.429)	(10.066)
High school	0.054	0.166	0.440	0.206
	(0.226)	(0.372)	(0.496)	(0.405)
Observations	13,576	27,153	13,577	54,306

Note: Standard deviation in parentheses. Baseline vote propensities are computed using logit model.

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 sample into three equal groups—that is, in bottom 33th, 33th–67th, and top 33th percentiles (see Online Appendix Table A16).

The primary distinction among the three subsets lies in their levels of 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.[15] 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 pretax income differences between the three groups are relatively small and negligible because all study participants were unemployed at the beginning of the experiment.

This methodology aligns with the conventional approach in GOTV literature for investigating marginality, which involves estimating baseline vote propensities through pretreatment covariates (e.g., Arceneaux & Nickerson, 2009; Enos et al., 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 "overfitting"—that is, 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 & Hastie, 2005, and Hastie et al., 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.[16,17]

Survey data

In addition to our administrative records, we utilize survey data sourced from the Finnish Social Science Archive (Hirvonen et al., 2022; KELA, 2018). These data were gathered between October and December 2018, encompassing the final 3 months of the BI program. Researchers conducted telephone interviews with 2000 BI recipients and 5000 randomly chosen control participants. The response rate was 28% (Nt = 569) among the treatment group and 20%

[15] 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.

[16] Online Appendix Figure A3 shows the distribution of the predicted voting propensity both for logit and the Elastic Net logit models.

[17] In the Online Appendix (Figure A1), we also explore treatment effect heterogeneity across three groups computed with the honest causal forest machine learning algorithm by Wager and Athey (2018) and find consistent results.

(Nc = 1028) among the control group. In total, there were 1597 observations.

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 A25, in the Online Appendix), excluding 45 nonrespondents to the vote intention question, and compare the results with our main estimates using registry data (Table 4). Furthermore, in the Online 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.

Drawing from previous research (Kangas et al., 2021), we proceed to assess the impacts of BI treatment on various outcome variables that serve as potential mediators. These variables include trust in parliament (measured on an 11-point scale), interpersonal trust toward "most people" (also on an 11-point scale), trust in politicians (likewise on an 11-point scale, commonly utilized in the literature to gauge external efficacy; Soss, 1999), perception of having had the opportunity to influence social issues over the past 2 years (measured on a 5-point scale, resembling standard indicators of internal efficacy; Soss, 1999), self-reported general health status (on a 5-point scale), and current experience of stress and anxiety (on a 5-point scale). In our analysis, we standardize the outcome variables from survey data to z-scores to facilitate comparisons across different question items in terms of standard deviations. We exclude nonresponses when analyzing these survey data and explore variations based on baseline vote propensity utilizing the methods previously described.

MAIN EFFECTS

Table 3 reports the average treatment effect (ATE) of the experiment. The initial column shows that receiving BI boosts 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 standard error of the coefficient, when progressively adding controls. The last column also includes municipality fixed effects. Given

TABLE 3 Average treatment effect.

	(1)	(2)	(3)	(4)
Basic income (BI) treatment	.029†	.027†	.028†	.027†
	(.016)	(.016)	(.015)	(.015)
Controls	No	Female	Female	Female
		Age	Age	Age
		Ln income	Ln income	Ln income
			SES	SES
			Education	Education
Municipality FE	No	No	No	Yes
Untreated $\bar{Y}$	.359	.359	.359	.359
Observations	54,522	54,516	54,516	54,516

Note: The outcome is voting in 2017. Municipality-level clustered standard errors in parentheses. Controls comprise gender, age, ln of pretax income, education groups, and socioeconomic status (SES) (profession) groups.
$^{\dagger}p < .10$, $^{*}p < .05$, $^{**}p < .01$.

that treatment status is randomized, adding controls should result in stable point estimate and somewhat increased precision. This is what we see. Table A13 in the Online 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.

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 upper panel shows results for voting propensities estimated by the logit model. Although the impact of treatment is substantial and statistically significant (6.5 p.p.) among study participants with intermediate baseline voting propensities, we do not observe a statistically significant effect among low and high propensity voters, which aligns 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 $p < .05$.[18]

The baseline turnout among untreated voters in various groups, as presented in Table 2 offers a valuable point of reference for assessing the magnitude of our treatment effects. Our models suggest that the impact of receiving BI among marginal voters (6.5 p.p.) exceeds one-third of the average disparity 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

[18] In Online Appendix Table A16, 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 terms of size of the coefficient and statistically significance ($p < .05$).

TABLE 4 Heterogeneity, by vote propensity (2017 municipal elections).

	Low propensity (bottom 25%)	Marginal voters (25%–75%)	High propensity (top 25%)
Logit			
Basic income (BI) treatment	−.011	.065*	−.010
	(.033)	(.027)	(.029)
Controls	Yes	Yes	Yes
Untreated $\bar{Y}$	.202	.349	.540
Observations	13,576	27,153	13,577
Differences	Marginal	Marginal	High
	vs. Low	vs. High	vs. Low
	.075†	.075†	.000
	(.043)	(.040)	(.044)
Elastic net logit			
BI treatment	−.026	.084**	−.019
	(.030)	(.027)	(.031)
Controls	Yes	Yes	Yes
Untreated $\bar{Y}$	.202	.347	.539
Observations	13,629	27,258	13,629
Differences	Marginal	Marginal	High
	vs. Low	vs. High	vs. Low
	.110**	.102*	.007
	(.040)	(.041)	(.043)

Note: The outcome is voting in 2017. Municipality-level clustered standard errors in parentheses. Controls comprise gender, age, ln of pretax income, education groups, and socioeconomic status (SES) (profession) groups. Baseline vote propensities are computed using logit model for the upper panel and an elastic net model for the lower panel. Treatment effects for vote propensity groups are estimated by ordinary least squares (OLS). See Online Appendix Table A16 for robustness check splitting sample into three groups of equal size.

† $p < .10$, * $p < .05$, ** $p < .01$.

24% of the turnout gap between all unemployed (average turnout 36%) and all employed (average turnout 63%) voters in our sample voting areas.[19] This provides strong evidence that BI reduces turnout inequality, on average.

The lower panel in Table 4 shows 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. Also, when estimates of low propensity and high propensity voters are compared, respectively, to the coefficient for marginal voters, the differences are statistically significant for comparison between marginal voters and low propensity groups and 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 estimating 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.[20] In the second column of Table 5, the upper panel, 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 and is also discernible from the point estimate among "low propensity voters." Using Elastic Net logit to predict voter types in the lower panel, the second column shows an estimate of 5.2 p.p. for marginal voters, and the differences between groups also comport with our predictions. When looking at 2021 municipality elections from Table A15 in the Online Appendix, the estimate (3.3 p.p.) using logit is smaller in magnitude and not anymore statistically discernible from

[19] Turnout by employment is computed using figure 2 and Appendix Table 2 from Statistics Finland (2017).

[20] 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 Online Appendix Table A23—which facilitates comparisons across election types.

TABLE 5 Heterogeneity, by vote propensity (2019 national parliamentary elections).

	Low propensity (bottom 25%)	**Marginal voters (25%–75%)**	**High propensity (top 25%)**
Logit			
Basic income (BI) treatment	−.042	.049*	−.010
	(.033)	(.022)	(.026)
Controls	Yes	Yes	Yes
Untreated $\bar{Y}$	.376	.512	.690
Observations	15,605	31,210	15,605
Differences	Marginal vs. Low	Marginal vs. High	High vs. Low
	.091*	.059†	.032
	(.040)	(.034)	(.042)
Elastic net logit			
BI treatment	−.029	.052*	−.023
	(.036)	(.022)	(.026)
Controls	Yes	Yes	Yes
Untreated $\bar{Y}$	.368	.510	.696
Observations	15,989	31,979	15,989
Differences	Marginal vs. Low	Marginal vs. High	High vs. Low
	.081†	.076*	.006
	(.042)	(.034)	(.045)

Note: The outcome is voting in 2019. Municipality-level clustered standard errors in parentheses. Controls comprise gender, age, ln of pretax income, education groups, and socioeconomic status (SES) (profession) groups. Baseline vote propensities are computed using logit model for the upper panel and an elastic net model for the lower panel. Treatment effects for vote propensity groups are estimated by ordinary least squares (OLS).
$^{\dagger}p < .10$, $^{*}p < .05$, $^{**}p < .01$.

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.

MECHANISMS

We now turn to our analysis of possible mechanisms, which builds on a non–peer-reviewed preprint that was previously published online as a policy report (Hirvonen et al., 2022). We start by investigating whether the influence of BI on voter turnout is attributable to increased income—such as if recipients secure employment while still receiving BI—or reduced conditionality—such as if recipients remain unemployed but receive BI without the requirement of proving active job-seeking efforts. To explore these distinct pathways, we analyze the effects of BI among individuals with varying employment statuses. However, conditioning treatment effects by post-treatment employment status carries the potential for "post-treatment" bias (Bullock et al., 2010), it is not likely to happen in our case as Verho et al. (2022) find zero effect of BI on employment for 2017, which is our outcome period.

Table 6 displays the impact estimates of BI on voter turnout among study participants categorized by their post-treatment employment status. Columns 1 and 3 show that, among those who remained unemployed for more than 9 months or for the whole year in 2017,[21] the treatment effect is 5.4 p.p. and statistically significantly different from zero for the group of who were unemployed more than 9 months. In contrast, columns 2 and 4 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 discernible from zero. Yet, it should be noted that, compared to our main findings, we are underpowered here due to missing data on post-treatment employment status leading

[21] Due to data availability issues, we focus on unemployment status after 9 and 12 months even though the 2017 municipal elections were held in April (i.e., after 4 months). Online Appendix Table A24 shows trends in unemployment and income by the treatment status.

TABLE 6 Heterogeneity, by postintervention employment.

	More than 9 months			12 months	
	Unemployed		Employed	Unemployed		Employed
	(1)		(2)	(3)		(4)
Basic income (BI) treatment	.054*		−.023	.049†		−.046
	(.026)		(.051)	(.029)		(.074)
Controls	Yes		Yes	Yes		Yes
Untreated $\bar{Y}$	.344		.412	.349		.417
Observations	16,811		8388	12,211		5816
Differences		.077			.095	
		(.058)			(.079)	

Note: The outcome is voting in 2017 elections. Municipality-level clustered standard errors in parentheses. Controls comprise gender, age, ln of pretax income, education groups, and socioeconomic status (SES) (profession) groups.
† $p < .1$, * $p < .05$, ** $p < .01$.

TABLE 7 Effect of basic income (BI) treatment on trust in parliament.

	All	Low propensity (bottom 25%)	Marginal voters (25%–75%)	High propensity (top 25%)
	(1)	(2)	(3)	(4)
BI treatment	.173**	.284*	.212**	−.057
	(.051)	(.117)	(.072)	(.091)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	−.059	−.395	−.114	.351
Observations	1562	361	806	395
		Marginal vs. Low	Marginal vs. High	High vs. Low
Differences		−.073	.269*	−.341*
		(.137)	(.116)	(.148)

Note: The outcome is standardized trust in parliament. Huber–White standard errors in parentheses. Controls comprise gender, age groups, education groups, and socioeconomic status (SES) (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pretreatment occupation.
† $p < .10$, * $p < .05$, ** $p < .01$.

to smaller sample sizes. In addition, the effect of BI on disposable income may be negligible in some cases—for example, among treated participants who found a new low-income job, but also received housing and child support (Verho et al., 2022). With these caveats in mind, Table 6 provides suggestive evidence for the hypothesis that the unconditionality of BI benefits—rather than an income effect—is one of the mechanisms behind our results.

Next, we estimate the impacts of treatment on potential mediators of voter turnout employing additional survey data. In the Online Appendix, we benchmark these survey-based results comparing the effect of BI on self-reported vote intentions (Table A25) 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.

In Table 7, the first column shows that receiving BI boosts trust in parliament by approximately .17 standard deviations. This aligns with the notion that enhancing trust in political institutions might aid in bolstering voter turnout. Additionally, we observe that this impact is substantial and statistically meaningful among low propensity and marginal voters, but there is no notable effect among high propensity voters, who already exhibit elevated levels of government trust initially.

When comparing the impacts detailed in Table 7 with our primary effects outlined in Table 4, we observe that BI enhances trust in parliament but does not directly influence actual turnout among low propensity voters. However, this inconsistency aligns with our theoretical framework, which operates on a "threshold" model of voter mobilization (Arceneaux & Nickerson, 2009). This is exemplified by the substantial variances in baseline trust levels in parliament

TABLE 8 Effect of basic income (BI) treatment on interpersonal trust.

	All (1)	Low propensity (bottom 25%) (2)	Marginal voters (25%–75%) (3)	High propensity (top 25%) (4)
BI treatment	.175**	.272*	.189**	.060
	(.050)	(.113)	(.070)	(.094)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	−.060	−.328	−.066	.194
Observations	1587	366	822	399
		Marginal vs. Low	Marginal vs. High	High vs. Low
Differences		−.083	.129	−.212
		(.132)	(.117)	(.147)

Note: The outcome is standardized trust in other people. Huber–White standard errors in parentheses. Controls comprise gender, age groups, education groups, and socioeconomic status (SES) (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pretreatment occupation.
$^{\dagger}p < .10$, $^{*}p < .05$, $^{**}p < .01$.

TABLE 9 Effect of basic income (BI) treatment on trust in politicians.

	All (1)	Low propensity (bottom 25%) (2)	Marginal voters (25%–75%) (3)	High propensity (top 25%) (4)
BI treatment	.161**	.180	.234**	−.030
	(.052)	(.112)	(.075)	(.095)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	−.055	−.311	−.092	.247
Observations	1558	360	806	392
		Marginal vs. Low	Marginal vs. High	High vs. Low
Differences		.054	.263*	−.209
		(.135)	(.121)	(.147)

Note: The outcome is standardized trust in politicians. Huber–White standard errors in parentheses. Controls comprise gender, age groups, education groups, and socioeconomic status (SES) (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pretreatment occupation.
$^{\dagger}p < .10$, $^{*}p < .05$, $^{**}p < .01$.

among various categories of untreated voters as shown in Table 7. Our findings indicate that post-treatment, low propensity voters still maintain relatively low levels of institutional trust, whereas BI elevates trust in parliament among marginal voters to a level near that of high propensity voters.

Table 8 presents the impact of BI on interpersonal trust. Once more, we observe a positive average effect, particularly concentrated among low propensity and marginal voters. Yet, it is important to point out that the average disparities between voter types are not statistically significant.

Tables 9 and 10 show the effects of treatment on trust in politicians (measuring external efficacy) and the feeling that one can influence social issues (measuring internal efficacy). For both, we document a positive effect of treatment, and for trust in politicians, we find that the effect is mainly coming from marginal voters, as that group is the only one with a coefficient statistically different from zero and the difference between high propensity group is statistically significant ($p < .05$). For feeling that one can influence social issues, we find, similarly as for trust in parliament and interpersonal trust, that the effect is concentrated among low propensity and marginal voters. However, the differences between voting propensity groups are not statistically different from zero. Overall these results give evidence for political efficacy, both external and internal, being relevant for increasing turnout.

TABLE 10 Effect of basic income (BI) treatment on opportunity to influence social issues.

	All (1)	Low propensity (bottom 25%) (2)	Marginal voters (25%–75%) (3)	High propensity (top 25%) (4)
BI treatment	.230**	.264*	.259**	.120
	(.052)	(.112)	(.073)	(.106)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	−.085	−.299	−.065	.067
Observations	1538	359	797	382
		Marginal vs. Low	Marginal vs. High	High vs. Low
Differences		−.005	.139	−.144
		(.134)	(.128)	(.154)

Note: The outcome is standardized opportunity to influence social issues. Huber–White standard errors in parentheses. Controls comprise gender, age groups, education groups, and socioeconomic status (SES) (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pretreatment occupation.
$^{\dagger}p < .10$, $^{*}p < .05$, $^{**}p < .01$.

TABLE 11 Effect of basic income (BI) on health.

	All (1)	Low propensity (bottom 25%) (2)	Marginal voters (25%–75%) (3)	High propensity (top 25%) (4)
BI treatment	.130**	.140	.142*	.095
	(.050)	(.106)	(.070)	(.100)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	−.054	−.320	−.059	.192
Observations	1587	365	822	400
		Marginal vs. Low	Marginal vs. High	High vs. Low
Differences		.002	.047	−.044
		(.126)	(.122)	(.145)

Note: The outcome is standardized general health. Huber–White standard errors in parentheses. Controls comprise gender, age groups, education groups, and socioeconomic status (SES) (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pretreatment occupation.
$^{\dagger}p < .10$, $^{*}p < .05$, $^{**}p < .01$.

In Tables 11 and 12, we examine additional evidence about plausible mechanisms focusing on subjective general health and stress. Our results show that the treatment increased general health, on average, and the effect is larger among low propensity and marginal voters. Additionally, it is only statistically different from zero ($p < .05$) for marginal voters group. However, the relative difference between "high propensity" voters is smaller compared to the earlier results in this section, indicating that general health is probably not a major mechanism in place. Regarding stress, our findings indicate that BI generally decreased stress levels. Although this effect may be more pronounced among low propensity and marginal voters than among high propensity voters, the differences between coefficients lack statistical significance. Furthermore, baseline stress levels are similar across the three groups. Consequently, this mechanism is improbable to account for the significant heterogeneity observed in our primary effects.

In Online Appendix Tables A27, A28, A29, A30, A31, and A32, 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 and trust in politicians is statistically significant, whereas for interpersonal trust, influence on social issues, general health, and stress, it is not. This provides suggestive evidence for the importance of trust in institutions and external efficacy as proposed mechanisms for the positive effect of BI on voter turnout.

TABLE 12 Effect of basic income (BI) on stress.

	All (1)	Low propensity (bottom 25%) (2)	Marginal voters (25%–75%) (3)	High propensity (top 25%) (4)
BI treatment	−.217**	−.348**	−.198**	−.107
	(.051)	(.111)	(.072)	(.096)
Controls	Yes	Yes	Yes	Yes
Untreated $\bar{Y}$	.072	.158	.043	.053
Observations	1592	368	825	399
		Marginal vs. Low	Marginal vs. High	High vs. Low
Differences		.150	−.091	.241
		(.132)	(.120)	(.147)

Note: The outcome is standardized currently feeling stressed. Huber–White standard errors in parentheses. Controls comprise gender, age groups, education groups, and socioeconomic status (SES) (profession) groups. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pretreatment occupation.
$^{\dagger}p < .10$, $^{*}p < .05$, $^{**}p < .01$.

CONCLUSION

The consequences of social policy design on political participation among the poor have 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 an unconditional BI increases turnout among the unemployed. Yet, while we find a large effect among the type of unemployed who sometimes votes, we find no impact among the type who rarely votes and among the type who usually votes. 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, that is, changes in the bureaucratic process 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 interpretive effects including higher levels of political and interpersonal trust, as well as external and internal efficacy. Our findings also suggest that improved physical and mental well-being may contribute to higher voting participation.

Assessing the generalizability of our findings is challenging. Yet, there are reasons to believe that many of our insights would likely travel to other established democracies. Indeed, the heterogeneity by baseline vote propensity comports with prior theory and empirical findings from other contexts. 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 trust and efficacy among treated participants who remain unlikely to vote, which also comports with a “threshold model” of voter mobilization (Arceneaux & Nickerson, 2009). Yet, when it comes to additional income not affecting turnout, our results may possibly reflect the high levels of social protection and/or taxation in Nordic welfare states, and show the need for further research on interventions that manipulate income directly. Moreover, one should keep in mind that in Finland redistributive politics tends to be comparatively consensual and receiving welfare carries little stigma (Schofield et al., 2022). Although our results comport with the feedback effects of policies like the G.I. bill that have been adopted with bipartisan support in US history (Mettler, 2002), voters’ reaction to government assistance like BI might be more polarized in other contexts such as the contemporary United States (e.g., Anzia et al., 2022).

Taking country context and implementation aspects into account is also important to evaluate other outcomes affected by BI and to develop policy recommendations. In particular, despite some previous expectations, the Finnish BI experiment had minimal impact on labor force participation (Verho et al., 2022). 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 et al., 2013), and lessons from this experiment may also provide guidance to researchers and policymakers interested in BI design in other contexts. First, the important heterogeneity in our findings suggests that some aspects of BI policies may be less costly and easier to implement than others—for example, 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 versus unconditionality. Our findings seem to support the notion that BI policies are more efficient when they are targeted rather than "universal" (Hanna & Olken, 2018), though our data do not directly speak to that debate. Second, 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 et al., 2016).

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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