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The Effect of Unconditional Cash Transfers on Voting Participation:

Evidence from the Finnish Basic Income Experiment.
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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 tends to be higher among citizens with higher levels of income and education (e.g., Leighley and Nagler 2013). This is troublesome because unequal political participation usually leads to unequal representation (Lijphart 1997). The income-turnout relationship seems particularly problematic given that income is the most important demographic with which governments determine 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 unconditional cash transfers, such as Basic Income (BI), can help to break this link.

We address this question focusing on the political consequences of a unique policy experiment in Finland, which randomly assigned 2,000 unemployed to receiving a monthly BI over a period of two years (2017-19). At the beginning of this intervention, both treatment and control participants were unemployed, and the level of BI payments was the same as the basic level of unemployment insurance (Kangas et al. 2021). Yet, unlike the control group, treated individuals continued receiving BI payments even if they found a new job or stopped looking for employment. Thus, the goal of the policy was to make conditional cash transfers unconditional, though a majority of BI recipients remained eligible and applied for means-tested programs such as housing assistance or child support (Verho, Hämäläinen and

Kanninen 2022).

We provide theoretical reasons to expect that this policy should stimulate political engagement among the unemployed. Leveraging representative survey data, we find support for our predictions, showing that receiving BI boosts self-reported vote intentions, an effect that is likely driven by a large increase in trust in government. The effect on vote intentions is concentrated among “marginal voters” with intermediate baseline vote propensities – who, in our data, typically have a high school degree but no university education. At the end of this report, we discuss the evidence about whether the BI treatment simply mobilized voters in response to the policy or had a deeper transformative effect on their behavior. We also point to the limitations of the available data and research design and propose avenues for further research.

2 Theoretical Background: SES and Voter Turnout

To theoretically explain the relationship between income and turnout, the influential “resource model” posits that having a sufficient level of income is a necessary condition to participate in politics (Brady, Verba and Schlozman 1995). 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 empirically 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 focuses on 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 administrative data from Italy to avoid measurement error common in self-reported surveys, and confirm 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 suggests that interventions aiming to improve the economic conditions of low income voters will increase turnout, the magnitude and persistence of this effect are *a priori* less clear. Using multiple sources of survey data from the U.S., Ojeda (2018) argues that there are in fact two income-participation gaps: one reflecting inequality in current income, and another reflecting unequal opportunities in early life. While the former may be malleable in adult life – e.g., after receiving BI – the latter will likely stick over time. Consistently with this theory, Akee et al. (2020) find that unconditional cash transfers related to Native American casinos do not significantly increase turnout among low-income voters in the short-run, but have a positive effect on their children’s participation in the long-run.

A second strand of related work examines how political context moderates the income-turnout relationship. This literature shows that the participation gap between rich and poor is larger when the rich are mobilized to avoid taxation (Kasara and Suryanarayan 2015), and smaller when the poor are mobilized to support redistributive policies (Anderson and Beramendi 2012). Focusing on the Affordable Care Act in the U.S., Clinton and Sances (2018) demonstrate that a policy expanding government transfers to low-income voters led to a large increase in county-level turnout. Although smaller in the following election, this effect persisted over time. However, there is a dearth of causal evidence on how policies affect electoral politics (“policy feedback”).

Another limitation of prior empirical work is the lack of individual-level evidence on who is mobilized to vote, in spite of theoretical reasons to expect strong heterogeneity. In particular, the Get-Out-the-Vote (GOTV) literature, studying the impact of electoral campaigns, suggests that there are three types of voters: “always-voters”, “marginal voters”, and “never-voters” (Arceneaux and Nickerson 2009; Fowler 2015). This body of research indicates 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, which should raise election salience and increase political efficacy through various mediators. To our knowledge, though, no prior work has examined this heterogeneity to study policy feedback.

Marginality is likely a function of demographic predictors of turnout like education and age. Although highly educated individuals often tend to be “always voters”, education may,

conditional on unemployment (i.e., in our sample), be a good predictor of being marginal. For example, BI recipients with who finished high school may be more responsive to treatment, compared to BI recipients who didn't finish high school, because they have better civic skills and higher levels of interest in politics (Sondheimer and Green 2010). Moreover, voting behavior may be more malleable among young voters (18-25) who have not yet completed their formative years (Akee et al. 2020). Yet, it is unclear whether these predictions hold at the bottom of the income distribution. In the context of the Finnish experiment, which targeted the unemployed, we may expect that, compared to the entire voting population, the proportion who always vote should be smaller, if anything, whereas the proportion of marginal voters should be larger. However, who exactly is marginal with respect to BI is an empirical question.

3 Theoretical Background: Possible Mechanisms

Distinguishing between alternate causal mechanisms is always challenging (Bullock, Green and Ha 2010). Yet, we can build on prior work to formulate hypotheses about plausible pathways. In particular, the literature suggests that BI may affect 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. Showing an effect on possible mediators may provide suggestive evidence about whether the effects of BI on voting behavior are transformative or rather ephemeral.

A large literature documents the links between social capital and participation in elections (Putnam, Leonardi and Nanetti 1994; Putnam et al. 2000). The Finnish BI experiment explicitly aimed to improve the social inclusion of low-income citizens (Kangas et al. 2021). Thus, we may expect that receiving BI will increase trust – both interpersonal and towards governmental institutions. This effect should be stronger among marginals and low propensity voters, who should have lower levels of baseline social capital.

While higher trust in institutions implies that BI may raise levels of *external* efficacy – i.e., “an individual’s perceptions about the responsiveness of government officials and institutions to citizen demands” (Niemi, Craig and Mattei 1991) – the literature also suggests a possible role of *internal* efficacy – defined as “what an individual believes about her own ability to understand and participate effectively in politics” (Niemi, Craig and Mattei 1991). Prior work on the consequences of welfare state provision for political participation show that government programs may have demobilizing effects, particularly so if they involve negative experiences with the bureaucracy (Soss 1999). By reducing the bureaucracy involved in receiving payments from the government, BI may increase the feeling of internal efficacy and raise electoral participation, particularly so among low- and mid-propensity voters.

4 Experimental Design, Treatment, and Methods

To empirically estimate the effects of BI on political participation, we leverage a unique experiment conducted by the Finnish government between January 2017 to December 2018. This

policy intervention randomly assigned 2,000 unemployed individuals (about 1% of unemployed in Finland) to receiving partial BI in lieu of unemployment benefits. These monthly cash payments were set at €560 – i.e., the level of basic unemployment insurance. Yet, compared to the control group, BI recipients received this money without any job search requirements and payments continued for a period of two years even if they found a new job (Kangas et al. 2021).

By replacing unemployment insurance with partial BI, this intervention aimed to make conditional cash transfers unconditional. However, several aspects of the policy’s implementation deserve special attention. First, because BI payments were designed to only partially cover their needs, a majority of treated study participants still had to apply for additional means-tested government programs such as housing and social assistance (Verho, Hämäläinen and Kanninen 2022). Second, many BI recipients applied for additional benefits such as child support or sick leave that involved the same conditions as unemployment insurance (Verho, Hämäläinen and Kanninen 2022). As a result, the BI program only partially succeeded in reducing the conditionality of government transfers and likely had heterogeneous effects. In particular, unconditionality affects to some extent all in the treatment group, but income effects concern only those who find a job.

In this report, we employ survey data provided by the Finnish Social Science Archive.¹ These data were collected between October and December 2018, that is, during the last three

¹https://services.fsd.tuni.fi/catalogue/FSD3488?tab=descriptionlang=en&study_language=en

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% ($N_t=569$) in the treatment group and 20% ($N_c=1,028$) in the control group. The total number of observations was 1,597.

The main outcome of interest is whether survey respondents intended to vote in the 2019 parliamentary election. We exclude from the analysis 42 individuals who “can’t say” if they intend to vote.

Building on prior work (Kangas et al. 2021), we also estimate effects on other 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 stress and anxiety (5 point scale). We exclude non-responses when analyzing these secondary outcomes.

We estimate our main effects using the following linear regression model:

$$Y_i = \beta_0 + \beta_1 Treatment_i + \mathbf{X}'_i \boldsymbol{\beta} + \epsilon_i \quad (1)$$

where Y_i indicates whether an individual intends to vote (or the response to the secondary outcomes); β_1 is the estimated treatment effect; \mathbf{X}_i is a vector of controls (described in greater detail below, see Table 1), including age, gender, education, and pre-treatment

occupation;² ϵ_i is the error term, using Huber-White robust standard errors.

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 survey respondents into different groups based on their baseline vote propensity, which we predict using the following logistic regression model:

$$Pr(Y_i = 1|\mathbf{X}_i) = \frac{\exp(\mathbf{X}_b)}{1 + \exp(\mathbf{X}_b)} \quad (2)$$

where $Pr(Y_i = 1|X_i)$ is the predicted probability of intending to vote, given the survey respondent’s age, gender, education, and pre-treatment occupation. These covariates are selected because, although they are measured in the post-treatment survey, they plausibly vary independently from treatment assignment, and are predictive of vote intentions. This is consistent with the standard approach in the GOTV literature, which estimates baseline vote propensities using pre-treatment covariates (Arceneaux and Nickerson 2009).

Table 1 shows descriptive statistics for the different groups in our sample, which were all unemployed at the beginning of the experiment. In our data, age is measured on a 5 point scale (under 30, 30-34, 35-44, 45-54, 55+), education on a 6 point scale (no high school degree, vocational high school, general high school, vocational college, applied university, university), and pre-treatment occupation by 7 categories (self-employed, employee, home-

²Note that all study participants were unemployed when treatment was assigned. However, we control for their occupation prior to November 2016.

Table 1: Demographic Covariates by Vote Propensity

	All (1)	"Low Propensity" Bottom 25% (2)	"Marginal Voters" 25-75% (3)	"High Propensity" Top 25% (4)
Education (6 pt.)	2.967	1.408	2.713	4.917
Age (5 pt.)	4.293	4.543	4.366	4.306
Male	.520	.620	.503	.463
<i>Occupation 2016</i>				
Employed	.250	.113	.266	.323
Unemployed	.415	.687	.403	.187
Not Seeking Work	.190	.095	.190	.279
Other	.145	.084	.140	.211
Observations	1,597	368	827	402

maker, student, unemployed, retired, other), which we regroup here into 4 due to the small number of observations in some cases. In Table 1 we split the sample in 25th-50th-25th percentiles due to our theoretical expectation that, compared to the general population, there should be more “marginal voters” among the unemployed. We report robustness checks with different relative group sizes in Tables B1 and B2 in the Appendix.

The main difference between the three subsample is education: Whereas the average “low propensity” voter didn’t finish high school, the average “marginal voter” has a general high school degree, and the average “high propensity” voter has at least an applied university degree. While there are no large differences in age between the three groups, “low propensity” voters are somewhat more likely to be male. Note also that “low propensity” voters were much less likely to be either employed or not looking for a job in 2016, thus suggesting that

they were more likely to be long-term unemployed.

5 Main Effects: Vote Intention

Table 2 reports the main effects from our empirical analysis. The first column indicates that receiving BI increases vote intentions by 2.3 percentage points (p.p.), on average, though the difference between treatment and control is not statistically significant at conventional levels. The following columns reveal important heterogeneity corroborating our theoretical expectations. While the effect of treatment was large and statistically significant (7.3 p.p., $p < .05$) among study participants with intermediate vote propensities, we find no statistically significant effect among low and high propensity voters. When we compare the coefficient for “marginal voters” with the coefficients for “low propensity” and “high propensity” voters, the differences are significant at the $p < .10$ level.³ 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” (7.3 p.p.) is about half the average difference between untreated “marginal voters” and “high propensity” voters (16.3 p.p.).⁴

³We report alternative specifications of the relative group sizes in Tables B1 and B2 in the Appendix.

⁴These 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 knocking on doors is 8 p.p..

Table 2: 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	.023 (.022)	-.041 (.053)	.073** (.030)	-.013 (.034)
Controls	Yes	Yes	Yes	Yes
Untreated \bar{Y}	.758	.647	.738	.901
Observations	1,555	364	803	388
		Marginal	Marginal	High
		- Low	- High	- Low
Differences		.114* (.061)	.086* (.045)	.028 (.063)

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, two-tailed. Huber-White standard errors in parentheses. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pre-treatment occupation.

6 Mechanisms: Political Efficacy and Social Capital

We now turn to our models estimating the effects of treatment of possible mediators of voter turnout. Column 1 in Table 3 shows that receiving BI increased trust in parliament by about .5 points ($p < .001$) on an 11-point scale, on average. This comports with the theory that an increase in 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.

Table 3: 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	.491*** (.144)	.799*** (.329)	.597*** (.202)	-.153 (.255)
Controls	Yes	Yes	Yes	Yes
Untreated \bar{Y}	4.423	3.485	4.276	5.566
Observations	1,565	361	807	397
		Marginal	Marginal	High
		- Low	- High	- Low
Differences		-.202 (.386)	.750** (.325)	-.952** (.416)

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, two-tailed. Huber-White standard errors in parentheses.

Comparing the effects shown in Table 3 with our main effects in Table 2 reveals that

BI increases trust in parliament but not vote intentions among “low propensity” voters. However, this discrepancy is consistent with our theoretical framework, as illustrated by the large differences in baseline levels of trust in parliament between different types of untreated voters in Table 3. Our results suggest that, after receiving treatment, “never voters” still have relatively low levels trust in institutions, whereas among “marginal voters” BI increases trust in parliament close to the level of “high propensity” voters.

Table 4: 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	.403*** (.122)	.665*** (.170)	.461*** (.202)	.158 (.232)
Controls	Yes	Yes	Yes	Yes
Untreated \bar{Y}	6.302	5.648	6.290	6.915
Observations	1,590	366	823	401
		Marginal	Marginal	High
		- Low	- High	- Low
Differences		-.204 (.264)	.303 (.264)	-.497 (.308)

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, two-tailed. Huber-White standard errors in parentheses.

Tables 4 and 5 show the effects of treatment on interpersonal trust and internal efficacy, 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

Table 5: Effect of BI Treatment on Internal Efficacy

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	.288*** (.065)	.331** (.138)	.320*** (.090)	.158 (.130)
Controls	Yes	Yes	Yes	Yes
Untreated \bar{Y}	2.553	2.291	2.579	2.736
Observations	1,541	359	798	385
Differences		Marginal - Low -.011 (.165)	Marginal - High .162 (.158)	High - Low -.173 (.190)

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, two-tailed. Huber-White standard errors in parentheses.

differences between types of voters are not statistically discernible from 0, and relatively smaller for interpersonal trust and internal efficacy compared to trust in parliament. This indicates that the latter likely plays a greater role in driving turnout effects.

7 Mechanisms: More Money or Less Conditionality?

Table 6: Heterogeneity by Post-Treatment Occupation

Outcome: Intention to Vote				
	Unemployed (1)	Not Seeking Work (2)	Blue Collar (3)	White Collar (4)
BI Treatment	.021 (.036)	-.016 (.062)	.017 (.040)	.075 (.048)
Controls	Yes	Yes	Yes	Yes
Untreated \bar{Y}	.719	.831	.755	.859
Observations	666	187	487	215
	Unemployed - Not Seeking	Unemployed - Blue Collar	Unemployed - White Collar	Not Seeking - Blue Collar
	.037 (.072)	.004 (.054)	-.054 (.060)	.033 (.074)
Differences	Not Seeking - White Collar	Blue Collar - White Collar		
	.091 (.078)	-.058 (.062)		

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, two-tailed. Huber-White standard errors in parentheses.

Another theoretical question with important policy implications is whether the effects of BI on voter turnout are driven by more money – e.g., if individuals find a new job while continuing to receive BI – or less conditionality – e.g., if individuals remain unemployed but

Table 7: Heterogeneity by Post-Treatment Income Source

Outcome: Intention to Vote			
	Only Government Transfers (1)	Other Employment Income (2)	Difference
BI Treatment	.043 (.035)	-.002 (.028)	.045 (.045)
Controls	Yes	Yes	
Untreated \bar{Y}	.706	.810	
Observations	720	835	

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, two-tailed. Huber-White standard errors in parentheses.

receive BI without having to prove that they are actively looking for a job. Here we explore these different pathways by comparing the effects of BI among individuals with different employment status, though these results should be interpreted with caution as they bear the risk of “post-treatment” bias (Bullock, Green and Ha 2010). We gather additional evidence by estimating the effects of treatment on stress, which has recently been linked with low voting participation among the poor (Schaub 2021).

Table 6 shows the effects of BI on vote intentions among study participants with different post-treatment occupation status. Due to the small sample sizes, none of the effects are statistically discernible from 0. Results in column 4 of Table 6 suggest that treatment effects may have been large among participants who found a well-paid white-collar job. However, we cannot reject the null hypothesis, thus suggesting that this finding may be due to chance.

Table 7 examines heterogeneity in the main treatment effects depending on whether study

participants (and their spouses) received all their income from government transfers or had additional employment income. Results are not statistically significant at conventional levels. Thus, it is difficult to draw any conclusions about mechanisms from these results.

Table 8: 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	-.261*** (.060)	-.403*** (.131)	-.235*** (.085)	-.141 (.114)
Controls	Yes	Yes	Yes	Yes
Untreated \bar{Y}	2.686	2.796	2.651	2.668
Observations	1,595	368	826	401
Differences		Marginal - Low .168 (.156)	Marginal - High .094 (.142)	High - Low .262 (.173)

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, two-tailed. Huber-White standard errors in parentheses.

In Table 8 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.

8 Discussion and Directions for Future Research

Our preliminary analysis of the political consequences of BI in Finland using individual-level survey data shows several robust results. First, we find that BI tends to increase self-reported vote intentions, but this effect is heterogeneous. While the impact of BI is large among “marginal voters” with intermediate vote propensities, there is no effect among “low propensity” and “high propensity” voters. Second, we consistently find evidence indicating that this effect may be driven by an increase in political efficacy through various mediators. In particular, BI has a large positive impact on trust in political institutions. In contrast, we do not find that these effects are larger among BI recipients who found a new job over the period of study and thus benefited the most financially.

Together, our findings suggest that BI may have a transformative effect on voter behavior, above and beyond the mobilizing effect of “policy feedback”. Yet, it is difficult to distinguish between alternate mechanisms with the available data and research design. Further research using registry data may provide additional evidence about the persistence of BI effects on turnout over time and across different types of elections – e.g., municipal vs. national parliamentary elections.

Although the generalizability of our findings is hard to assess without comparable empirical evidence from other countries, there are reasons to believe that many of our insights regarding the impact of BI on voter turnout would likely travel to other established democracies. In particular, the heterogeneity in our main results speaks to a large body of prior

theory and empirical findings on voter mobilization. We advance the literature by showing that “marginal voters” are most likely to be affected by policy feedback effects. Our suggestive findings about possible mechanisms should be interpreted with caution, but they also build on the literature about the causes of unequal political participation. Further research leveraging administrative registry data may provide additional insights about how political context moderates the effect of BI on voter turnout. This may lead to “scope conditions” specifying in what circumstances we would expect our theory to hold.

Paying close attention to the specific context of the Finnish BI experiment will also be of paramount importance for future work focusing on other outcomes of interest to social scientists. For example, Verho, Hämäläinen and Kanninen (2022) show that, contrary to some prior expectations, the Finnish BI program had negligible effects on labor supply – possibly in part because of a tightening of job search requirements among recipients of unemployment insurance starting in 2018.⁵

⁵Note that Verho, Hämäläinen and Kanninen (2022) find large discrepancies between self-reported surveys and administrative register data with respect to labor supply in the BI treatment group. However, a preliminary analysis of voter turnout registry data conducted by the authors of this report show effects that are very similar with those presented above.

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9 Appendix: Robustness Checks

In this appendix section, we explore whether our main results reported in Table 2 are robust to alternative specifications of the relative sizes of the three subgroups of interest: “low propensity”, “high propensity”, and “marginal voters”. We begin by reporting in Table B1 regressions that separate voters in three roughly equal groups. Neither the individual coefficients nor the difference between them are significant in this specification. While these results may raise some concerns about the sensitivity our main findings, they also confirm that the theoretically-motivated 25-50-25 split reported in Table 2 does a better job at capturing the hypothesized heterogeneity, plausibly because it reflects important differences in education (see Table 1).

In Table B2, we show results from regressions splitting voters by the 20-50-30 percentiles. This specification is motivated by the idea that there might be few true “never voters” in our sample given the high levels of average vote intentions among “low propensity” voters. Results are consistent with the main results in Table 2. Although the difference between “marginals” and “low propensity” voters becomes greater and the difference between “marginals” and “high propensity” voters becomes smaller, this may plausibly be due to random chance. However, further research may leverage a machine learning approach to better fit the heterogeneity of interest without “over-fitting” the data (Chernozhukov et al. 2018).

Table B1: Effect of BI Treatment on Vote Intentions: 33-33-34 Percentiles

Outcome: Intention to Vote				
	All	"Low Propensity" Bottom 33%	"Marginal Voters" 33-66%	"High Propensity" Top 34%
	(1)	(2)	(3)	(4)
BI Treatment	.023 (.022)	.012 (.044)	.028 (.040)	.032 (.028)
Controls	Yes	Yes	Yes	Yes
Untreated \bar{Y}	.758	.640	.757	.875
Observations	1,555	521	509	525
		Marginal	Marginal	High
		- Low	- High	- Low
Differences		.016 (.059)	-.004 (.049)	.020 (.052)

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, two-tailed. Huber-White standard errors in parentheses. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pre-treatment occupation.

Table B2: Effect of BI Treatment on Vote Intentions: 20-50-30 Percentiles


Outcome: Intention to Vote				
	All	"Low Propensity" Bottom 20%	"Marginal Voters" 20-70%	"High Propensity" Top 30%
	(1)	(2)	(3)	(4)
BI Treatment	.023 (.022)	-.064 (.058)	.075** (.033)	.014 (.030)
Controls	Yes	Yes	Yes	Yes
Untreated \bar{Y}	.758	.641	.726	.890
Observations	1,555	316	764	575
		Marginal	Marginal	High
		- Low	- High	- Low
Differences		.139** (.067)	.061 (.045)	.078 (.065)

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, two-tailed. Huber-White standard errors in parentheses. Baseline vote propensities are computed using a logit model regressing vote intention on gender, age, education, and pre-treatment occupation.

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