Nudging Turnout:

Mere Measurement and Implementation Planning of Intentions to Vote

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Abstract

Randomized experiments, conducted during the 2006 US midterm election and the 2005 German federal election, examined the impact on voter turnout of two simple treatments. The effects of a mere measurement treatment (asking people if they intend to vote) and an implementation intentions treatment (asking people how they intend to vote), were estimated for both one-shot goals (e.g., voting on Election Day) and open-ended goals (e.g., voting early) with deadlines in either days or months in the future. Mere measurement increased voter turnout for open-ended goals and for proximal one-shot goals but not for distant one-shot goals. Implementation intentions increased voter turnout for both open-ended and one-shot goals in the near and long term.

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As electoral participation is of central importance to both governments and political parties, increasing voter turnout is one of the greatest practical and psychological challenges facing policymakers. Since World War II, in over 1,600 national elections in 170 countries, voter turnout has averaged about 65% of the voting age population (Ellis et al., 2006; Pintor & Gratschew, 2002). Of these 170 democracies, 18% have deemed voting important enough to justify laws under which non-voters can face fines and other punishments. The US Congress has recently authorized 3.9 billion dollars for the Help America Vote Act, and state governments have invested in alternative voting methods. Worldwide, governments reward voters with tax breaks, job opportunities, scholarships, and even high-stakes lotteries (Ellis et al., 2006). Alongside government campaigns are partisan ones: US Democrats and Republicans spent roughly 100 million dollars on turnout programs in the 2000 election alone (Dao, 2000).

What drives voter turnout? Political theory speaks of the costs and benefits of voting and the slight probability that one's vote will be decisive (Blais, 2000). In practice, programs focus on low voter motivation or high obstacles to voting (Converse, 1971). Motivation-focused initiatives aim to impart the desire to vote by emphasizing the importance of an election, a sense of duty, rewards, punishments, or social comparisons. Obstacle-focused programs aim to make voting easier, such as by introducing same-day or automatic registration, voting by mail, or early in-person voting. Interestingly, both of these strategies put the burden of increasing turnout on those who want to get others to vote. Could there not also be strategies that put the challenge to the people themselves, helping them use their self-regulation skills? We explore the effectiveness of two such strategies. The *mere-measurement* treatment pertains to simply asking people if they

intend to vote, causing them to reflect on their intentions. The *implementation-intentions* treatment extends to asking people how they intend to cast their vote, thus asking them to plan.

The Mere Measurement Effect

The technique of asking people if they intend to vote comes from research on attitude accessibility and self-fulfilling prediction. In what was termed "the self-erasing error of prediction," Sherman (1980) found that respondents over-predicted how likely they were to engage in socially desirable behaviors (e.g., volunteering for the American Cancer Society). More interestingly, however, these same participants actually changed their subsequent behaviors in the predicted direction. Surprisingly, uttering a response may not even be necessary. Spangenberg et al. (2003) posted the question "Ask Yourself ... Will You Recycle?" at key locations on a university campus, and measured a 75% increase in actual recycling behavior. In a study of 60 college students, Greenwald and colleagues (1987) found early, encouraging results suggesting that asking questions may increase voter turnout. This particular finding may have arisen from mere measurement, but also from the practical information that was given out as part of the experiment (participants were told where and how to register to vote). In what follows, we isolate the mere-measurement effect from that of receiving practical information and go beyond this previous investigation in terms of sample size, sample diversity, types of treatment, and manner of voting studied.

How does the mere measurement effect arise? Being presented with questions may increase the accessibility of attitudes toward a target behavior (Morwitz & Fitzsimons, 2004) and remind respondents of the inconsistency between what they want to do and what they should do, leading them to avoid cognitive dissonance by taking action (e.g., Spangenberg & Greenwald, 1999; Spangenberg, Sprott, Grohmann, and Smith, 2003). Each of these mechanisms predicts the

same outcome: question-asking leads to an increase in positively-viewed behaviors, and a decrease in negatively viewed ones.

The Implementation Intention Effect

The second technique, asking people *how* they intend to vote, comes from research on implementation intentions, that is, if-then plans (Gollwitzer, 1999). To form an implementation intention, one needs to identify a future goal-relevant situational cue (in the if-component) and a related planned response to that cue (in the then-component). Whereas a mere *goal intention* specifies the desired outcome in the form of "I intend to perform Behavior X " (e.g., to intend to vote in the upcoming election), an *implementation intention* specifies both an anticipated goal-relevant situation and a proper goal-directed response Thus, an implementation intention follows the form "If Situation Y arises (e.g., When I close up shop on Election Day, …), then I will perform Behavior Z (e.g., then I will bicycle to the voting location to cast my vote)."

Implementation intentions provide benefits over and above goal intentions: a meta-analysis by Gollwitzer and Sheeran (2006) involving over 8,000 participants in 94 independent studies reported a substantial effect size (Cohen's d = .65), representing the additional facilitation by implementation intentions compared to goal intentions alone. As mere goal intentions have already a facilitating effect (Cohen's d = .36; Webb & Sheeran, 2006), the size of this effect is considerable.

How do the effects of implementation intentions arise? Implementation intentions facilitate goal attainment on the basis of processes that relate to both selecting a critical situational cue (specified in the if-part) and linking it to an intended goal-directed response (specified in the then-part). Selecting a critical situational cue leads to a heightened activation of its mental representation (Gollwitzer, 1999), meaning that people with implementation intentions

are in a good position to identify and take notice of the critical situation when they subsequently encounter it (e.g., Webb & Sheeran, 2004, 2007, in press). Linking the critical situation to an intended goal-directed response in the then-part of the plan leads to automated action initiation in the presence of the critical situation, meaning that action initiation becomes immediate, efficient, and redundant of conscious intent. With implementation intentions (as opposed to with mere goal intentions), people no longer have to deliberate about when and how they should act. Evidence that if-then planners act quickly (Gollwitzer & Brandstätter, 1997, Experiment 3), deal effectively with cognitive demands (Brandstätter, Lengfelder, & Gollwitzer, 2001), and do not need to consciously intend to act at the critical moment (Sheeran, Webb, & Gollwitzer, 2005, Study 2) is consistent with this process assumption.

One-Shot and Open-Ended Goals

Voting can be a "one-shot" goal that can be realized at only one time (e.g., voting on Election Day) or an "open-ended" goal that can be realized on many possible days (e.g., voting by mail). In fact, early-voting methods accounted for roughly 20% of the votes cast in the 2004 US election (United States Election Assistance Commission, 2005). Morwitz, Johnson and Schmittlein (1993) found that mere measurement can have an effect on open-ended goals, such as buying a product, over a period of six months. We investigate whether the attainment of a *distant, one-shot* goal of election-day voting might possibly be influenced by a treatment that occurred months previously.

The Present Research

Through randomized experiments, we estimate the causal effects of mere-measurement and implementation-intention treatments for open-ended and one-shot voter turnout in two national elections: the 2006 US Midterm Election and the 2005 German Federal Election. In the

US study, 1,209 voting-age members of a nationwide research panel took part in a brief survey approximately two months before the election, giving people time to take advantage of open-ended voting opportunities (e.g., postal or early in-person voting). In it, a mere measurement group was asked about intentions to vote, an implementation intentions group was additionally asked to formulate plans to vote, and a control group completed a filler task. The German study, which involved 1,426 people, took place 1 to 4 days before the election, leaving treatments fresh in the minds of participants, but making voting a one-shot goal that must happen on Election Day or not at all. In addition, in both the German and the US settings, we ran a follow-up study to measure turnout (election-day voting and early voting). We make statistical adjustments for non-random dropout in both experiments. In addition, we queried official electoral rolls to further validate US voting.

<center>Study 1: 2006 US Midterm Election Experiment</center>

Participants, Design, and Procedure

The experiment consisted of two phases. The first took place seven to eight weeks before the election of November 7th, 2006, and the second took place one to four days after the election. Participants were voting age adults randomly sampled from a national online research panel and assigned to three groups. A control group, mere measurement (MM) group, and implementation intention (II) group were sent identical emails inviting them to participate in an online study on "decision making" for a one dollar payment in addition to entry in a cash lottery. To improve the efficiency of the resulting causal estimates, we used a matched-pair design where complete randomization of the treatments was conducted within each group of three observations with similar characteristics (Greevy et al., 2004; Imai, et al., 2008) such as years in residence, gender, age, marital status, employment status, annual income, years of education and beyond. A consent

page, completely identical across conditions, was clicked on by 400, 430, and 379 participants in the control, MM and II groups, making the sample of 1,209 we analyze. As expected, the observed covariates are well-balanced between the three groups, for example, differences in gender, marital status, and employment status are not statistically significant.

During the first phase, the control group provided basic demographic information, then completed a five-minute filler task to equalize time. The MM group was identical to the control, except that before the filler task, participants were asked to indicate the strength of voting intentions by rating the following statements from "I agree completely" to "I disagree completely": "I intend to vote in the upcoming US Midterm Election", "I am very committed to voting in the upcoming US Midterm Election," and "It would not take much for me to abandon my goal of voting in the upcoming US Midterm Election."

The II group was the same as the MM group but without the filler task and with the addition of the following three questions, each followed by a text entry box: 1) "If you are not registered to vote, please write a few sentences answering the following questions by listing specific steps. How will you find out about registering? When will you find out about registering? When will you register? Where will you register? How will you register?"', 2) "Listing specific steps, please write a few sentences answering the following questions. How will you vote (in person or by mail)? Where will you look for information on voting? If you vote in person, how will you find out where to vote? When will you find out your voting location? If you will vote by mail, how will you find out about postal voting? When will you find out about postal voting?", and 3) "Listing specific steps, please write a few sentences answering the following questions. If you vote in person, what time of day will you go to vote? Where will you vote? How will you get to your voting location? If you vote by mail, when will you mail your

ballot? Where will you mail it from?" Each of the three items was followed by a question asking for a contingency plan they could enact if obstacles arise, and what they could do to prevent them. People not intending to vote were instructed to answer as if they did intend. At no point in the experiment was the possibility of a follow-up study mentioned.

Voting behavior was validated in two ways. After the election, we checked the names and addresses of the participants against official voting records. In addition, because official records are incomplete, a diary method was used to check in-person voting during the four days after the election. All the original invitees received a seemingly unrelated boilerplate email invitation about a "short research study on memory." In the study, participants were asked to remember, in as much detail as possible, what they did hour by hour on a particular day. The day in question was Tuesday, and no mention was made of it being the midterm election day. Completed diaries were submitted irrevocably online. At this point, participants were asked if they voted in person on Tuesday, voted before Tuesday (by post or early in-person voting), or did not vote. In the analysis phase, respondents were coded as having voted in person if and only if they listed having voted in their diaries. The number of participants who agreed to participate in the second phase in the control, MM, and II groups was 242, 260, and 207, respectively. In this experiment, the possibility of non-random dropout is an important methodological issue and is addressed in our statistical analysis and by our validating votes against the official rolls.

[PLEASE INSERT FIGURE 1 ABOUT HERE]

Results and Discussion

Details on the statistical analysis are provided in the Appendix. As can be seen in Figure 1, for the open-ended goal of early voting, the mere measurement treatment given two months in advance had a moderate positive effect on turnout probability as compared to the control group.

The effect of the implementation intention treatment on early voting was also positive, and even 2.7 percentage points greater. Interestingly, for election-day voting, mere measurement was no longer effective; only implementation intentions enhanced turnout as compared to the control group. It appears that the mere measurement treatment affects one-shot voting behavior only when question and behavior are in close temporal proximity, suggesting such treatments may fade over time. Mere measurement is still effective on open-ended goals like early-voting, possibly because the elevated probability of voting can be converted into a vote on many possible days. This is not the case for the implementation intention treatment. Indeed, the implementation intention treatment positively affected open-ended early voting as well as later one-shot, election-day voting. The latter finding is in line with research on implementation intentions and the execution of health behaviors, where implementation intention effects could be observed even after a period of 3 months (Gollwitzer & Sheeran, 2006).

Validation Against Official Electoral Rolls

We validated votes in the US study for all states which had electoral voter rolls available at the time of analysis. A total of 304 names and addresses were checked against the rolls. The analysis of the validated vote data yields results that are consistent with those of the analysis of self-reported votes. Among those matching the rolls, we obtain a statistically significant difference between the II and control groups ($t = 2.21$, $p = .03$, $p\text{-}rep = .94$) while the difference between the MM and control groups is not statistically significant ($t = .40$, $p = .69$, $p\text{-}rep = .61$), consistent with Figure 1. Even if we assume those voters whose votes failed to be validated did not vote, the results are similar; the difference between the II and control groups is more pronounced than that between the MM and control groups (t-stats $= 1.93$ and 1.55, respectively). Although there was some variation in the proportion of validated votes across treatment and

control groups, the observed difference was not statistically significant using the chi-square test ($p = .16$).

To test whether the mere measurement treatment would have an effect on a proximal, one-shot goal, we look at the German Federal Election in Study 2. We ran both treatments (i.e., mere measurement and implementation intention) one to five days before Election Day. Note that in Study 1, 14% of those who were assigned to the implementation intentions group failed to write plans while only 3% of the mere measurement group declined to provide intentions. In Study 2, we used a simplified implementation intention treatment that was as easy to follow as the mere measurement treatment.

Study 2: 2005 German Federal Election Experiment

Participants, Design, and Procedure

Participants were voting-age adults in two German Web research panels. The experiment consisted of two phases. The first phase ran one to five days before the election of September 18th, 2005, and the second phase took place one to four days after. Panel members were randomly assigned to the control group, mere measurement group, or implementation intention group and sent identical emails inviting them to participate in an online study on "decision making." Consent page Web links in 249, 579, and 586 invitations were clicked on, making the sample of 1,414 we analyze (the latter two treatment conditions were designated in advance to collect more responses in the interest of detecting differences between them). During the first phase, the control group provided basic demographic information. The MM group was identical to the control group except that it was given a yes-no item about intention to vote. The II group was identical to the MM group, except that it was given two additional items. The first asked those intending to vote to list one main obstacle that might prevent them from voting. The

second requested that they write a plan they could use to overcome this obstacle should it arise. In the second phase, in the week after the election, participants were asked whether they voted in person, voted by mail, or did not vote. All respondents were invited to participate after the election, and 204, 485, and 512 individuals consented. Similar to the US election experiment, the possibility of non-random dropout is addressed in the statistical analysis.

[PLEASE INSERT FIGURE 2 ABOUT HERE]

Results and Discussion

In Study 2, both the mere measurement and the implementation intention treatment turned out to increase voter turnout on Election Day (see Figure 2). When the measurement treatment comes just days before the critical event of voting, it shows the expected positive effect on action initiation. This fits nicely with the assumed underlying processes of mere measurement effects: both forces (i.e., heightened accessibility of respective attitudes, experienced discrepancies) can be expected to dissipate over time. Whereas in Study 1 the course of action implied by casting one's vote on Election Day was elaborated by asking respective questions prior to forming implementation intentions, in Study 2 participants only had to name their most crucial obstacle and then form an if-then plan on how to overcome it. Still, the implementation intention treatment increased voter turnout.

Conclusion

This paper has presented a real-world test of mere-measurement and implementation-intention treatments on voter turnout: a behavior of key concern for the health of democracies worldwide. Recently, a number of randomized field experiments have been conducted to investigate the effectiveness of various mobilization methods to increase turnout (see e.g., Green and Gerber 2008 and references therein). Increasingly, researchers are directing their attention to

psychological factors that can engage the electorate (e.g., Gerber, Green, & Shachar, 2003; Gerber, Green, & Larimer, 2008), and this research continues that tradition. By using large, heterogeneous samples of eligible voters, by running the studies in two distant countries, and by validating results through multiple means including the external criterion of official voting records, the basic effect of these simple psychological treatments passes a reasonable test of robustness. The Bayesian confidence intervals afforded by the statistical analysis should provide policymakers useful information about the size and probability of the effects.

From the perspective of the people who are trying to drive turnout, the two studies taken together provide a straightforward suggestion: When the critical event of voting is just around the corner, or when open-ended early-voting options exist, the mere measurement treatment suffices to enhance participation. However, when the critical event is a one-shot future opportunity, an additional implementation intention treatment is advised. From a pragmatic point of view then it appears that using both treatments in tandem is the way to go when one wants to assure that people take on the enactment of the socially desirable behavior of voting.

This work contributes to a growing body of research suggesting that policies might benefit from working in concert with psychological research focusing on self-regulation mechanisms of action control. While some policies benefit from a tendency toward inaction (e.g., people's preference for default options can lead to increased membership in organ donor pools and participation in retirement savings plans (Johnson & Goldstein, 2003; Madrian & Shea, 2001), others must help people to act. To construct effective campaigns to increase voter turnout, policy makers might consider treating voting as a behavioral goal, one that is aided by stating intentions and planning implementation.

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Appendix: Statistical Analysis

We estimate the (sample) average treatment effect (ATE) for MM and II groups separately. We have developed a statistical method to address the issue of non-random dropout in our experiments. The mathematical details of this method are described elsewhere (Imai, 2009), and here we only provide a brief non-technical summary. Note that in addition to this statistical analysis, we address the issue of non-random dropout by collecting validated turnout data from official electoral rolls.

The main advantage of the method we use is that it does not rely on the usual "missing at random" (MAR) assumption. In the context of our experiments, the MAR assumption implies that a voter's decision to report voting behavior in the post-election survey may depend on the treatment he/she received, but the decision is assumed to be independent of whether he/she voted in the election (given the received treatment and observed covariates). This assumption is problematic in our experiments because the existing evidence suggests that those who voted in the election are more likely to report their voting behavior (e.g., Burden, 2000). In contrast, our method allows for the possibility that a voter's decision to answer the post-election survey in our experiments may depend on his/her voting behavior itself by assuming that treatment assignments affect the response decision only indirectly through the voting behavior. This alternative assumption is plausible in our experiments; the filler tasks given to the control and MM groups in the pre-election survey are intended to equalize the survey time, thereby minimizing the possibility of the direct effects of the treatments on the dropout mechanism. Under this non-ignorability (NI) assumption, it can be shown that the average treatment effects are identified.

To estimate the ATE, we conduct a Bayesian inference under the NI assumption. In particular, we use a joint model of turnout and missing data mechanism that consists of two probit regressions with a diffuse independent normal prior distribution on each coefficient. When modeling non-random drop-out, it is important to control for relevant confounding covariates (e.g., Horiuchi et al., 2007). The key variable we use for this purpose is the vote intention from the pre-election survey, which was measured for those in the MM and II groups. We also include an indicator variable for the voters whose vote intention variable was not observed (i.e., those in the MM and II groups who did not answer this question as well as everyone in the control group). These variables are not included in the turnout equation because they constitute a part of the treatments of interest. Furthermore, we include several pre-treatment control variables in both the turnout and non-response equations. For the US experiment, we include gender, age, age squared, education, marital status, number of years in residence, employment status, log income, and an indicator variable about whether any of the pre-treatment covariates is missing. For the German experiment, we include gender, age, age squared, and the missing covariate indicator variable.

Finally, a Markov chain Monte Carlo algorithm is constructed to sample from the posterior distributions. The algorithm is based on a standard Gibbs sampler for the probit regression, but we apply marginal data augmentation to improve its convergence (Imai & van Dyk, 2005). For each analysis, a total of one million draws are obtained and the inference is based on every 10th draw of the second half of the chain. The standard diagnostics tools indicate that a satisfactory degree of convergence is attained.

Author Note

Figure Captions

Figure 1. The figure shows 50% (thick bars) and 95% (thin lines) Bayesian confidence

intervals of the average treatment effect (ATE) as well as the point estimates in the US midterm

election, which are based on the 50,000 Monte Carlo draws from its posterior distribution. For

the US experiment, in which treatments came two months before the election, mere measurement

does not increase the probability of in-person voting (the estimated average treatment effect or

ATE is −0.8% and Pr(ATE > 0%) = 0.38, which is the posterior probability of the positive ATE),

while it has a modest positive effect on the probability of early voting (ATE is 3.2% with

Pr(ATE > 0%) = 0.85). The implementation intention treatment increases the probability of both

in-person voting (ATE is 4.3% and Pr(ATE > 0%) = 0.93) and early voting (ATE is 5.8% and

Pr(ATE > 0%) = 0.97).

Figure 2. For the German experiment, in which treatments immediately preceded the

election, both mere measurement and implementation intention treatments increase the

probability of in-person voting (ATE = 4.0% and Pr(ATE > 0%) = 0.95 for mere measurement;

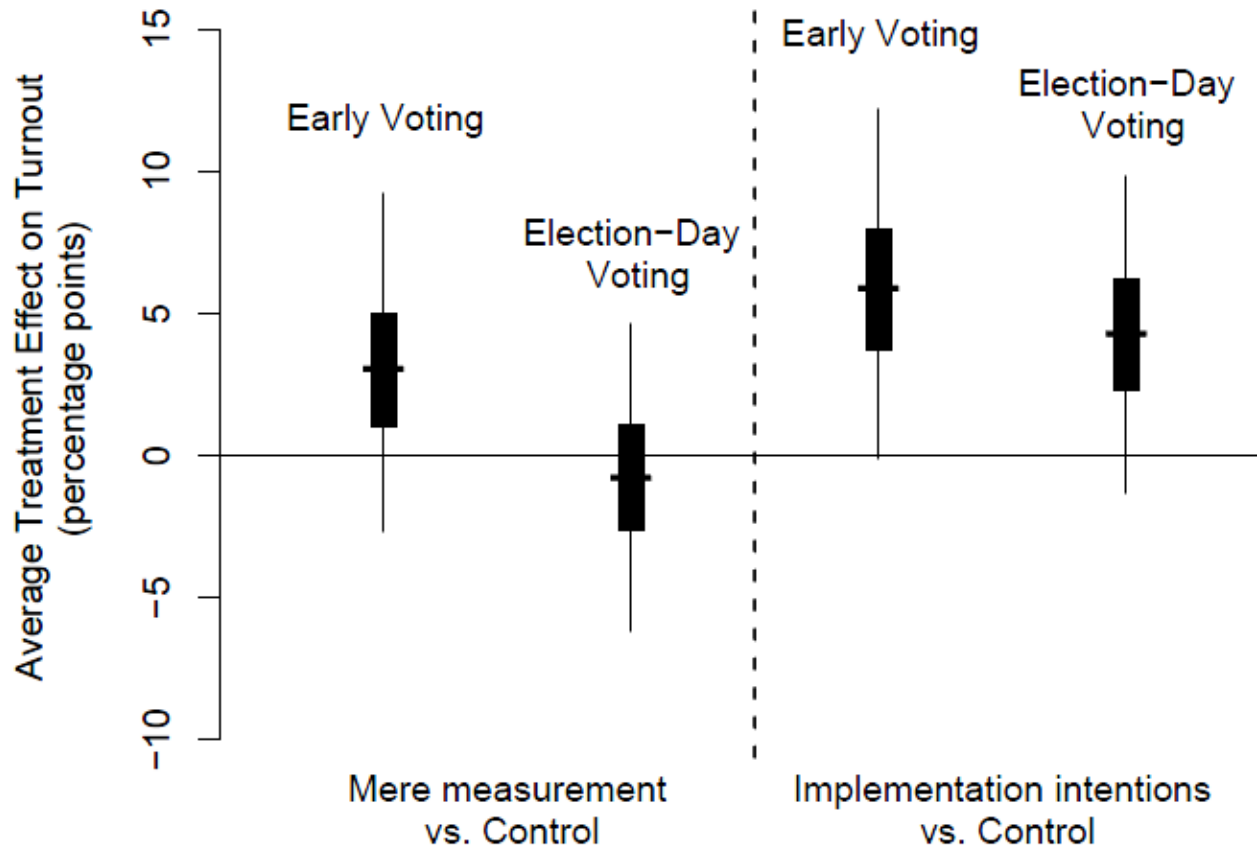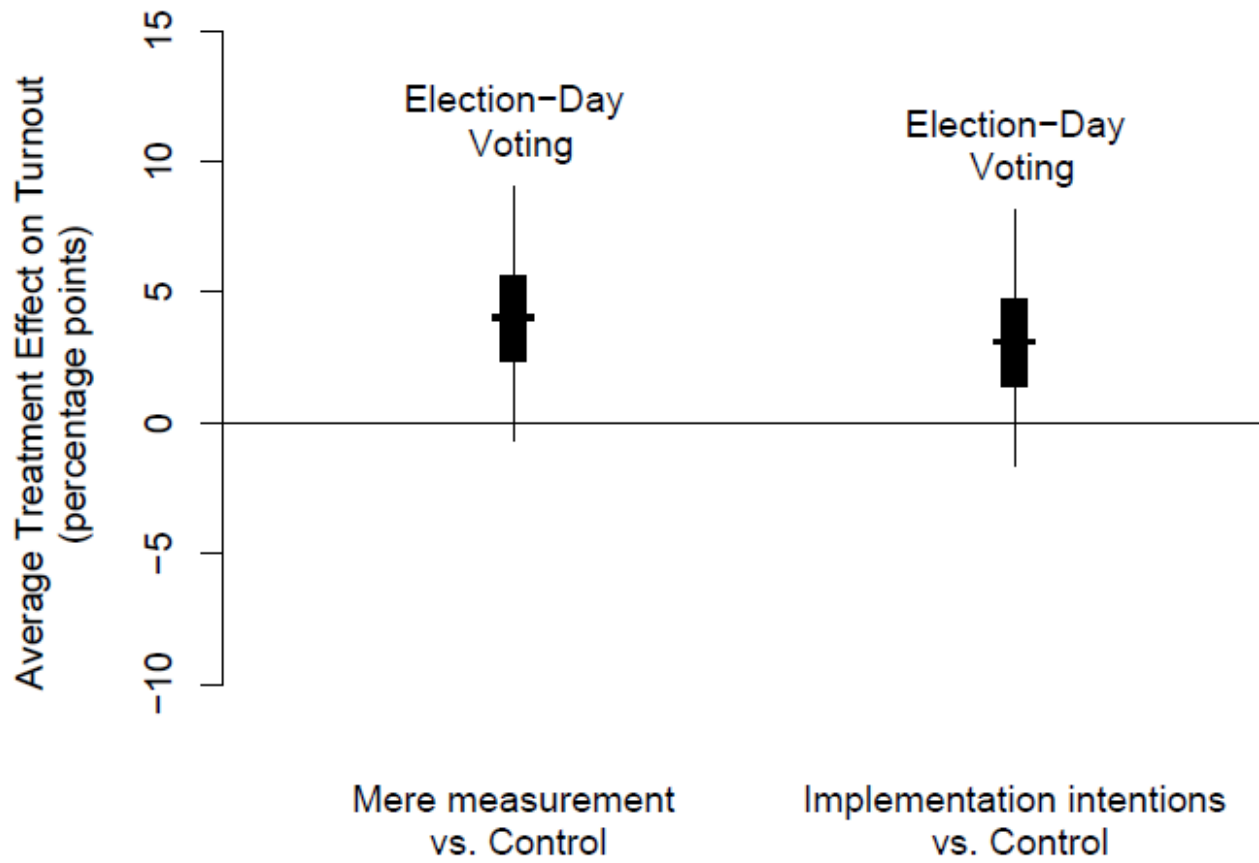ATE = 3.1% with Pr(ATE > 0%) = 0.89 for implementation intentions).

FIGURE 1

FIGURE 2