

**Ballot Secrecy Concerns and Voter Mobilization: New Experimental Evidence about Message Source, Context, and the Duration of Mobilization Effects**

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## **Abstract**

Recent research finds that doubts about the integrity of the secret ballot as an institution persist among the American public. We build on this finding by providing novel field experimental evidence about how information about ballot secrecy protections can increase turnout among registered voters who had not previously voted. First, we show that a private group's mailing designed to address secrecy concerns modestly increased turnout in the highly contested 2012 Wisconsin gubernatorial recall election. Second, we exploit this and an earlier field experiment conducted in Connecticut during the 2010 congressional midterm election season to identify the persistent effects of such messages from both governmental and non-governmental sources. Together, these results provide new evidence about how message source and campaign context affect efforts to mobilize previous non-voters by addressing secrecy concerns, as well as show that attempting to address these beliefs increases long term participation.

How do individuals' beliefs about the voting process affect political participation? Can communication during a campaign change those beliefs? If changes in beliefs affect participation, do those effects endure over time? Recent research suggests that one impediment to greater participation is doubt about ballot secrecy among potential voters. In particular, individuals who have not previously voted are more likely to believe that their vote choices will be revealed and that election officials and others at the polls may seek to intimidate them (Gerber et al. 2013a). Building on this finding, Gerber et al. (2013b) present field experimental evidence that communication from a government source about ballot secrecy protections appears to ameliorate these concerns. In that experiment, treated registrants who had not previously voted participated at higher rates in the 2010 Connecticut midterm election than those not sent this information.

This paper addresses the robustness and significance of that study in two ways. First, we report results from a new field experiment conducted in Wisconsin prior to the 2012 recall election in which Governor Scott Walker retained office. A private nonpartisan and nonprofit voter mobilization organization implemented a randomized mailing campaign that provided assurances about ballot secrecy protections to selected registrants who had not previously voted. We examine whether that intervention increased turnout in the June 2012 recall election and the November 2012 presidential election. Second, we assess whether the initial increase in 2010 turnout associated with the intervention reported by Gerber et al. (2013b) persists over time by examining turnout in the 2012 primary and general elections in Connecticut. In so doing, we both replicate prior research and assess three important questions about the efficacy of voter mobilization efforts.

The first question concerns whether campaign mobilization efforts that attempt to change beliefs can be successful (i.e., persuasive) when undertaken by private (i.e., non-governmental) actors. Whereas the earlier Connecticut study measured the effects of reassurance from a government office, the Wisconsin study explores whether non-governmental actors can successfully reach and persuade individuals despite their lack of inherent credibility as overseers of the election process. Alternatively, do members of the targeted population ignore those private communication efforts or regard them as untrustworthy? The second question relates to context. Whereas Gerber et al. (2013b) examine the effect of providing ballot secrecy assurances on voting in a Congressional midterm election year, the 2012 Wisconsin recall took place after a heated presidential primary, during the course of an ongoing presidential election in a state deemed *ex ante* a “battleground,” and in the face of extensive voter mobilization and advocacy efforts by outside groups. For this reason, it is likely that very few individuals susceptible to traditional mobilization efforts had not already been subject to those appeals. Prior research has found that additional voter mobilization efforts are much less likely to be effective in these more contested and active environments (Green and Gerber 2008; Green et al. 2013). Does such a context undercut the effectiveness of interventions designed to assuage doubts about ballot secrecy?

Third, we use both the new Wisconsin study and the original 2010 Connecticut study to understand whether addressing concerns about ballot secrecy has long-term effects on political participation. If these interventions are successful in increasing rates of voting in the short term by addressing doubts about the voting process, their effects may endure over time by permanently changing those beliefs. Any long-term effect would be consistent with research on the lasting influence of social pressure mobilization efforts (Davenport et al. 2010) and the habit-

forming nature of political participation (Gerber et al. 2003). Alternatively, however, even addressing those beliefs may not be sufficient to generate future participation, or those beliefs themselves may revert to their original (pre-treatment) state. Whether ballot secrecy messages have substantive long-term effects on participation therefore remains an open question.

To briefly summarize our results, we find that non-partisan communication from a private group can be successful in modestly increasing participation, even in the context of a highly contested campaign. In particular, registrants sent a single piece of mail addressing doubts about ballot secrecy were about 1 percentage point more likely to vote in the 2012 Wisconsin recall than those in an untreated control group (whose base voting rate was about 13%). Additionally, some of this increase in turnout appears to persist over time. In Wisconsin, we estimate that turnout in the November 2012 general election was half a point higher in the treatment group than in an untreated control group (although this result is not statistically significant, with a minimum p-value of .18), a result that suggests roughly half of the original treatment effect persists about five months later (and compares well with the typical effects of non-partisan mail in presidential elections; see Green and Gerber 2008). Further, in following up on Gerber et al.'s (2013b) original analysis of the Connecticut experiment, we find that turnout in the 2012 (non-presidential) primary is about 1.1 points higher among those sent a treatment letter addressing ballot secrecy issues in 2010 rather than a placebo letter ( $p < .01$ , one-tailed), and about 1.6 points higher ( $p = .15$ , one-tailed) in the general election. Relative to an initial increase in November 2010 turnout of 3.9 percentage points, this implies that between 28 and 41% of the original effect persists two years later. Both the Wisconsin and Connecticut results suggest that if the initial increase in turnout takes place because of belief changes, such beliefs are sustained over time and that altering them affects future turnout.

The remainder of this paper is organized as follows. We begin by reviewing the prior literature on addressing beliefs about political institutions and their effects on turnout, focusing our attention on recent work concerning ballot secrecy. We also discuss the literature on the persuasiveness of mobilization communication by private actors, the influences of campaign context on mobilization effectiveness, and the relative persistence of patterns of turnout over time. We then present the design and results of the new experiment performed during the 2012 Wisconsin recall election, as well as new analyses of the study reported in Gerber et al. (2013b). Finally, we discuss the implications of our findings and conclude.

### *Political Beliefs, Participation, and Persuasion*

There is an extensive literature detailing the motivations for, and barriers to, individual political participation. Of most relevance for this paper is work examining how beliefs about political institutions affect participation. Recent research (Gerber et al. 2013a; Gerber et al. 2013b; Grose and Russell 2008; Karpowitz et al. 2011) documents an association between doubts about the secrecy and integrity of the voting process and political participation. If such doubts raise the expected costs of voting, they may deter individuals from turning out. In light of these findings, recent field experimental research has sought to understand whether providing information designed to alleviate these concerns can thereby increase political participation.

In particular, Gerber et al. (2013b) report results from a field experiment conducted in Connecticut in 2010. In that experiment, the researchers worked with the Connecticut Secretary of State to send a randomly selected sample of registrants who had not previously voted a letter addressing doubts about ballot secrecy.<sup>1</sup> Compared to a group also sent a similar placebo letter that did not address ballot secrecy fears, they found that being sent a treatment letter increased participation by 3.9 percentage points in the 2010 general election, a proportional increase in

participation of about 17%.<sup>2</sup> This research thereby finds that communication from a government source addressing doubts about ballot secrecy can substantially increase participation in proximate elections. (At the same time, they report no effect of this intervention on the turnout behavior of those who had previously voted, consistent with survey data [Gerber et al. 2013a] showing that concerns about ballot secrecy are far more common among those who have never voted.)

Nonetheless, several important theoretical and empirical questions are not addressed by this work. The first concerns source credibility. Would communication from different sources, including private actors engaged in trying to shape the electorate and voting patterns, be similarly effective? The second is about context. The 2010 election in Connecticut featured one closely contested race (for governor), but the statewide Senate race was decided by more than 10 percentage points, and of the five House races in the state, only one was decided by fewer than 8 points. Would communication be similarly effective in a more saturated environment where more groups were active in trying to mobilize citizens? Finally, is addressing doubts about ballot secrecy effective in altering long-term patterns of political participation? We discuss prior research in these three areas below.

### *Source Credibility*

Scholars have long noted the importance of source credibility for the effectiveness of political and psychological manipulations, including efforts at persuasion (Howland and Weiss 1951-52), priming (Miller and Krosnick 2000), and framing (Druckman 2001). Similarly, Lupia (1994; see also Lupia and McCubbins 1998) notes that when an information provider or source is viewed as credible, individuals will rely on messages sent by that source to facilitate political action and decisions, whereas similar messages from less credible sources will not affect beliefs.

In the context of campaign mobilization efforts, Malhotra et al. (2012) contend that a messenger's identity affects whether such efforts spur political participation because of source credibility. The authors find that an election mobilization email message from an official source (in this case a county registrar) increased turnout modestly, while identical messages sent from a fictitious voter mobilization group had no effect on voting rates. They argue that the official's messages were effective because participants trusted the authoritative government source.<sup>3</sup> Such a difference could arise either because individuals were more likely to ignore the message from a non-government source or because, even if they read the non-official message, they were less persuaded by it. In the specific case of attempting to address beliefs about the government-supervised process of voting, one might therefore reasonably assume that government sources are also likely to be more effective than non-government sources.

At the same time, most GOTV efforts, and most experimental evaluations, concern non-governmental private communication efforts, either by campaigns, allied groups, or non-partisan voter mobilization organizations (see Green and Gerber 2008 for a review). In general, those efforts have consistently found positive but small effects of many forms of communication. Additionally, Malhotra et al.'s (2012) study uses a contacting strategy (email) that research consistently demonstrates exerts no effect when sent from a non-governmental source, regardless of the message (Green and Gerber 2008; Nickerson 2007). Overall, prior research suggests that while governmental contact should be more effective than non-official efforts at persuasion, it is unclear how much less effective private communication will be, particularly when that communication relates to election administration.



### *Campaign Context*

In addition to concerns about source credibility, a distinct theoretical question is whether persuasive efforts will be similarly effective in highly contested electoral contexts. Most published work that documents successful mobilizations efforts, regardless of the strategy employed or medium used to deliver the message, involves low salience contests such as local or special elections, primaries, or attorneys general races (see, e.g., Gerber et al. 2008; Green et al. 2003; Nickerson 2008).<sup>4</sup> But such efforts may be less effective when conducted in highly competitive contests because other actors are also trying to mobilize eligible citizens, thereby “treating” both members of the control and treatment groups (Green and Gerber 2008, 38-39; Green et al. 2013). Furthermore, in these highly competitive contexts, citizens may already be aware of expectations concerning participation or may choose to ignore additional campaign communications.

At the same time that a more robust campaign environment is likely to increase the chances that most registrants receive a standard mobilization message, the content of those messages may be irrelevant for citizens who hold doubts about the voting process. If prior work is correct, it is not that these individuals are unaware of an election or feel less civic duty to vote, but instead that they have beliefs about the voting process that cause them to avoid it. If this is the case, a more robust campaign environment may do little to reduce the efficacy of messaging intended to address beliefs about ballot secrecy, even if any given message is less likely to be received by its target due to the overall volume of campaign communication.

### *Persistence*

The final question raised by existing work concerns the persistence of political participation. Gerber, Green, and Shachar (2003) provide field experimental evidence that

standard mobilization efforts appear to persist across multiple elections. Specifically, about 45% of the canvassing mobilization effect found for a November general election persists to the next year's local election. Examining six published social pressure field experiments, Davenport et al. (2010) similarly identify persistence effects one to two years after the initial intervention in the range of one-third to one-half their initial influence. Meredith (2009) exploits the quasi-experiment created by differences in the age at which an individual experiences the first presidential election in which they can vote and finds that voting in one presidential election increases the chances of doing so in subsequent elections by about 5 percentage points. What is uncertain in this literature is the mechanism by which initial increases in turnout are sustained over time (Allcott and Rogers 2012; Coppock and Green 2013). And, because the mechanism is unclear, it remains an open question as to how efforts designed to change beliefs about political institutions would affect subsequent participation.

Recent work notes that interventions like social pressure messages may have particularly long-lasting effects because they alter individuals' beliefs about the public nature of their turnout behavior in the face of a potentially strong social norm to participate. This mechanism is in addition to persistence effects that operate through habit formation (Aldrich et al. 2011; Coppock and Green 2013; Cutts et al. 2009; Gerber et al. 2003) or learning (Allcott and Rogers 2012). Furthermore, establishing a record of turnout may increase the likelihood of being a target of mobilization efforts by parties, candidates, and advocacy organizations in subsequent election cycles (Gerber et al. 2003). Ballot secrecy messages are distinct from social pressure messages, however, in that they seek to address beliefs about a key institution of the voting process itself. On the one hand, if beliefs about the voting process are an impediment to participation, altering those beliefs may increase participation in the long run because individuals may now view voting

as less onerous and be more amenable to other forms of campaign mobilization activity. On the other hand, new information may not alter long-term attitudes, as individuals may revert to their prior beliefs. Furthermore, beliefs about the voting process are just one of many potential barriers to becoming more engaged over the long-term, and individuals who hold those beliefs about the voting process may also be generally resistant to the mobilization activities of other actors. Testing for persistence is therefore important for understanding whether and how effectively these types of messages affect beliefs.

Overall, prior literature suggests that questions about the importance of source credibility, the effectiveness of efforts to change beliefs in more competitive electoral contexts, and whether the effects of those interventions persist over time, are unanswered. In light of this uncertainty, we now turn to describing the design and analysis of two experiments, each of which sheds light on one or more of these questions.

#### *Experiment #1: Message from a Private Actor during the 2012 Wisconsin Recall Election*

The 2012 recall election in Wisconsin provides an opportunity to extend our understanding of how providing assurances about ballot secrecy mobilizes previous non-voters. In this case, we can examine both the short- and long-term effects on turnout of an intervention delivered by a private group that was designed to address concerns about ballot secrecy in a highly competitive campaign environment.

#### *Political Context*

Republican governor Scott Walker was elected in 2010. After taking office, he implemented many aspects of his conservative political agenda, including the revocation of collective bargaining rights for most public workers. In response, Democrats, unions, and other allied groups attempted to unseat Walker through the recall provision of the Wisconsin

Constitution. After collecting substantially more than the 540,000 signatures necessary to initiate the recall vote, Walker was forced to again challenge his 2010 Democratic opponent, Tom Barrett. On June 5, 2012, Wisconsin voters retained Walker, who received 53% of the vote.

The 2012 recall election took place in an extremely active campaign advocacy environment. It followed the contested April 3, 2012 Republican primary in which nearly 790,000 individuals cast ballots, and as the presidential campaigns and outside groups geared up for a stiff competition in one of the contested “battleground” states in the November election. In the recall, outside groups and the candidates combined to spend nearly \$80 million on advertising and other campaign activities, a figure more than double the \$37 million spent during the 2010 governor’s race, a contest that itself set the previous state record for spending in a gubernatorial contest (Wisconsin Democracy Campaign). These resources translated into extensive field operations designed to reach even those not necessarily predisposed to turn out.<sup>5</sup> Television advertising alone accounted for more than \$18 million in spending leading up to the recall vote (Steinhauser 2012), compared to \$25.5 million spent on advertising in all races during the 2008 electoral campaign in Wisconsin (University of Wisconsin Advertising Project 2010). Overall, the campaign environment more closely mirrored a contested presidential election than either a standard midterm race or a June special election contest.

Not surprisingly, turnout was high in the recall election: 75% of registered citizens voted. By comparison, in the previous midterm election in 2010 that included both a gubernatorial election and a competitive Senate race, only 64% of registrants participated, a gap of 11 percentage points.<sup>6</sup> We note that while these figures are high, turnout still lagged behind Wisconsin’s recent track record in presidential races, which was 86% (of registrants) in 2008 and 87% in 2012. Overall, this campaign environment is therefore substantially more active than in

Connecticut in November of 2010, the environment for Gerber et al.'s (2013b) original field experiment, when only about 58% of active registrants voted.

### *Experimental Design*

A field experiment was designed and implemented by the Voter Participation Center (VPC), a nonprofit, nonpartisan organization dedicated to increasing the participation of unmarried women and other historically underrepresented groups.<sup>7</sup> The group identified a set of citizens believed unlikely to participate in the 2012 recall election in the absence of additional outreach. Specifically, they focused on unmarried women and minority (i.e., non-white) registrants who had no prior record of having voted in Wisconsin elections (demographic groups whose mobilization is the mission of the organization).<sup>8</sup> Eligible subjects were then randomly assigned to a control or treatment condition. The treatment consisted of a mailing sent under the group's letterhead that sought to address salient concerns about ballot secrecy. The control group was not contacted. The outcome of interest is participation in the June 5, 2012 recall election as recorded in Wisconsin administrative records. The experiment was designed and implemented by the VPC. We were provided with the data from the experiment after it had been executed.

The experiment proceeded as follows. The group first obtained a list of eligible Wisconsin registrants from an outside private vendor. The vendor regularly collects voter files from Wisconsin, cleans the data, makes the records uniform, and merges the data with vote history information from previous voter files as well as a number of other variables sold by consumer data vendors. The voter records used to generate the sample for this experiment came from a voter file produced by the Wisconsin Secretary of State in April of 2012. The private vendor processed that file and verified registrants' addresses using a National Change of Address filter.

Second, the organization selected the study population from this list of registrants. Records were first removed if they had an invalid mailing address or if they were included in another experiment run by the organization.<sup>9</sup> From the remaining records, registrants were retained if they 1) were unmarried women or members of a minority group<sup>10</sup> (i.e, non-Caucasians), 2) were listed as active registrants, 3) had registered in December of 2006 or later, and 4) had no record of having voted in any general primary, general election, or special election.<sup>11</sup> This procedure yielded a final study population of 17,360 eligible registrants.<sup>12</sup> In accordance with prior research (Gerber et al. 2013b), we limited our analysis to the 10,200 individuals who had been registered before the November 2008 election but had not participated in that high salience race.<sup>13</sup> The logic of this choice is that later registrants might have voted in 2008 had they been eligible to do so, and are therefore less likely to hold doubts about ballot secrecy than those voters who had forgone voting in a highly competitive environment (e.g., Gerber et al. 2013b find no effect of communication addressing ballot secrecy concerns for registrants with a previous participation history).

Third, the organization randomly assigned half of the members of the subject population to the treatment condition and the other half to the control.<sup>14</sup> Approximately 4 to 7 days before the election,<sup>15</sup> the treated group was sent a mailing emphasizing the integrity of the secret ballot, which we discuss in greater detail below. The control group received no mailing.

After the election, the group obtained information about participation in the recall election and the 2012 general election from its vendor. Recall participation was obtained from a Wisconsin voter file updated in September of 2012, while general election turnout was from a file produced in February 2013. For individuals in this file, turnout was coded 1 if the registrant

voted in the recall election and 0 if not. Additionally, for respondents who were no longer listed in the updated file, turnout was coded as 0.<sup>16</sup>

#### *Description of Treatment Letter*

The VPC mailed informational letters to the treatment group in an envelope listing its name and emphasizing its status as a “non-government, nonprofit, and nonpartisan 501(c)(3) organization” (full text of all letters and envelopes appears in the supplemental materials). The treatment letter was printed on the organization’s letterhead, addressed to the recipient’s first name, and began and ended with general information about the election and an encouragement to vote. The bulk of the letter’s text, however, focused on information intended to alleviate any potential concerns about ballot secrecy. Specifically, it stated:

**Your ballot is secret.** Poll workers keep only a list of who voted, not how they voted. No record of how you or any other voter filled out their ballot is created. Your ballot choices cannot be matched up with your name.

Additionally, voting booths provide a private place for you to fill out your ballot. You place your ballot into the voting machine on top of the locked ballot box without anyone else looking at it.

**Voting is free of intimidation of any kind.** A set of rules is enforced at each polling place to ensure that voters are comfortable casting votes for whomever they prefer. For example, poll workers are not permitted to ask you for whom you voted, and campaigning is prohibited inside of or within 100 feet of any entrance to a polling place.

The treatment letter sought to mitigate anxieties about voting and focused on three sorts of fears about ballot secrecy. The first paragraph addressed concerns about whether individuals’ vote choices could be matched to their names after their ballots were cast.<sup>17</sup> The second paragraph emphasized that the act of voting takes place in private. Therefore, neither poll workers nor other voters would be able to witness which candidates a voter selected. Finally, the third paragraph sought to relieve fears about intimidation and concerns that a voter might be

pressured to divulge or change her vote choice. The treatment letter language is highly similar to the “Secrecy combined” intervention used in Gerber et al. (2013b).

### *Results*

The design of this experiment allows for a straightforward analysis of the effect of being sent a private group’s message about ballot secrecy protections on turnout in both the recall election and the November 2012 election. Before describing those results, however, we address two important issues. The first is about the interpretation of our coefficient estimates. Specifically, the estimated treatment effects that we report are for *assignment* to treatment, rather than for actual exposure to the ballot secrecy treatment language. One of the key reasons that a non-governmental source’s messages could be less effective is that they may be less likely to reach the intended audience—individuals may be more likely to ignore the mailing (i.e., leave it unopened and unread) than they would be to ignore correspondence from a government source. Second, the organization’s experimental design did not include a placebo intervention that transmitted information about the upcoming election without addressing ballot secrecy concerns. Instead, members of the control group were simply not contacted. For this reason, we must be cautious in interpreting the estimates reported below as arising solely due to the ballot secrecy content of the treatment letter. It could be, for example, that simply being reminded that an election is upcoming would increase turnout among this population. While we view this scenario as unlikely, particularly given the intense campaign environment surrounding the recall, we cannot definitively rule it out.<sup>18</sup> Indeed, it is likely that members of both the control and treatment groups in our study had already been exposed to language similar to the non-secrecy content of the letter via mobilization efforts by other groups, making the secrecy content the novel additional treatment. Additionally, Gerber et al. (2013b) find larger effects for prior non-



voters of communication addressing ballot secrecy concerns than for either placebo interventions containing the same non-secrecy information or for standard GOTV messages. (We also present in endnote 22 evidence that for a similar population, an intervention that did not address secrecy concerns had a much smaller effect on turnout.)

Table 1 presents a series of regression models comparing participation in the recall election in the treatment and control groups. Receiving information about the integrity of the secret ballot institution from an unofficial source is modestly, but statistically significantly, associated with increased turnout in the gubernatorial recall election. In column (1), which presents a simple bivariate regression with robust (Huber/White) standard errors, we find that turnout in the recall was 12.9% among those in the control group and 14.0% among those in the treatment group.<sup>19</sup> This difference of 1.1 percentage points, or about 8.5%, has a p-value (one-tailed test) of .051. We note that, as one might expect given the turnout history of the population of registrants examined in this study, voting rates among both the treatment and control groups are much lower than the 75% turnout rate among all registrants in the 2012 recall.

[Insert Table 1 Here]

We can also extract additional individual- and contextual-level characteristics for the registrants included in our study from the voter file provided to us (some of these measures are not available in voter files provided by the state, but are instead constructed from other sources used by the private vendor). We use these additional data in the remaining specifications in Table 1. In the column (2) specification, we include township-level fixed effects to control for unobservable contextual factors such as latent neighborhood baseline turnout propensities, geographically targeted advertising and canvassing efforts, neighborhood social context, and so on. While township may not capture all of these unobservable factors, it serves as a proxy for a

relatively low level of geographic aggregation at which we are still able to retain enough cases to estimate the within-contextual unit treatment effect. Further, including township-level fixed effects will not bias treatment estimates because they are independent of treatment assignment. Controlling for township-level heterogeneity slightly increases the estimated effect of being sent the ballot secrecy letter (to 1.4%) and also improves the precision of the estimate ( $p=.026$ , one-tailed).<sup>20</sup> In column (3), we add to the column (1) specification a number of covariates thought to influence turnout, while in column (4) we include both this set of covariates and the township fixed effects. The covariates include the number of years since the date of registration (from the voter file), the age of the individual (in years) on the day of the recall election, indicators for gender and various racial categories, and an estimate of median household income. Also included are indicators for whether the age, gender, race, and income measures are missing.<sup>21</sup> Accounting for those covariates that might also explain turnout will help to reduce any random differences between the treatment and control groups and also improve the precision of treatment estimates. Per column (3), after controlling for those factors, we continue to find a positive treatment effect of about .9 percentage points ( $p=0.081$ , one-tailed). As in the case without controls, controlling for township in the column (4) model increases the estimated effect of the treatment to 1.2 percentage points ( $p<0.05$ , one-tailed).<sup>22</sup>

Overall, these findings suggest that even in a highly contested campaign environment in which it is likely that most members of the experiment population were already targeted by traditional GOTV mobilization efforts, communication from a private actor about ballot secrecy protections can modestly increase turnout. Do these effects persist for subsequent elections? To address that question, in Table 2 we examine participation in the November 2012 general election in Wisconsin, about five months after the recall vote. In the table, we repeat the four

statistical specifications shown in Table 1, substituting November 2012 turnout for recall participation as the dependent variable. To summarize these results, we find smaller treatment effects across specifications—about half the absolute magnitude of those we find for the recall election—and these estimates are not statistically significant at conventional levels.

[Insert Table 2 Here]

For example, in the simple bivariate regression in column (1), we estimate a treatment effect of about .4 percentage points, a result that is not statistically significant ( $p=.31$ , one-tailed). About 28.5% of the control group votes, more than double the participation rate in the recall election. In column (2), where we account for town-level fixed effects, the estimated treatment effect doubles to .8% with a p-value of .18 (one-tailed), suggesting substantial variation in other causes of turnout by township in the general election (e.g., canvassing or other targeted mobilization efforts). As with recall turnout, adding covariates attenuates these estimates. In column (3) we estimate a .2 percentage point effect ( $p=.43$ , one-tailed), which increases to .6 points ( $p=.27$ , one-tailed) with town fixed effects.

These results are sensitive to model specification and the inclusion of covariates. In part, given the magnitude of the treatment effect in the recall election (about 1 percentage point), it is likely difficult to detect reliably a treatment effect of half that magnitude with our sample size. Furthermore, we should note that these estimates represent the cumulative downstream persistence effects, which may include factors other than belief changes induced by a long-term alleviation of ballot secrecy concerns. Nonetheless, the more important takeaway point is that addressing ballot secrecy concerns appears to have somewhat enduring effects in mobilizing a group that was largely not mobilized by other campaign efforts. Given the imprecision of these

estimates, we now turn to a second experiment that allows us to assess the long-term effects on participation of efforts to address ballot secrecy concerns.

*Experiment #2: The Long-Term Effects of Correspondence from a Connecticut Official*

To further explore the question of whether treatments intended to assuage concerns about ballot secrecy have persistent effects, our second analysis leverages an experiment originally conducted by Gerber et al. (2013b) in 2010. In that experiment, unlike the message sent from a private group in the Wisconsin study, the researchers worked with the Connecticut Secretary of State to send a sample of registrants who had not previously voted a treatment letter from the Secretary of State that addressed ballot secrecy concerns. (See Gerber et al. (2013b) for details about sample construction, etc. All registrants in that analysis were eligible to vote in the 2008 presidential election but did not do so.) In their analysis, they compare the turnout rate in the November 2010 election of those sent the letter to the turnout rate of those sent a “placebo” letter that contained similar information about voting but did not address ballot secrecy concerns. They find that turnout was about 3.9 percentage points higher (a proportional increase of about 17%,  $p < .01$ ) among those sent the treatment letter relative to those sent the placebo letter. What is unknown, however, is whether those effects are likely to endure over time. To the extent that the increases in turnout presented in the original study are the result of changes in beliefs, any downstream effects indicate that those belief changes persist in subsequent elections.

In order to address this question, we obtained from the researchers their original data concerning treatment assignment and 2010 participation. We then appended to these data information about turnout in three 2012 elections: The presidential preference primary held in April, the general (non-presidential) primary held in August, and the November general

election.<sup>23</sup> In all cases, individuals who were no longer in the voter file were coded as not having voted.<sup>24</sup> All other variables come from the original dataset produced by Gerber et al. (2013b).

Analyses using these data appear in Table 3. In column (1), we simply replicate Gerber et al.'s (2013b) original specification predicting turnout in the November 2010 election. Consistent with their reported results, we find that those registrants sent the secrecy intervention were 3.8 percentage points more likely to vote than those sent the placebo letter. In the remainder of the columns, we examine 2012 voting behavior. These outcomes took place two years after the initial interventions were delivered. On the one hand, this is a much longer period than we can study in Wisconsin, so we might imagine effects will be far less likely to persist. On the other hand, Gerber et al. (2013b) find substantially larger initial treatment effects in 2010 than we do in Wisconsin for 2012, which raises the possibility that the long-term effects will be larger than the short-term effects we find in Wisconsin.

[Insert Table 3 Here]

In column (2), we present analysis for a turnout index, which is the number of elections in 2012 (0 to 3) in which a registrant voted. The average score on this measure in the control group, which is shown in the bottom row of the table, is .33 for this sample, which is less than half of the .76 for the entire sample of registrants in the voter file. Per the column (2) estimate, those who were sent a treatment letter in 2010 score .03 points higher on this measure, an estimate that is significant at  $p=.03$  (one-tailed). In other words, this means the turnout index score is about 10% larger among those sent a treatment letter two years earlier than for those sent a placebo letter.

In columns (3) through (5), we examine the long-term effects of the treatment on voting in individual elections. We begin with the presidential primary election, in which .5% of the

sample voted. Those sent the treatment letter were .6 percentage points more likely to vote ( $p=.02$ , one-tailed), a proportional increase relative to the control group of more than 100%. In the regular primary, average turnout in the control group was 1.3%, but it was 1.1 percentage points higher among those sent a treatment letter ( $p<.01$ , one-tailed). This is a proportional increase of 84% relative to the control group. Finally, in the November 2012 presidential election, being sent a treatment letter two years earlier is associated with a 1.5 percentage point increase in turnout, but this result is not significant at conventional levels ( $p=.16$ , one-tailed). In this sample, the control group voted 31.5% of the time, implying a 5% increase in turnout. Of note, our sample of 3,744 is likely too small to detect reliably a 1.5 percentage point effect given a baseline participation rate of 32%.<sup>25</sup>

In summary, our analysis of 2012 Connecticut turnout using Gerber et al.'s (2013b) original 2010 intervention suggests that a substantial portion of those effects endure over time. In the most appropriate comparison of general election turnout, the original effect on 2010 turnout was 3.8 points, whereas the effect on 2012 turnout was 1.5 points. Setting aside differences in baseline turnout (32 versus 18% in the control groups, respectively), the 2012 effect is about 40% of the size of the 2010 estimate. Keeping in mind again that this is the cumulative downstream persistence effect, finding that a single piece of mail sent two years earlier has about 40% of its original effect suggests the initial intervention has substantial and enduring long-term effects in this context.

When compared to the downstream effects of the ballot secrecy intervention in the Wisconsin gubernatorial recall election, the strength of these downstream effects in Connecticut are substantial. Specifically, whereas the subsequent election used to examine the persistence of the ballot secrecy treatment took place only about five months later, with a downstream

treatment effect about half the size of the original, the subsequent election in the Connecticut analysis took place two years later, with a downstream treatment effect about 40% of the original. Further, the treatment effects from the original Connecticut study are estimated with far greater precision. Overall, the results from the two studies suggest that the continuing impact of an initial ballot secrecy treatment effect from an official source during a midterm general election is likely to be greater than the downstream effect of a similar message from a non-governmental source during a high-salience special election.

### *Discussion and Conclusion*

How do beliefs about political institutions affect participation? Can providing information designed to alleviate fears about ballot secrecy increase turnout? Building on prior research, we present evidence from a novel experiment conducted in Wisconsin to show that a message from a non-official and nonpartisan voter mobilization group designed to address fears about ballot secrecy modestly increased turnout in the 2012 gubernatorial recall election. In the process, we replicate previous work (Gerber et al. 2013b) that finds that allaying ballot secrecy fears can increase turnout among eligible voters who have never participated. Additionally, this new experiment addresses two unresolved questions about source credibility and the effectiveness of campaign communication in highly contested campaign environments.

In particular, we show that despite their intrinsic lack of credibility as government officials, communication from private voter mobilization organizations about the election administration process can nonetheless increase participation. Although we admittedly do not possess a direct measurement that demonstrates a change in fears or anxiety related to ballot secrecy, our results imply that governmental actors are not the only credible and effective sources for changing individuals' beliefs about the process of voting. We also show that efforts

to mobilize voters by addressing beliefs about political institutions can be effective even in highly contested races. Whereas prior research has argued that these intense campaigns make salient the importance of voting for most registrants and increase the chances that potential voters are mobilized by other groups, our findings suggest that contest intensity levels are less important when communication addresses a persistent threat to participation: beliefs about political institutions.<sup>26</sup>

The other theoretical question concerns the long-term effects of efforts to address doubt about ballot secrecy. In addition to examining turnout in November 2012 in Wisconsin, five months after the independent group delivered its message about ballot secrecy, we also leverage an earlier study conducted by Gerber et al. (2013b) in Connecticut in 2010 to study the long-term effects of communications about ballot secrecy protections. In Wisconsin, we find that the turnout of those targeted by the private group's communication increased by about half as much in November 2012 as it did in the June recall election, although those estimates are not statistically significant at conventional levels. In Connecticut, we examined turnout two years after the Secretary of State provided some registrants with information about ballot secrecy protections. Despite the passage of two years' time, we find that total 2012 turnout (in two primary elections and one general election) is about 10% higher among those sent a letter about ballot secrecy protections than for those sent a placebo letter. These increases in turnout are individually statistically significant for the two primary elections (p-values of .02 and <.01) and marginally significant for the general election (p=.15). Addressing doubts about ballot secrecy therefore appears to increase turnout in the election most proximate to the treatment and also in subsequent elections.



Beyond these theoretical and empirical questions, our work also suggests several important avenues for additional research, two of which we highlight here. The first concerns the most effective way to mobilize marginal voters in relatively competitive electoral contexts. In light of our findings, it may be that considering ways to further reach out to voters to make voting “easier” or emphasizing the civil or social obligation of doing so is not the area of greatest potential for marginal increases in turnout. Consistent with this rationale, Smith and Sylvester (2013) demonstrate that efforts to get low-propensity voters to convert to permanent vote by mail status are unsuccessful when framed as a way to reduce the cost of voting or heighten its convenience. Conversion increases, however, when messaging stresses the integrity of the alternative voting method, implying that beliefs about the voting process are an important source of differences in political behavior. Similarly, if beliefs about institutions are the causes of the unwillingness to vote, even private actors may be able to increase turnout by lessening these sorts of fears. Of course, this raises a set of questions about what other beliefs voters might have about political institutions and the voting process, which would suggest starting with an attempt to identify those beliefs and determine how best to address them. Such efforts might, for example, include delineating concerns about ballot secrecy from more general fears about voter intimidation. Although certainly related, these concerns pertain to distinct aspects of the voting process that may dissuade participation.

A second question relates to the political consequences of inaccurate beliefs about institutions for representation. While this work shows there are both short- and long-term effects of addressing ballot secrecy concerns, we know relatively little about the potential heterogeneous influence of such efforts along demographic, socioeconomic, and residential mobility lines. This ambiguity derives largely from our lack of understanding about how such beliefs—concerning

ballot secrecy or other aspects of political participation—are distributed across the entire population. Given that persistent differences in who participates in politics appear to be alleviated somewhat by addressing these beliefs, it would be useful to seek to identify and contact those groups for whom such beliefs appear most prevalent. Doing so would likely require a large experimental population for which would could reliably distinguish treatment effects across groups.

Setting these extensions aside, we present several novel experimental results in this paper. Briefly summarized, we show that even private actors seeking to increase participation in a highly contested campaign environment can increase turnout among registrants who have never before voted by providing information designed to address ballot secrecy fears. Additionally, these effects, as well as those of a similar communication provided by a state official, appear to persist over time, changing the composition of the electorate in both the short- and long-term. Taken together, these results imply that beliefs about ballot secrecy are an important and remediable barrier to participation not otherwise addressed by regular campaign mobilization efforts in any given election or over time.

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<sup>1</sup> Gerber et al. (2013b) define non-voters as those who had not voted in the 2008 presidential election despite having been registered at the time of the election.

<sup>2</sup> Two additional field experiments lend credence to these results. Karpowitz et al. (2011) find that random assignment to a voting booth with extra protections to ensure secrecy reduces concerns about privacy by local political minorities, a group that perceives greater threats to such secrecy in general. Additionally, Grose and Russell (2008) determine that information regarding the non-secret nature of one's vote in the Iowa Caucus decreases participation in that contest by 22 percentage points in comparison to those not informed of this fact.

<sup>3</sup> One can think of this credibility as arising for two distinct reasons. The first concerns the perceived relative expertise of a government source over a non-government source. The second is related to ideological position. Compared to a private group seeking to affect an election outcome, a government official may be seen as less biased.

<sup>4</sup> Of the thirty-one mobilization field experiment articles appearing in leading political science journals (specifically, *American Political Science Review*, *American Journal of Political Science*, *Journal of Politics*, *Political Behavior*, *American Politics Research*, *Political Research Quarterly*, *Electoral Studies*, and *Quarterly Journal of Political Science*) between 2003 and 2012 that identified a positive and statistically significant relationship, fourteen were conducted exclusively in one of these low participation contexts (three other studies found no effect). Five articles detailed a successful intervention in more salient midterm or gubernatorial elections, while an additional four included experiments carried out during both low- and high-salience elections.

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<sup>5</sup> See <http://www.cnn.com/2012/06/05/politics/wisconsin-recall/index.html> and <http://www.nationalreview.com/articles/302180/winning-wisconsin-ground-game-katrina-trinko>.

<sup>6</sup> Turnout and registration data are from Wisconsin's Government Accountability Board (GAB). Comparable figures (also from the GAB) for the turnout rate of the voting age population are 57% (Recall) and 50% (2010 General). Statewide voter registration rates are not available in Wisconsin prior to 2008, as not all municipalities were required to maintain voter registration lists (see <http://gab.wi.gov/elections-voting/statistics>).

<sup>7</sup> For more information about the organization's mission, see their website (<http://www.voterparticipation.org/about-us/>).

<sup>8</sup> A number of studies document the responsiveness of minorities in particular to GOTV efforts (e.g., Garcia Bedolla and Michelson 2012; Green 2004; Ramirez 2007), though indirect methods of contact, such as direct mailing (used by the VPC in this study and described below), are mostly ineffective. The fact that minorities tend to express greater concerns about ballot secrecy (Gerber et al. 2013a), however, suggests that efforts to alleviate these anxieties may be a particularly fruitful means to mobilize members of these groups.

<sup>9</sup> Specifically, the organization that implemented this design simultaneously implemented two others, each with independent study populations. They first defined the study population for one of their other experiments according to a particular set of voting and demographic characteristics and produced a list of records fitting those criteria. They then defined the study population for the secret ballot experiment using the criteria discussed in the text, produced another list of records, and crosschecked the two lists for overlap. Any records appearing on both lists were included in the first study but not the ballot secrecy study that is of interest here. For their third experiment, also run independently from, but simultaneously with, the secret ballot experiment,



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the organization defined a third study population according to certain criteria, produced a third list, crosschecked that list with the first two lists, and eliminated any records already included in the first two study populations.

<sup>10</sup> Gender, marital status, and race were determined by the vendor using proprietary algorithms and matches to outside data sources.

<sup>11</sup> The voter file used to determine the subject population did not include information about participation in certain off-cycle spring elections (e.g., elections for Circuit Court Judge, Court of Appeals Judge, State Superintendent, etc.). For this reason, it is possible that some of the individuals listed as non-voters had in fact participated in some prior election. Given the findings in Gerber et al. (2013b) that registrants who had previously voted were not affected by ballot secrecy assurances, including the likely very small number (if any) of individuals who had voted in those off-cycle elections but none others would tend to depress estimates of apparent treatment effects.

<sup>12</sup> These population criteria derive from the organization's particular interest in encouraging low-propensity voters to participate in the political process, and the researchers had no involvement in the selection of these characteristics. Given our theory, such individuals should rank among those most likely to respond to the ballot secrecy assurances.

<sup>13</sup> For each of the model specifications presented in the text examining the effect on turnout among the 10,200 pre-2008-general-election registrants, we also performed the same analyses on the full set of 17,360 registrants in the study population. Tables presenting those results are in the supplemental materials.

<sup>14</sup> Appendix A presents summary statistics and tests of balance between the treatment and control groups. There are no noteworthy differences in the distributions of covariates between groups. A

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test of the randomization procedure was performed by using OLS to regress assignment to the treatment group on all of the covariates presented in Table 1. A joint F-test indicates that we fail to reject the hypothesis that all of covariates are simultaneously equal to 0 ( $p=0.35$ ). This provides evidence that the covariates used in the analysis do not have significant explanatory power in predicting assignment to treatment or control.

<sup>15</sup> Treatment letters were sent out between May 29 and June 1 for the June 5 gubernatorial recall election.

<sup>16</sup> We include these individuals in our analyses as non-voters because the treatment could have affected the probability that a voter was later removed from the rolls. In the post-election voter file used to determine turnout in the June recall election, 259 individuals among the 17,360 in our sample were found to have been purged from the voter rolls, including 113 that registered prior to the 2008 general election. In the file used to determine turnout in the 2012 general election, 263 individuals were found to have been purged, including 115 that registered prior to the 2008 general election.

<sup>17</sup> The letter mentions that poll workers keep a list of those who voted but makes no reference to this record as being publicly available. The absence of this information distinguishes the message from those common in the social pressure literature (e.g., Gerber et al. 2008, 2010; Mann 2010) that explicitly state voting histories may be shared with family, friends, and/or neighbors.

<sup>18</sup> Green et al. (2013) note in their meta-analysis on the subject that non-advocacy mailings on average (i.e., across all electoral contexts) produce an approximate 0.19 percentage point increase in turnout.

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<sup>19</sup> Specifications employing logistic regression with the same covariate profiles are presented in the supplemental materials. Estimated treatment effects and significance levels do not differ substantially when using logistic regression.

<sup>20</sup> The analyses reported in Tables 1 and 2 that include township-level fixed effects do not report results using clustered standard errors. Employing clustering *reduces* the estimated standard error for the treatment variable, further improving indications of statistical significance. Out of an abundance of caution, we report the larger standard errors from unclustered analyses in the tables. To assess the importance of the clustering approach, we also tested the sharp null prediction of no treatment effect using exact standard errors and a normal approximation to the sampling distribution of the difference of means. We calculated the treatment effect as a weighted average (with weights proportional to the town's share of the overall sample) of the town-level effect for all towns with greater than 50 observations (we pool small towns into one group). Using the observed town-level variance, we calculated the analytical standard errors under the sharp null (see Gerber and Green 2012), and standard errors for the sampling distribution of the weighted average of the town-level treatment effects using standard formulas for the variance of a sum. For the recall election we find a standard error of .76 percentage points. The weighted town averages yield a treatment effect estimate of 1.34 points and, based on the normal approximation, a z-score of 1.34 ( $p = .04$ , one tailed.). For the general election analysis reported in Table 2, the estimated treatment effect is .97 percentage points, the standard error 1 point, and  $p = .33$  (one tailed). The results of the tests using randomization inference are presented in Table S7 of the supplementary material.

<sup>21</sup> Because the study population was intended to be only single women and people of color, it is important to clarify how race and gender information could be missing. Specifically, the VPC

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employed as selection criteria that the respondent be either non-white or both female and single.

The criterion that the registrants be non-white led to the inclusion of both males and females with missing racial information, as well as individuals with missing gender information. The criterion that the registrant be female and single also led to the inclusion of individuals with missing racial information.

<sup>22</sup> To further test the argument that the ballot secrecy messages are not likely to act as mere election reminders, we also analyzed a parallel experiment undertaken by the VPC during the Wisconsin gubernatorial recall campaign. Specifically, using a treatment intended to make salient intergroup competition between residents of different towns in Wisconsin, the VPC's "group competition" experiment provided letter recipients with the turnout rates of neighboring towns that had higher turnout rates in the 2010 midterm elections than the recipients' own town. Using the same specifications presented in Table 1, the group competition message was found to have a positive but small (between .1 and .4 percentage points) and not statistically significant effect on turnout among registered non-voters in the experiment population who registered prior to the 2008 general election (the same population that constitutes the sample for the ballot secrecy experiment). These estimated treatment effect sizes range between about 8% and 36% of those for the ballot secrecy experiment, depending on specification. Overall, the findings suggest that the effect of a non-secrecy treatment in the same campaign context was substantially smaller than that of the ballot secrecy treatment. These results, along with an example treatment letter, are presented in the supplemental materials.

<sup>23</sup> Only registered partisans can vote in the party primaries in Connecticut, although a registrant can change her party registration status up to 3 months before any primary. We include all

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registrants because of the possibility that individuals could choose to alter their party affiliation at any time.

<sup>24</sup> See endnote 16. Among the 3744 individuals from the pre-election Connecticut voter file that are included in the analysis in Table 3, 66 were found to have been purged from the post-election file used to determine turnout in 2012.

<sup>25</sup> This is because the OLS estimator is less sensitive to detecting small treatment effects for a binary outcome variable when the outcome is, on average, closer to .5 than when it is closer to either 0 or 1. As with the analyses of Wisconsin turnout, we calculated the exact standard errors of the sampling distribution using randomization inference. Specifically, Table S7 of the supplementary material presents estimated treatment effects for each of the models in Table 3, along with a summary of the corresponding randomization distribution and p-values for the sharp null (Gerber and Green 2012). All p-values from the randomization distribution are slightly larger than the asymptotic estimates for the treatment effects in Table 3, but the qualitative evaluation of statistical significance remains unchanged.

<sup>26</sup> To be clear, we find that communication from an outside group can be effective even in a contested campaign environment. However, the estimated treatment effects in Wisconsin in 2012 are still smaller than those reported by Gerber et al. (2013) in Connecticut in 2010. To understand the relative importance of context and source credibility in explaining these differences, a next step is to repeat these experimental designs exploiting the same message sources across campaign contexts or different message sources in the same campaign context.

**Table 1: Effect of Secrecy Intervention on Voting in Wisconsin 2012 Gubernatorial Recall Election**

	(1)	(2)	(3)	(4)
	Pre-2008 Registrants			
	Voted in 2012 Recall Election (Yes = 1)	Voted in 2012 Recall Election (Yes = 1)	Voted in 2012 Recall Election (Yes = 1)	Voted in 2012 Recall Election (Yes = 1)
Ballot Secrecy Treatment (Yes = 1)	0.011 [0.007]*	0.014 [0.007]**	0.009 [0.007]*	0.012 [0.007]**
Years Since Registration Date			-0.036 [0.014]***	-0.043 [0.016]***
Age on Election Day (Years)			0.000 [0.000]	0.000 [0.000]
Age Missing (Yes = 1)			-0.046 [0.019]**	-0.046 [0.020]**
Female (Yes = 1)			0.070 [0.008]***	0.071 [0.008]***
Gender Missing (Yes = 1)			0.024 [0.011]**	0.025 [0.011]**
Median Household Income (Thousands)			0.000 [0.000]	0.000 [0.000]
Median Household Income Missing (Yes = 1)			-0.105 [0.018]***	-0.110 [0.035]***
Asian (Yes = 1)			0.040 [0.019]**	0.038 [0.021]*
Black (Yes = 1)			0.082 [0.012]***	0.069 [0.014]***
Latino (Yes = 1)			0.013 [0.012]	0.007 [0.015]
Middle Eastern (Yes = 1)			0.042 [0.035]	0.030 [0.038]
Native American (Yes = 1)			-0.004 [0.038]	-0.048 [0.078]
Race Missing (Yes = 1)			0.076 [0.042]*	0.062 [0.042]
Constant	0.129 [0.005]***	0.128 [0.005]***	0.214 [0.059]***	0.252 [0.069]***
Observations	10200	10200	10200	10200
R-squared	0.000	0.000	0.015	0.015
Number of Townships		610		610
R-squared Within		0.000		0.015

Note: Cell entries are OLS regression coefficients with standard errors in brackets. Standard errors are robust in columns 1 and 3. Clustered standard errors for column 2 and 4 specifications yield smaller standard errors than those reported here. Dependent variable is vote in the 2012 Wisconsin Gubernatorial Recall Election (Yes = 1). Registrants not matched to post-election file are counted as non-voters. Analyses use subset of experimental subjects who were registered prior to the registration deadline for the 2008 General Election. \*p < .1, \*\*p < .05, \*\*\*p < .01, one-tailed tests for Ballot Secrecy Treatment.

**Table 2: Effect of Secrecy Intervention on Voting in Wisconsin 2012 General Election**

	(1)	(2)	(3)	(4)
	Pre-2008 Registrants			
	Voted in 2012 General Election (Yes = 1)	Voted in 2012 General Election (Yes = 1)	Voted in 2012 General Election (Yes = 1)	Voted in 2012 General Election (Yes = 1)
Ballot Secrecy Treatment (Yes = 1)	0.004 [0.009]	0.008 [0.009]	0.002 [0.009]	0.006 [0.009]
Years Since Registration Date			-0.054 [0.019]***	-0.046 [0.021]**
Age on Election Day (Years)			-0.001 [0.001]**	-0.001 [0.001]**
Age Missing (Yes = 1)			-0.111 [0.026]***	-0.121 [0.027]***
Female (Yes = 1)			0.117 [0.011]***	0.116 [0.011]***
Gender Missing (Yes = 1)			0.040 [0.014]***	0.043 [0.015]***
Median Household Income (Thousands)			0.001 [0.000]***	0.001 [0.000]**
Median Household Income Missing (Yes = 1)			-0.188 [0.028]***	-0.187 [0.046]***
Asian (Yes = 1)			0.029 [0.025]	0.023 [0.028]
Black (Yes = 1)			0.139 [0.015]***	0.119 [0.019]***
Latino (Yes = 1)			0.053 [0.016]***	0.039 [0.020]**
Middle Eastern (Yes = 1)			0.069 [0.047]	0.053 [0.051]
Native American (Yes = 1)			0.049 [0.059]	-0.008 [0.103]
Race Missing (Yes = 1)			-0.049 [0.042]	-0.072 [0.056]
Constant	0.285 [0.006]***	0.283 [0.006]***	0.399 [0.081]***	0.396 [0.091]***
Observations	10200	10200	10200	10200
R-squared	0.000	0.000	0.024	0.024
Number of Townships		610		610
R-squared Within		0.000		0.024

Note: Cell entries are OLS regression coefficients with standard errors in brackets. Standard errors are robust in columns 1 and 3. Clustered standard errors for column 2 and 4 specifications yield smaller standard errors than those reported here. Dependent variable is vote in the 2012 Wisconsin General Election (Yes = 1). Registrants not matched to post-election file are counted as non-voters. Analyses use subset of experimental subjects who were registered prior to the registration deadline for the 2008 General Election. \*p < .1, \*\*p < .05, \*\*\*p < .01, one-tailed tests for Ballot Secrecy Treatment.

**Table 3: Connecticut Persistence Analysis**

	(1)	(2)	(3)	(4)	(5)
	Voted in 2010 General Election (1 = Yes)	2012 Elections Voted In (0-3)	Voted in 2012 Pres. Primary (1 = Yes)	Voted in 2012 Non-Pres. Primary (1 = Yes)	Voted in 2012 General Election (1 = Yes)
Any Secrecy Treatment (Yes = 1)	0.038 [0.013]***	0.032 [0.017]**	0.006 [0.003]**	0.011 [0.004]***	0.015 [0.016]
Age (Mean-Deviated)	0.011 [0.002]***	0.009 [0.003]***	0.000 [0.000]	-0.002 [0.001]*	0.011 [0.002]***
Age-Squared (in Hundreds, Mean-Deviated)	-0.009 [0.002]***	-0.008 [0.003]***	0.000 [0.001]	0.002 [0.001]**	-0.010 [0.002]***
Female (Yes = 1)	0.138 [0.025]***	0.209 [0.033]***	0.009 [0.005]*	0.013 [0.010]	0.187 [0.031]***
Male (Yes = 1)	0.146 [0.026]***	0.178 [0.034]***	0.011 [0.005]**	0.014 [0.010]	0.153 [0.031]***
Registered Democrat (Yes = 1)	0.018 [0.014]	0.074 [0.019]***	0.002 [0.002]	0.023 [0.005]***	0.048 [0.017]***
Registered Republican (Yes = 1)	0.148 [0.024]***	0.203 [0.034]***	0.050 [0.010]***	0.073 [0.013]***	0.080 [0.025]***
Number in Household = 1	-0.180 [0.060]***	-0.106 [0.067]	-0.010 [0.016]	0.006 [0.017]	-0.103 [0.062]*
Number in Household = 2	-0.107 [0.061]*	-0.001 [0.069]	-0.006 [0.016]	0.015 [0.018]	-0.010 [0.063]
Number in Household = 3	-0.156 [0.061]**	-0.034 [0.070]	-0.004 [0.017]	0.010 [0.018]	-0.040 [0.064]
Number in Household = 4	-0.081 [0.066]	0.007 [0.075]	-0.003 [0.018]	0.004 [0.019]	0.006 [0.069]
Town = Bridgeport	-0.114 [0.022]***	-0.159 [0.032]***	-0.005 [0.002]***	0.003 [0.010]	-0.157 [0.028]***
Town = New Haven	-0.082 [0.028]***	-0.093 [0.045]**	0.002 [0.007]	0.013 [0.014]	-0.109 [0.036]***
Town = Stamford	0.007 [0.039]	0.156 [0.053]***	0.003 [0.010]	0.002 [0.013]	0.150 [0.050]***
Town = Hartford	-0.064 [0.029]**	0.004 [0.044]	0.025 [0.013]**	-0.015 [0.007]**	-0.006 [0.038]
Town = Waterbury	-0.110 [0.027]***	-0.072 [0.040]*	0.003 [0.008]	-0.017 [0.003]***	-0.057 [0.040]
Town = Norwalk	-0.079 [0.046]*	-0.015 [0.061]	-0.009 [0.003]***	-0.021 [0.004]***	0.016 [0.060]
Constant	0.187 [0.065]***	0.174 [0.074]**	-0.004 [0.017]	-0.024 [0.020]	0.202 [0.068]***
Observations	3744	3744	3744	3744	3744
R-squared	0.057	0.045	0.033	0.038	0.037
Mean of DV in Voter File	0.202	0.355	0.010	0.020	0.325
Mean of DV in Control Group	0.176	0.334	0.005	0.013	0.315

Note: Cell entries are OLS regression coefficients with robust standard errors in brackets. Dependent variables are described in column headings. Registrants not matched to post-election file are counted as non-voters. Column 1 results are from Gerber et al. (2013b). \*p < .1, \*\*p < .05, \*\*\*p < .01, one-tailed tests for Any Secrecy Treatment.



**Table A1: Summary Statistics by Control and Treatment Assignment**

	Assignment			
	Control		Treatment	
	Mean	S.D.	Mean	S.D.
Voted in 2012 Recall Election (Yes = 1)	0.129	0.335	0.140	0.347
Years Since Registration Date	3.922	0.235	3.921	0.233
Age on Election Day (Years)	10.545	20.191	10.436	19.916
Age Missing (Yes = 1)	0.752	0.432	0.750	0.433
Female (Yes = 1)	0.527	0.499	0.541	0.498
Gender Missing (Yes = 1)	0.133	0.340	0.128	0.334
Median Household Income (Thousands)	30.566	13.853	30.958	14.134
Median Household Income Missing (Yes = 1)	0.012	0.107	0.010	0.099
Asian (Yes = 1)	0.038	0.192	0.044	0.206
Black (Yes = 1)	0.587	0.492	0.593	0.491
Latino (Yes = 1)	0.172	0.378	0.155	0.362
Middle Eastern (Yes = 1)	0.009	0.092	0.009	0.097
Native American (Yes = 1)	0.005	0.074	0.006	0.078
Race Missing (Yes = 1)	0.008	0.090	0.007	0.081
Milwaukee Resident (Yes = 1)	0.715	0.451	0.704	0.456
Frequencies	5101		5099	

Note: Cell entries are means, standard deviations, and frequencies of vote history and demographic information on the subset of experimental subjects who were registered prior to the registration deadline for the 2008 General Election. Registrants not matched to post-election file are counted as non-voters. See text for details.

**Table A2: Balance Test, Assignment to Treatment Conditional on Covariates**

	Ballot Secrecy Treatment (Yes = 1)
Years Since Registration Date	-0.003 [0.021]
Age on Election Day (Years)	-0.001 [0.001]
Age Missing (Yes = 1)	-0.027 [0.028]
Female (Yes = 1)	0.013 [0.012]
Gender Missing (Yes = 1)	-0.007 [0.017]
Median Household Income (Thousands)	0.000 [0.000]
Median Household Income Missing (Yes = 1)	-0.024 [0.050]
Asian (Yes = 1)	0.032 [0.029]
Black (Yes = 1)	0.006 [0.017]
Latino (Yes = 1)	-0.028 [0.019]
Middle Eastern (Yes = 1)	0.017 [0.054]
Native American (Yes = 1)	0.023 [0.067]
Race Missing (Yes = 1)	-0.047 [0.060]
Constant	0.521 [0.092]***
Observations	10200
R-squared	0.001

Note: Cell entries are OLS regression coefficients with standard errors in brackets. Dependent variable is assignment to treatment group (Yes = 1). Sample is the subset of subjects who were registered prior to the registration deadline for the 2008 General Election. Joint  $F$ -test of null hypothesis that all estimates other than the constant are equal to 0 is not significant ( $F(13, 10186) = 1.10, p = 0.35$ ). \* $p < .1$ , \*\* $p < .05$ , \*\*\* $p < .01$ , two-tailed.