Do Perceptions of Ballot Secrecy Influence Turnout? Results from a Field Experiment

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Abstract: Although the secret ballot has been secured as a legal matter in the United States, formal secrecy protections are not equivalent to convincing citizens that they may vote privately and without fear of reprisal. We present survey evidence that those who have not previously voted are particularly likely to voice doubts about the secrecy of the voting process. We then report results from a field experiment where we mailed information about protections of ballot secrecy to registered voters prior to the 2010 general election. Consistent with our survey data, we find that these letters increased turnout for registered citizens without records of previous turnout, but did not appear to influence the behavior of citizens who had previously voted. The increase in turnout of more than three percentage points for those without previous records of voting is notably larger than the effect of a standard get-out-the-vote mailing for this group. Overall, these results suggest that although the secret ballot is a long-standing institution in the United States, beliefs about this institution may not match the legal reality and that providing basic information about ballot secrecy can affect the decision to participate to an important degree.

Fair and open elections, in which citizens can cast ballots as they see fit, are a defining characteristic of legitimate democratic systems. The secret ballot is generally thought of as an essential institution for protecting voters from fear of intimidation or coercion in these contests. Indeed, in the United States, the adoption of the secret ballot is viewed as a landmark progressive reform.[1] In this paper, we argue that although the secret ballot has been secured as a legal matter, formal institutions and practices to protect ballot secrecy are not equivalent to convincing citizens that they may vote privately and without fear of reprisal. This distinction between legal protections and beliefs is essential, because it is a citizen's beliefs about electoral institutions—and not the formal operation of those institutions—that affects her actions. While some previous work has examined the historical consequences of the formal implementation of the secret ballot, little work has considered the potential relationship between *beliefs* about ballot secrecy and contemporary voting behavior (but see Gerber et al. forthcoming).

We provide evidence that, even in the contemporary U.S.—a longstanding democracy that has used the secret ballot for over 100 years—doubts about ballot secrecy are surprisingly widespread. This long experience coupled with the practice of professional election administration suggest that the United States should be a difficult case for finding such doubts relative to more recent democracies. Despite this long history, we present results from a new opinion survey that shows many Americans have doubts about the secrecy of the ballot and that these doubts are more prevalent among inexperienced citizens (those who have never voted) than among those who have previously participated. Building on the results from opinion surveys, we also designed a large-scale, randomized placebo-controlled field experiment

[1] On the history of the adoption and spread of the Australian (secret) ballot see, for example, Benson (1941), Evans (1917), Gerber (1994), Rusk (1970), and Wigmore (1889). For related work in the comparative context, see Schaffer (1998) and Stokes (2005). Some scholars have argued that there may have been other motivations for the adoption of the secret ballot (see Burnham 1970; Kousser 1974) and other consequences of its implementation (see Heckelman and Yates 2002; Heckelman 1995; 2004).

that was conducted in cooperation with the Connecticut Secretary of State.[2] This experiment allows us to test the effects of providing registered voters with information about protections of ballot secrecy.

We find that beliefs about the secrecy and anonymity of the voting process have real consequences for political participation. In particular, the experiment shows that providing information about the anonymity of the ballot and the protections against intimidation at the polls to those who had not previously voted increased turnout for this group (who constitute around 20 percent of the registered voter population in Connecticut) in the November 2010 election by an estimated 3.5 percentage points compared to a control condition. This represents a substantial increase in turnout for this group of politically disengaged citizens—proportionally the intervention increases turnout by about 20 percent.[3] The estimates of these effects are larger than the estimates of both a placebo intervention that did not include information about ballot secrecy and a similarly designed intervention that emphasized the civic responsibility associated with voting, the latter message is an example of a common get-out-the-vote (GOTV) intervention.

By contrast, for those who had previously voted (the remaining 80% of registrants), providing information about ballot secrecy protections appears to have negligible effects on participation, particularly when compared to other GOTV interventions. These differences in effects across groups are

[2] None of the costs of this research project were born by the Secretary of State. Partnering with state administrative agencies to assess the effect of information about government procedures is an important contribution of this paper and follows on the work of others (e.g., Blumenthal, Christian, and Slemrod 2001; Slemrod, Blumenthal, and Christian 2001).

[3] Registered non-voters are 21.4 percent of the Connecticut voter file we used to construct our sample, and 16.2 percent of the sample targeted in our experiment (after applying the filters discussed on page 9). By comparison, 2010 voter files from Florida and Colorado show 22.7% (2.8 million) and 24.0% (841,000) of registrants, respectively, have never voted.

consistent with the survey evidence showing that doubts about ballot secrecy are more prevalent among those who have not previously voted.

Our work has three important implications. First, while other research has considered how either perceptions of ballot secrecy (Gerber et al. forthcoming; Karpowitz et al. 2011) or conflict avoidance (Mutz 2002; Ulbig and Funk 1999) affect political behavior and participation as reported in surveys, this is the first work to test the effect of addressing doubts about ballot secrecy and the potential for conflictual interactions at the polls on behavioral outcomes in a field experimental setting. Our field experimental findings suggest doubts about ballot secrecy discourage political participation for this segment of the population, and that addressing those fears can ameliorate them. Importantly, because these interventions can be readily targeted at those most likely to hold these infelicitous beliefs with information readily available in the voter file (e.g., a concerned policymaker can send letters directly to non-voters in the voter file without wasting letters on those who normally participate), addressing these beliefs is an achievable public policy goal. Substantively, our simple intervention mobilized citizens who are rarely targeted by campaigns, but whose lack of participation is a point of concern for American democracy: those who were registered, but had not previously participated in elections.

Second and more broadly, while a large body of research considers ways to increase turnout by decreasing the transaction costs of voting (e.g. registration reforms, mail elections), our experiment identifies a potential limitation of these "convenience" reforms in the presence of doubts about the voting process and shows that a fruitful alternative is to instead attempt to remediate those doubts. While those beliefs appear incongruent with how elections are actually conducted within statutory and administrative rules, those fears are still an important source of perceived "costs" to voting independent of the administrative ease of obtaining and casting a ballot.

Third, and most generally, our research provides behavioral evidence that beliefs about the operation of institutions, independent of how those institutions are actually constructed, can have important behavioral consequences. Policymakers should therefore take seriously citizens' attitudes about the political process, and consider ways to address infelicitous beliefs that inhibit political participation.

Institutions may effectively accomplish goals such as protecting the secrecy of the ballot. However, mechanically achieving these goals may not be enough if citizens are unaware of these institutions or do not believe that they work. Our findings suggest that as states and counties across the United States move to implement convenience voting, voting by mail, electronic voting, and other new voting procedures, more careful attention should be paid to not only the design and operation of these new electoral institutions (see, e.g., Alvarez and Hall 2010 on electronic voting), but also to beliefs about each new institution's operation. We show that Americans harbor doubts of meaningful consequence for real political behavior even about the long-standing, well-used, and well-known institution of the secret ballot at polling place elections.

The remainder of the paper proceeds as follows. In the next section we first briefly outline our expectations for how beliefs about ballot secrecy may be associated with political participation. We then discuss our survey results about the nature of doubts about ballot secrecy and our particular interest in those citizens who have never previously voted. Next, we describe our experimental design and then discuss the results of our analysis. The final section identifies some extensions to our work, its limitations, and the broader implications of our findings.

I. Beliefs about Ballot Secrecy and Political Participation

Scholarship on turnout in advanced democracies implicitly assumes that the formal institution of the secret ballot is sufficient to remedy any concerns about ballot secrecy that might discourage participation. Despite extensive research on the causes and correlates of turnout, such an assumption has not, to our knowledge, been subject to rigorous empirical review. In the United States, for example, political scientists have offered many explanations for why some citizens do not participate, including individual-level differences (e.g., socioeconomic status [Milbrath and Goel 1977; Wolfinger and Rosenstone 1980], genetics [Fowler, Baker, and Dawes 2008], personality [Gerber et al. 2011], political socialization [Campbell et al. 1960], etc.), campaign effects (e.g., campaign mobilization [Rosenstone and Hansen 1993], negative campaigning [Ansolabehere and Iyengar 1995], etc.), social pressure (Gerber, Green, and Larimer 2008), and political institutions (e.g., the rules for translating votes into seats [Powell

1986], restrictions on who and how individuals may register and vote [Wolfinger and Rosenstone 1980], and the process of voting itself like the availability and location of polling locations [Brady and McNulty 2011]). Although the effects of *de jure* electoral institutions such as registration rules have been investigated, the effects of actual citizen beliefs about the institutions, which may be in conflict with *de jure* reality, have been the subject of less focus.

In a recent paper however, Gerber et al. (forthcoming) suggest that formal rules about ballot secrecy may not be sufficient to allay citizen concerns about the voting process. That paper focuses on the implications for vote choice if citizens are concerned about the anonymity of the voting process. Gerber and colleagues contend that the decisions people make in the voting booth may be affected by social or group influences if they believe those choices may be revealed (through either monitoring of how they fill out their ballot or, after the fact, by matching the cast ballot to their name), a common factor noted in explaining voter decision-making in the pre-secret ballot era (e.g., Bensel 2004; Bishop 1893). Similarly, Karpowitz et al. (2011) find that members of local political minorities perceive greater threats to privacy when voting, and Claassen et al. (2008) find that poll workers are evaluated less favorably in places where privacy protections are perceived to be weaker.

What has received less attention in prior work is that fears about a lack of secrecy may also deter political participation.[4] Although previous research suggests that the social consequences of failing to participate may play an important role in encouraging turnout (e.g., Gerber, Green, and Larimer 2008; Gerber et al. 2012a), if citizens have doubts about the voting process, then the perceived threat of social consequences may also *depress* turnout. For example, citizens may worry that they would suffer direct sanctions for the choices they make when voting if those choices are revealed and instead choose to shield themselves from these sanctions by staying home on Election Day. Similarly, doubts about secrecy may

[4] Heckelman (1995) contends that the adoption of the secret ballot in the U.S. may have diminished turnout historically because, without the ability to verify vote choices, secrecy eliminated the ability of parties and candidates to offer payments (bribes) to voters for voting for a particular candidate.

cause voters to think that the process of voting involves direct conflict, possible embarrassment, or intimidation—that when voting they would have to justify their choices or would be challenged by those who disagree with them. Such potential conflict or stress can be avoided by abstention. Outside of the context of ballot secrecy perceptions, Mutz (2002) documents a pattern consistent with this argument, showing that exposure to political disagreement depresses turnout, particularly for those who are conflict avoidant (see also Ulbig and Funk 1999).

Although views about a lack of ballot secrecy are expressed in survey data by around one quarter of the population (Gerber et al. forthcoming), a key question is whether these beliefs are real and consequential. On the one hand, survey responses indicating doubts about secrecy, because they are costless to express, may be efforts by non-voters to justify lack of participation. On the other hand, those beliefs may be real but uncorrectable or irrelevant—even after being told that voting is secret, these citizens may not change their attitudes or might still stay home for other reasons. We investigate both possibilities.

New Survey Evidence on Beliefs about Ballot Secrecy

Building on Gerber et al. (forthcoming), we fielded a survey after the 2010 midterm election to assess citizens' beliefs about the process of voting, both generally and with respect to ballot secrecy concerns.[5] The results suggest that concerns about ballot secrecy are particularly widespread among those who have never voted. The survey asked respondents who reported having ever voted a series of questions about their experience the last time they voted. Those who had never voted before were asked analogous questions about their expectations for what the voting experience would be like if they were to vote. In Figure 1, we summarize responses to four of the questions we asked, which are representative of our entire set of questions. The figure displays the proportion of respondents (weighted to reflect a

[5] Polimetrix/YouGov completed on our behalf a survey of a nationally representative sample of 3,000 citizens 25-years and older in the month following the November 2010 election. Please see the Supporting Information (SI) for complete information about sample construction.

national sample) who responded "Yes" (white part of the bars), "Don't Know" or "Don't Remember" (light gray part of the bars), and "No" (dark gray part of the bars) to each statement, broken down by whether they reported having ever voted.[6]

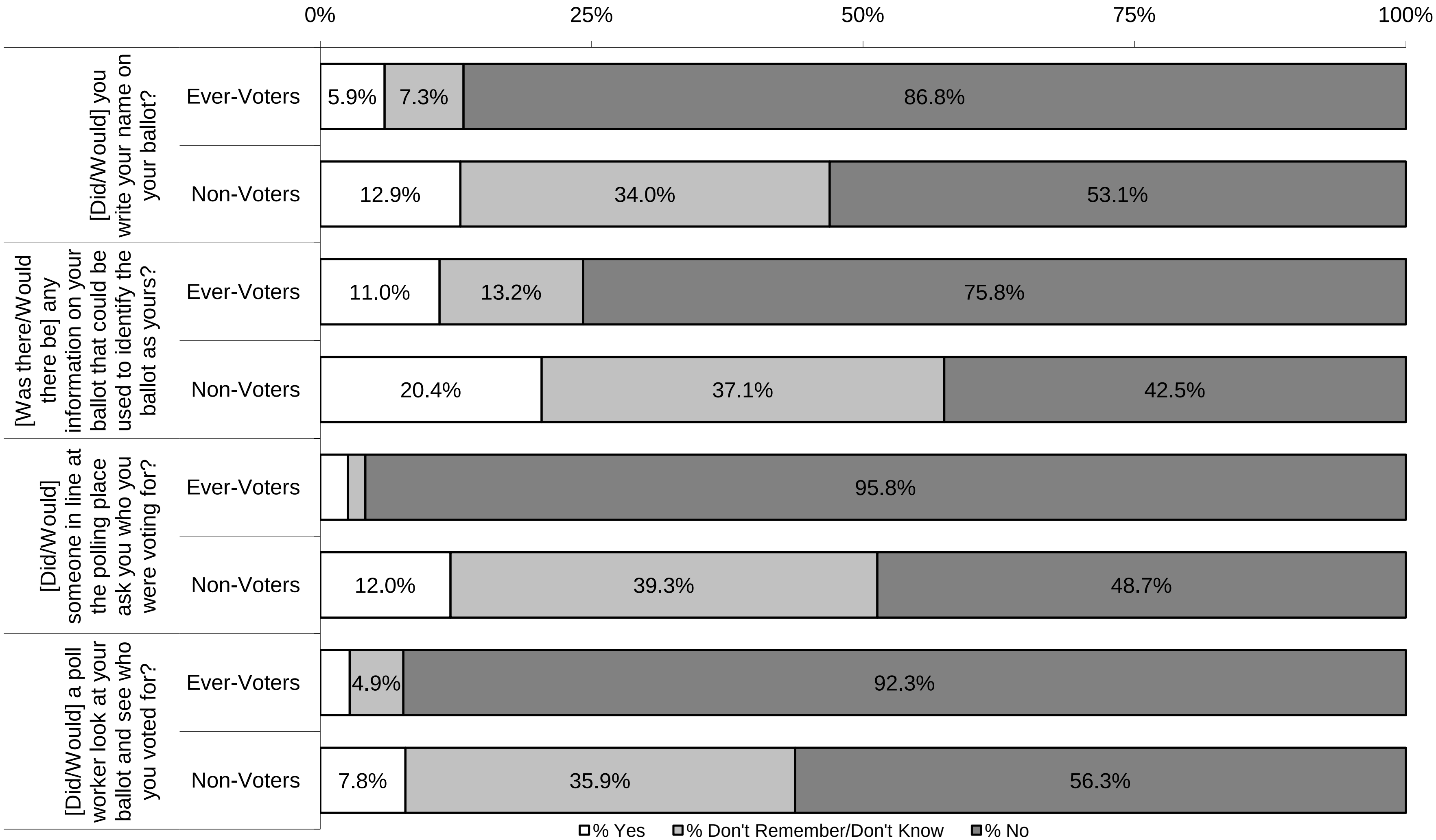
[Figure 1 About Here]

These data show that those who report never having voted before are more likely than those who report having voted to believe (1) that they would write their name on their ballot (13 versus 6% among all respondents), (2) that some information on their ballot could be used to match it to them after the fact (20 versus 11%), (3) that someone at the polling place would ask them who they voted for (12 versus 3%), and (4) that a poll worker would be able to see who they voted for (8 versus 3%). We discuss which of these fears are addressed by our different treatment interventions below. All differences in responses between the two groups are statistically significant at $p < .05$. We also note that for each item, over one-third of non-voters gave a "Don't Know" response, and that these responses could suggest doubts and uncertainty about the secrecy of the voting process.[7] In this light, if we restrict our attention to those who offered a "Yes" or "No" response to the questions displayed in Figure 1 the differences between Ever-Voters and Non-Voters are even more stark. Specifically, the percentages responding "Yes" (of "Yes" and "No" responses) to each of the four items are for Never-Voters and Ever-Voters, respectively, (1) 20 versus 6%, (2) 32 versus 13%, (3) 20 versus 3%, and (4) 12 versus 3%. In sum, the findings presented in Figure 1 broadly suggest that those who do not have experience voting are substantially less likely to believe that the ballots voters cast are anonymous and more likely to believe that voters may divulge their

[6] Among ever-voters, the sample is restricted to those whose last reported vote was in person (early or on Election Day). Nineteen percent of survey respondents reported voting absentee, by mail, or could not recall how they last voted.

[7] The discussion in the remainder of this paragraph assumes that "Don't know" responses represent actual uncertainty, not lack of engagement with the survey or uncertainty about the meaning of a question.

Figure 1: Beliefs About Ballot Secrecy Among Ever-Voters and Non-Voters



Source: 2010 Ballot Secrecy Survey. Cell entries are weighted percentages. Ever-Voters include those whose last reported vote was in person (early or on Election Day); 19% of survey respondents reported voting absentee, by mail, or could not recall how they last voted and are excluded from this analysis. Empty cells are 2.6% Yes and 1.6% DR/DK for Ever-Voters to "...ask you who you were voting for?" and 2.7% Yes for Ever-Voters to "...see who you voted for?". All differences in distributions of responses between Ever- and Non-voters are statistically significant at p<.05 (test-statistic calculated with a weighted multinomial logit regression predicting responses with an indicator for Non-voters, robust [Huber/White] standard errors). N ranges from 2418 to 2429.

choices to people they encounter at the polling place. These perceptions, which are interesting in their own right, may help to explain their lack of participation on Election Day.

An extant experimental literature highlights the importance of social and experiential concerns, like the ones we suggest may operate here, in explaining participation. For example, Addonizio (2004) shows that exposing inexperienced voters (high school students) to the process of voting by having them walk through a mock polling place and practice voting increases subsequent turnout. This effect is consistent with non-voters being dissuaded from participating by lack of knowledge of the process, which may include beliefs that one's vote would not be kept secret. Other work finds that making the negative social consequences of being revealed as not having voted more salient increases participation (e.g., Gerber, Green, and Larimer 2008).This focus on revealing whether someone has complied with a positive social norm is in contrast to a field experiment reported in Grose and Russell (2008). Grose and Russell find that telling voters before the Iowa Caucus that their vote in the caucus is not secret reduces turnout by about 22 percentage points relative to an identical treatment that lacks information about voting not being secret (N=232 for these two treatments). Among potential caucus goers, this suggests that being reminded of the potential for open disagreement reduces turnout. What remains unclear from this research is whether directly addressing doubts about ballot secrecy in non-caucus settings would alter voter behavior.

II. Experimental Design

In order to investigate whether beliefs about ballot secrecy affect the decision to participate, we designed and implemented a randomized field experiment in Connecticut during the 2010 midterm election. To assess the particular importance of beliefs about ballot secrecy on turnout, we implemented a variety of interventions, each of which was a mailing that conveyed different information. The design included both a non-treated (no mailing) control group and a set of placebo treatments, which we detail below. Our outcome of interest is participation in the 2010 general election held on November 2, 2010. We measure participation using turnout as recorded in the Connecticut voter file.

The theory and survey evidence outlined above suggest that individuals who have not previously participated are most likely to hold beliefs about ballot secrecy that would deter participation. To the extent that these beliefs are incongruent with the experiences of voters and the practice of professionalized election administration, we hypothesize that delivering corrective information might reduce concerns about the secrecy of the voting process and increase the likelihood of voting. For this reason, we stratified our experimental design to allow us to more precisely estimate the effects of information about ballot secrecy on those active Connecticut registrants who have not previously voted. We also assess the effects of our treatments on those who had previously voted to test our assumption that beliefs about secrecy were most important for current non-voters.

The experiment proceeded in four stages, each of which is detailed in Table S1 in the SI. In Stage 1 we identified our eligible sample, beginning with a list of all voters in the Connecticut voter file produced by the Connecticut Secretary of State in June 2010. From this list we removed all individuals who 1) were listed as inactive, 2) lacked a valid current Connecticut mailing address (including failing to pass a National Change of Address [NCOA] filter), or 3) had their mail delivered to a post office box.[8] The first restriction removes registrants who were unlikely to vote. The latter restrictions focus on making sure recipients would be likely to receive a mailing sent in the days preceding the election. Similarly, we eliminated any voter who voted by absentee ballot in 2006, 2008, or 2010 because many of these individuals may have already voted at the time the mailing was sent. We also removed any registrants less than 18 years of age or over the age of 85 out of concern that they would be unlikely to participate. Finally, we removed records from households with more than five registrants at a single address because

[8] The United States Postal Service provides a National Change of Address service that checks a name and address against the USPS list of changed addresses. This service, provided for fees by licensees, helps reduce undeliverable mailings, though not all movers take advantage of the service. To the extent there are still movers in our target sample and we sent them mailings they did not receive, they cannot be effective.

these individuals are likely to live in temporary (e.g., school dorms) or group housing where mail would be less likely to be delivered on time, or where the registrant is more likely to have moved. From this pool of eligible registrants, if there were multiple eligible registrants from a single household (address), we randomly selected a single registrant. This sampling procedure yielded 894,791 eligible registrants in unique households.[9]

In Stage 2 we identified our three experimental strata and assigned treatments. Each stratum is a stand-alone group of registrants on which the experiment is run, allowing us to estimate separate treatment effects for each group. Our first stratum is recent registrants—those who registered after the 2004 general election—who had never voted (no record since 1999). We label this stratum *recently registered non-voters.* The second stratum is earlier registrants (registered prior to the 2004 general election) who had also never voted. We label this stratum *longstanding registered non-voters.* We believe that those who had not previously participated are most likely to hold concerns about election administration that deter participation. However, we separate these non-voters into two groups for reasons of efficiency: Those who registered prior to 2004 but have never voted are less likely to reside now at the address listed on the voter file.

We define our third stratum as those who were listed to have ever voted in any election in the voter file. We label this stratum *ever-voters*. *Recently registered non-voters* make up about 9 percent of our targeted sample, *longstanding registered non-voters* about 9 percent, and *ever-voters* the remaining 82 percent. Although we ran all registrants for the first two strata through the NCOA list, for reasons of cost we randomly selected a subset of *ever-voters* to verify their addresses. Our sample of registrants that passed the NCOA check is 69,488 *recently registered non*-voters, 68,859 *longstanding registered non-*

[9] It is important to note that the treatment effects we identify in this experiment are unbiased only for the population we have created through these sample filters. That is, we cannot predict the potentially larger (or smaller) treatment effects that we might find for inactive registrants, absentee voters, etc.

voters, and 18,586 *ever-voters*. We present summary statistics by stratum of covariates in the voter file in Table S2 in the SI.

In Stage 3, we randomly assigned each registrant to one of eight conditions within each stratum.[10] Each treatment was a letter, described in detail below. Letters were mailed on October 28 to arrive between October 29 and November 1, 1 to 4 days prior to the election. We sent a total of 16,556 treatment letters. In order to enable detection of small effects in our primary target stratum (*recently registered non-voters*) in the presence of resource constraints, we significantly under-weighted the share of the treatments sent to *longstanding registered non-voters* in favor of treating a larger share of the recently *registered non-voters*. We allocated approximately 32 percent (5,357) of our treatments to the *recently registered non-voters* stratum (a within-stratum treatment rate of 7.7 percent). We assigned 5 percent (836) of the treatments to the *longstanding registered non-voters* stratum (a treatment rate of 1.2 percent) and 63 percent to the much larger *ever-voters* stratum (a treatment rate of 55.8 percent of the *ever-voters* checked against the NCOA list). Balance statistics (summary statistics for all demographic variables) for control and each treatment group in all strata appear in Table S3 in the SI.

Description of Treatments

We partnered with the Connecticut Secretary of State to send informational letters to our sampled registered voters. Six of our seven treatment letters appeared on Secretary of State letterhead (full text of all letters appears in the SI). Our primary interest is in how perceptions of ballot secrecy influence participation, and three of our treatment letters addressed concerns about ballot secrecy. The first, *SOS Secrecy 1 (Anonymity)*, was sent on Secretary of State letterhead and emphasized how the choices a voter made in the voting booth would be kept secret with the following text:

> **We maintain the secrecy of the ballot.** 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 other than your anonymous ballot. Your 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.

[10] Within each stratum, randomization took place within blocks. See the SI for complete details.

This letter is designed primarily to address doubts about whether a voter's name is linked to her ballot, and can therefore be viewed by election administrators or elected officials, as well as the possibility that poll workers or other voters can observe the ballot when it is being cast.

The second treatment letter, *SOS Secrecy 2 (No Intimidation)*, was similar to the first intervention but emphasized a different element of secrecy in the voting process. It sought to ameliorate concerns that elected officials, election administrators, or even other voters may seek to pressure a voter to change her mind or reveal her choices. In place of the paragraph listed above, it read:

> **We make sure that you can vote free from intimidation.** 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 who you voted for, and campaigning is prohibited inside of or near polling places.

Finally, the third secrecy intervention, *SOS Secrecy Combined*, included both paragraphs of text from the *SOS Secrecy 1 (Anonymity)* and *SOS Secrecy 2 (No Intimidation)* treatments. Because each argument addresses a different area of concern, it was our expectation that the combined effect of this mailing would be larger than the effect of each of the two interventions alone. This intervention also allows us to test for a dose-response pattern in the treatment effects; dose-response experimental designs are a commonly-used approach to validating the causal effect of a treatment. The basic idea is that, if the treatment has an effect on the outcome of interest, then multiple delivery of the treatment should increase the estimated size of the effect, either by increasing individual-level response, or by increasing the probability of receiving the intervention. In our case, the design allows us to see if the delivery of two paragraphs addressing secrecy concerns increases the probability of turnout relative to the delivery of one paragraph.

Because of random assignment, comparing individuals assigned to the control group with those sent each of the letters that addresses secrecy concerns yields an unbiased estimate of the effect of those interventions. However, one concern is that if these letters are effective in increasing turnout, it may not be because of their particular content concerning ballot secrecy. Each letter was addressed to the individual registrant on Secretary of State letterhead and also included information that there was an election occurring on November 2, 2010. In order to help isolate the effects of information about ballot secrecy from the effect of receiving election-related mail from the Secretary of State, we also fielded two “placebo” treatments. *SOS Short*, which was also sent on Secretary of State letterhead, is identical to the three secrecy letters but lacks any text addressing concerns about ballot secrecy. Because it lacks any text addressing ballot secrecy concerns, the *SOS Short* letter is shorter than any of the secrecy letters. To ensure that any differences in treatment effects were not due to the amount of text, we also fielded *SOS Control*, which is a longer version of *SOS Short* with information about the role of the Secretary of State in election administration unrelated to ballot secrecy. This text was produced based on information available on the office of the Connecticut’s Secretary of State’s public website. We note that, like the secrecy interventions, this letter makes clear the SOS office’s interest in election administration.

Our last two treatments are the *SOS Civic Duty* and *Generic Civic Duty* letters. *SOS Civic Duty* adds to the *SOS Short* letter standard text focusing on the civic duty associated with voting (see SI). This is a standard GOTV intervention and is typically associated with a less than one percentage point increase in turnout in midterm elections (Green and Gerber 2008). As such, we use it to compare the effects of the secrecy interventions to a more standard GOTV appeal. The text of *Generic Civic Duty* is identical, but is sent under cover of “Connecticut Votes” rather than the Secretary of State.[11]

We gathered information about participation in the 2010 election from an updated Connecticut voter file from February of 2011. For participants listed in this voter file, turnout was coded 1 if the respondent voted in the 2010 general election and 0 if not. For respondents who were no longer listed in the updated voter file, turnout was coded 0.[12] In Figure S1 in the SI, we present a diagram describing the design of the experiment from sample definition through population filters and random assignment.[13]

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[11] Comparing these last two treatments allows us to assess whether there are any increased mobilization effects associated with mail delivered under the Secretary of State’s letterhead relative to a non-governmental GOTV group, an analysis we conduct in other work.

[12] Excluding the 507 out of 894,791 registrants in our target population who did not match to the post-

III. Analysis

Our analysis proceeds in two stages. We begin by comparing, for each stratum, the effect of receiving any of the individual secrecy mailings to being in the control group, to which no mail was sent. This analysis employs the entire sample for which treatment randomization took place. Next, we compare the effect of receiving any of the three secrecy mailings to the effect of the other (placebo) SOS mailings and to the civic duty mailings. These comparisons showing the relative effectiveness of the secrecy messages are necessarily limited only to those sent letters. Our results show that informing voters of the formal institutional protections of ballot secrecy increased participation among registrants who had not previously voted, but had negligible effects on those who had previously voted. The positive estimated effects of the secrecy intervention among inexperienced voters hold when comparing them to the control group and to either a civic duty intervention or to a placebo intervention with non-secrecy content. Note that the treatment effects estimated in these analyses are a combination of the rate with which the treatments were complied with (i.e. letters were opened and read by their intended recipient) and the treatment's effectiveness (i.e., the effect on turnout when opened and read).

The Effects of Providing Information about Ballot Secrecy Protections

In Table 1, we present 2010 turnout by control and treatment assignment for each stratum. As one might expect given the habitual nature of voting and the definitions we use for our three strata, in the control group we find that turnout is 62.6 percent among *ever-voters*, substantially higher than *recently*

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election file, rather than coding their turnout as zero, does not substantively change any of the results presented. We focus on the analysis coding missing voters as non-voters because our treatments could have affected the probability a voter was later removed from the rolls.

[13] We collected and recorded each mailed treatment letter returned as undeliverable by the US Postal Service—780 of 16,556 (4.7% of) mailed letters were returned. Analysis restricted to those respondents whose mail (i.e., excluding the control group, comparing across treatments) was not returned produces similar results. Results are available upon request.

registered non-voters (17.0 percent), which is higher still than turnout among *longstanding registered non-voters* (13.2 percent). Table 1 also indicates that the secrecy interventions increased turnout. *Recently registered non-voters* sent a secrecy letter voted in 2010 at rates 2.8 to 4.1 percentage points higher than those assigned to the control condition (a proportional increase of 16 to 24 percent for this stratum). Among *longstanding registered non-voters,* turnout is .2 to 4.3 percentage points higher for those sent secrecy letters than those in the control group (a proportional increase of 2 to 33 percent). The effect is .1 to 1.2 percentage points among *ever-voters* (0 to 2 percent proportional). In short, the effects of the secrecy letters appear to be larger for non-voters.

[Table 1 About Here]

By contrast, the effects of the two placebo letters, *SOS Control* and *SOS Short*, are small for both *recently registered non-voters* (-.2 and -.7 percentage points, respectively) and *longstanding registered non-voters* (-3.1 and 1.1 percentage points, respectively). Among the *ever-voters* stratum, these two treatments each have a 2.4 percentage point effect relative to the control group. The *SOS Civic Duty* and *Generic Civic Duty* treatments also present modest effects relative to the control group across strata: 1.2 and .6 percentage points among the *recently registered non-voters*, -2.0 and -2.5 for *longstanding registered non-voters*, and -.5 and 1.6 for *ever-voters*.

We are able to detect a positive and statistically significant effect of the secrecy interventions on turnout in the *recently registered non-voters* stratum in our experiment. Difference-of-means tests are statistically significant for all three secrecy letters relative to the control condition (p=.04, .002, and .009 for *SOS Secrecy 1 (Anonymity)*, *SOS Secrecy 2 (No Intimidation)*, and *SOS Combined*, respectively). We find no statistically distinguishable difference among *longstanding registered non-voters* or *ever-voters*, though the positive point estimates for longstanding registered non-voters are similar in average magnitude to those for the recently registered non-voters.[14] Due to the uncertainty of those estimates,

[14] The p-values on difference of means tests comparing any of the three secrecy interventions (e.g., pooling the three treatments to form a single group) to either of the SOS Placebo interventions (pooling

Table 1: 2010 Counts and Turnout Percentage by Control and Treatment Assignment by Stratum

Assignment	Stratum: Recently Registered Non-Voters	Longstanding Registered Non-Voters	Ever-Voters
Control Group	17.0%	13.2%	62.6%
	(0.1)	(0.1)	(0.5)
	64,131	*68,023*	*8,223*
Treatment: SOS Secrecy 1 (Anonymity)	19.8%	13.4%	62.7%
	(1.4)	(3.1)	(1.2)
	824	*119*	*1,564*
Treatment: SOS Secrecy 2 (No Intimidation)	21.1%	14.8%	63.8%
	(1.4)	(3.1)	(1.2)
	812	*135*	*1,569*
Treatment: SOS Secrecy Combined	20.5%	17.5%	63.7%
	(1.4)	(3.6)	(1.2)
	819	*114*	*1,582*
Treatment: SOS Short (Placebo)	16.3%	14.3%	64.9%
	(1.4)	(3.4)	(1.3)
	652	*105*	*1,275*
Treatment: SOS Control (Placebo)	16.8%	10.1%	65.0%
	(1.5)	(3)	(1.4)
	637	*99*	*1,249*
Treatment: SOS Civic Duty	18.2%	11.2%	62.1%
	(1.3)	(2.6)	(1.2)
	822	*143*	*1,555*
Treatment: Generic Civic Duty	17.6%	10.7%	64.2%
	(1.4)	(2.8)	(1.2)
	791	*121*	*1,569*

Note: Cell entries are percentage voting as recorded in CT Voter File, standard errors in parentheses calculated from test of sample proportion, with counts in italics. Registrants not matched to post-election file counted as non-voters.

which likely arises from the sparseness of our treatment assignments in that stratum, we cannot eliminate either the possibility that those treatment effects are 0 or that they are the same as the treatment effects estimated for the recently registered non-voters stratum. We now turn to regression models to formalize these differences and to test for robustness to the inclusion of pre-treatment covariates, but note that regression results are consistent with the simple differences of means.

In Table 2 we estimate parallel OLS regression models with robust (Huber/White) standard errors. Our basic models estimate the effects of the randomly assigned treatments with no control variables (odd numbered columns), with a subsequent specification using measures from the voter file as covariates (even numbered columns).[15] In columns (1)-(6), the excluded category is assignment to the control condition—individuals in our target population randomly assigned to receive no mailing of any kind.

Focusing first on the results for *recently registered non-voters* in column (1), we find that the secrecy intervention increased turnout by between 2.8 and 4.0 percentage points relative to turnout in the control group with all three coefficients statistically significant at p<.05. No other treatments have statistically significant effects in this stratum, and each of the secrecy interventions has a larger effect than the effect of any of the other treatments.[16] The results are virtually unchanged with the addition of covariates from the voter file in column (2).

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the placebo treatment groups into a single group) are .003 (*recent registered non-voters*), .330 (*longstanding registered non-voters*), and .181 (*ever-voters*).

[15] For space reasons, Table 2 does not report the coefficients and standard errors for the voter file measures; Table S4 in the SI presents the full model results.

[16] The *SOS Civic Duty* and *Generic Civic Duty* treatments are associated with 1.2 and .5 percentage point increases in turnout, consistent with the results of published field experiments. These effects are not statistically distinguishable from zero in our sample, however.

Our *longstanding registered non-voters* stratum has a much smaller number of cases assigned to each treatment condition. The results reported in columns (3) and (4) reflect the small sample sizes with large standard errors. The pattern of findings is consistent with the notion that, as with the *recently registered non-voters*, messages based on secrecy concerns are effective for this subgroup. However, the estimates are imprecise. The only statistically significant (p<.10) coefficient is for the *SOS Secrecy Combined* treatment, with a point estimate of 5.7 percentage points in column (4). The other secrecy treatments have smaller positive point estimates—from 1.3 to 2.3 percentage points in column (4). The largest point estimate for any other treatment is less than 1 percentage point.

Finally, among *ever-voters* (columns [5] and [6]), the only statistically significant (p<.10) effect is a 2.4 (column [5]) to 2.5 (column [6]) percentage point increase in turnout for the *SOS Short* intervention. The secrecy interventions are estimated to increase turnout by between .2 and 1.3 percentage points in column (6), but none of these effects are statistically distinguishable from zero.

[Table 2 About Here]

Overall, these results suggest that the secrecy letters increased the propensity to vote among those citizens who are legally registered to turn out but have no record of previously exercising that right (members of both the *recently registered non-voters* and the *longstanding registered non-voters* strata). Relative to not being sent any intervention, mailings that emphasized the protections available for ballot secrecy increased turnout by a statistically significant 16 to 24 percent among *recently registered non-voters*, with similar but less precisely estimated effects among *longstanding registered non-voters*. For those who had ever voted, the secrecy interventions had much smaller effects that are statistically indistinguishable from zero. The question remains as to whether these effects arise due to secrecy concerns per se, or are instead the result of the fact that our interventions involved contact from an important government official and conveyed information about the upcoming election. Our next set of comparisons focuses on differences in outcomes between the secrecy interventions and the other interventions.

Table 2: OLS Regressions Predicting 2010 Turnout by Stratum

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Stratum:	Recently Registered Non-Voters		Longstanding Registered Non-Voters		Ever Voters		Recently Registered Non-Voters, Pooled Secrecy vs. Placebo		Longstanding Registered Non-Voters, Pooled Secrecy vs. Placebo		Ever Voters, Pooled Secrecy vs. Placebo	
Included Assignments	All Assignments and Control						Restricted to those assigned to Placebo (SOS Short and SOS Control) or Any Secrecy (SOS Secrecy 1, SOS Secrecy 2, SOS Secrecy Combined)					
Comparisons:	To Control (Omitted Category)						To Placebo Treatments (Omitted Category)					
Treatment: SOS Secrecy 1 (Anonymity)	0.028	0.028	0.002	0.013	0.001	0.002						
	[0.014]**	[0.014]**	[0.031]	[0.030]	[0.013]	[0.012]						
Treatment: SOS Secrecy 2 (No Intimidation)	0.040	0.035	0.016	0.023	0.012	0.010						
	[0.014]***	[0.014]**	[0.031]	[0.028]	[0.013]	[0.012]						
Treatment: SOS Secrecy Combined	0.035	0.038	0.043	0.057	0.011	0.013						
	[0.014]**	[0.014]***	[0.036]	[0.034]*	[0.013]	[0.012]						
Any Secrecy Treatment							0.039	0.038	0.030	0.044	-0.016	-0.014
							[0.013]***	[0.013]***	[0.030]	[0.029]	[0.012]	[0.011]
Treatment: SOS Short (Placebo)	-0.008	-0.008	0.011	0.009	0.024	0.025						
	[0.015]	[0.014]	[0.034]	[0.031]	[0.014]*	[0.013]*						
Treatment: SOS Control (Placebo)	-0.002	-0.001	-0.031	-0.028	0.024	0.020						
	[0.015]	[0.015]	[0.030]	[0.029]	[0.015]*	[0.013]						
Treatment: SOS Civic Duty	0.012	0.010	-0.020	-0.006	-0.004	-0.009						
	[0.014]	[0.013]	[0.026]	[0.025]	[0.013]	[0.012]						
Treatment: Generic Civic Duty	0.005	0.005	-0.025	-0.031	0.016	0.016						
	[0.014]	[0.013]	[0.028]	[0.029]	[0.013]	[0.012]						
Constant	0.170	0.185	0.132	0.132	0.626	0.154	0.165	0.281	0.123	-0.019	0.650	0.277
	[0.001]***	[0.011]***	[0.001]***	[0.010]***	[0.005]***	[0.036]***	[0.010]***	[0.058]***	[0.023]***	[0.050]	[0.009]***	[0.060]***
Observations	69488	69488	68859	68859	18586	18586	3744	3744	572	572	7239	7239
R-squared	0.000	0.046	0.000	0.099	0.000	0.191	0.002	0.052	0.002	0.137	0.000	0.178
F-Test p-value on joint significance of three Secrecy Treatments	0.001	0.001	0.621	0.304	0.700	0.657	0.003	0.003	0.319	0.122	0.180	0.187
F-Test p-value on joint significance of two SOS Placebos	0.858	0.845	0.564	0.590	0.087	0.070	n/a	n/a	n/a	n/a	n/a	n/a
Covariates included?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Note: Results from OLS Regressions with dependent variable 2010 turnout (1=yes, 0=no or no record in post-election voter file). Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. Full model results, including covariates, available in Table S4. Covariates are turnout indicators for 2004, 2006, and 2008, age, age squared, gender, party of registration, number in household indicators, and town indicators for six largest Connecticut towns (Bridgeport, New Haven, Stamford, Hartford, Waterbury, and Norwalk).

Ballot Secrecy Effect Relative to Other Letters

To isolate the effects of the content of the different treatment letters, we now examine differences in turnout between the different treatment letters. In columns (7) through (12) of Table 2, we repeat our earlier statistical analysis but now include only those respondents sent one of the secrecy interventions or either of the placebo interventions sent on SOS letterhead (*SOS Short* and *SOS Control*). As there is no statistically significant difference between the two SOS placebo treatments in any strata in columns (1)-(6), we pool these treatments and use them as the excluded comparison category (we refer to those pooled treatments as *SOS Placebo*). Additionally, we pool the three secrecy treatments (*SOS Secrecy 1 (Anonymity)*, *SOS Secrecy 2 (No Intimidation)*, and *SOS Secrecy Combined*) into *Any Secrecy Treatment*, coded 1 for an individual assigned to any of the three secrecy interventions. Thus, the coefficient for *Any Secrecy Treatment* is the average effect of assignment to any of the three secrecy letters relative to the average effect of assignment to either *SOS Placebo*.

Comparing *Any Secrecy Treatment* to *SOS Placebo* is particularly compelling because the two placebo messages allow us to distinguish the effect of the secrecy content from both (1) contact by the Secretary of State and (2) the communication of information about the upcoming election and the Secretary's efforts in overseeing and administering election rules. The statistically significant .039 and .038 coefficients (p<.01) on *Any Secrecy Treatment* in columns (7) and (8) indicate that among the *recently registered non-voters* the average effect of the secrecy interventions was to increase turnout by about four percentage points relative to the average effect of the two SOS placebo messages. Overall, this is a substantial increase in turnout for a relatively straightforward set of interventions that communicate factual information about the voting process to inexperienced voters. Additionally, that the secrecy interventions each have larger point estimates than either the *SOS Civic Duty* or *Generic Civic Duty* messages (in columns [1] and [2]) implies that addressing secrecy concerns is not increasing turnout simply by making voters believe they have a greater obligation to vote.

Turning to the other two strata, among the *longstanding registered non-voters* (columns [9] and [10]) the effect of *Any Secrecy Treatment* is roughly similar to that for *recently registered non-voters*, 3.0

in column (9) without covariates and 4.4 percentage points in column (10) with covariates, but is not statistically distinguishable from the effects of the placebo treatments.[17] Finally, among *ever-voters*, the effect of *Any Secrecy Treatment* is a negative 1.6 (1.4) percentage points relative to the placebo messages in column (11) (column [12]), but this effect is also not statistically significant (p=.18 and .19). The effects among *recently registered non-voters* appear distinct from those among *ever-voters*, with confidence intervals on the effects that do not overlap.[18] It is interesting that for experienced voters, we find that the two placebo messages are associated with the largest increase in turnout relative to the control (see columns [5] and [6]).[19]

Finally, we consider whether there is any evidence that the effect of the secrecy interventions follows a dose-response pattern among *recently registered non-voters* (small cell sizes make comparisons for *longstanding registered non-voters* infeasible). Direct inspection of the coefficients in column (2) of Table 2 suggests that the combined effects of the two secrecy arguments from *SOS Secrecy 1 (Anonymity)* and *SOS Secrecy 2 (No Intimidation)* in *SOS Secrecy Combined* do not generate a cumulative effect equal to the sum of the individual messages. The combined message increases turnout by only 1 percentage point relative to *SOS Secrecy 1 (Anonymity)* and only .3 percentage points relative to *SOS Secrecy 2 (No*

[17] Pooling both *longstanding* and *recently registered non-voters* together the effect of *Any Secrecy Treatment* relative to *Placebo* is 3.8 percentage points with a standard error of 1.2, p<.01 (two-tailed).

[18] Ninety-five percent confidence intervals for the *Any Secrecy Treatment* coefficient in the non-covariate specifications in columns (7) and (11) of Table 2 are [1.34, 6.51] and [-3.9, 0.73], respectively.

[19] By contrast the *SOS Civic Duty* message is associated with a -.9 percentage point decrease in turnout relative to control, which is a -3.3 (with rounding) percentage point effect relative to the *SOS Short* placebo message (p=.043). The *SOS Civic Duty* letter is identical to the *SOS Short* letter with the addition of text emphasizing the civic duty associated with voting. We offer no speculation as to why experienced voters might have reacted negatively to this effort to promote the civic duty of voting, and given the number of available comparisons of this sort, some “effects” are expected by chance.

Intimidation). More formally, testing whether the coefficients on (*SOS Secrecy 1 (Anonymity)* + *SOS Secrecy 2 (No Intimidation)*) - *SOS Secrecy Combined* is equal to zero yields a *p*-value of .28. Thus, although we cannot rule out the possibility that the effect of the combined treatment is equal to the sum of the individual treatments, the sum of the coefficients on the *SOS Secrecy 1 (Anonymity)* and *SOS Secrecy 2 (No Intimidation)* is 2.6 percentage points greater than the coefficient on the *SOS Secrecy Combined* variable and 68% larger than that coefficient.

In sum, the regression results replicate the difference of means results in Table 1. We find that letters providing information about the formal and administrative protections of the ballot increase turnout for registrants without a previous record of casting a ballot. These effects are statistically significantly larger than the effects of other election correspondence from the Secretary of State; the effects are also larger than (but not statistically distinguishable from) the effects of standard civic duty appeals. When compared to a placebo intervention letter also sent from the Secretary of State about the election, but providing information about the election other than about the secrecy of the ballot, we find that the effect of the secret ballot treatment is limited to registrants who had not previously voted, while registrants who had previously voted appear to be no more responsive to secrecy information than to other election information from the Secretary of State.

IV. Discussion

In this research, we examined public perceptions of ballot secrecy and whether those registered voters most likely to have doubts about secrecy are more likely to vote after being provided with information about the voting process. Our survey evidence shows that despite the formal practice of government supervised and administered elections with longstanding protections for ballot secrecy, many Americans say they harbor doubts about the secrecy of the voting process. These doubts are particularly concentrated among those who have not voted before. The findings from the field experiment suggest a causal relationship between doubts about ballot secrecy and the decision to participate in elections in the contemporary United States.

We find that an intervention providing simple information about protections of ballot secrecy increased participation among recent registrants who have not previously participated. This intervention can be targeted directly to the registered non-voter population on whom we estimate it to be effective. The magnitude of this effect was substantial and larger than the effects of a standard GOTV mailing, such as the civic duty letters sent as part of this experiment. While only 17.0% of the individuals in our recently registered non-voter stratum who were not sent a letter turned out to vote, 20.4% of those sent a letter containing information about ballot secrecy protections turned out—a proportional increase of 20%. Taken together, our survey and field experiment are consistent with the argument that the beliefs about secrecy that people express in surveys are both real and somewhat remediable—when these doubts are addressed, participation increases. While a great deal of research has focused on decreasing the cost of voting, such efforts may be limited in their ability to attract to the polls voters who are concerned about factors apart from the administrative inconvenience of registering and casting a ballot.

More broadly, this work illustrates that an important area for research is to understand the sources and consequences of beliefs about the operation of political institutions. Our findings suggest that beliefs about how a political institution works can be politically consequential, even if they are at odds with the reality of how that institution operates. We note that our focus on beliefs about ballot secrecy in the context of elections in the contemporary United States is in many ways a difficult case. In countries with new electoral institutions, doubts about secrecy may be more prevalent and depress turnout, even in cases where the institutions are well-designed and properly implemented. Therefore, an especially promising avenue for research deals with beliefs about the operation of electoral institutions in other countries.

Similarly, our findings suggest that in order to understand how changes to election procedures in the U.S. and other long-standing democracies affect behavior, it is essential to consider how the public understands the new institution. Evidence from our field experiment suggests that sending letters that provide simple information about the secrecy of the voting process may correct errant perceptions about that process and thereby increase turnout among a group of individuals who are typically difficult to mobilize—those who are registered to vote but have not voted before. If comparable misperceptions about other institutional procedures exist, they too may have important consequences for patterns of participation. Remedying such misperceptions is particularly important given that these beliefs may be self sustaining—someone who chooses not to participate because they doubt the secrecy of the ballot effectively shields herself from learning about secrecy protections by failing to engage in the voting process.

The treatment effects we find originating in a single letter sent in the days before an election might be a lower bound relative to the effect of a more sustained and broad-based public education campaign. An area for subsequent research would be to examine the effect of embedding education about ballot secrecy into voter registration initiatives so as to directly address any mistaken beliefs ahead of time. One could also conduct standard opinion surveys before and after any such intervention to assess changes in these attitudes and measure the relative contribution of particular changes in secrecy attitudes (e.g., beliefs about whether poll workers, elected officials, or other voters are likely to learn about a vote choice or seek to influence it) to changes in participation. Indeed, one remaining question is whether doubts about secrecy are an impediment to registering in the first place. Our experiment only includes registrants. It could be that the interventions we used would be even more consequential among those who have previously been unwilling to register. Experiments designed to facilitate registration (e.g., Mann 2011) might be expanded to incorporate messages that address the doubts identified in this research.

Additional areas for ongoing research include whether the effects of addressing doubts about ballot secrecy generate a persistent change in behavior and affect other registrants in the voter's household. Regarding the former, if doubts about ballot secrecy do stem from lack of information about how elections are conducted, then addressing those concerns should permanently remove a persistent barrier to participation and yield long term changes in patterns of participation. Examining how our treatments and similar treatments affect participation in subsequent elections is a natural next step.

Finally, there are questions of generalizability and replicability. One should be cautious in assuming that similar results would hold in other states and in other electoral contexts (e.g., presidential or purely local elections). For example, in states that allow voting by mail or where a substantial share of voting does not take place at the polling place on Election Day (because early voting is allowed, unlike in Connecticut), concerns about violations of privacy in the polling place may not be an impediment to participation because voters can readily avoid polling places altogether.[20] Similarly, beliefs about secrecy may also vary, even among inexperienced voters, based on things such as educational efforts by state officials, generational experiences with technology, or observing shared public events that affect generational beliefs. Repeating this experiment in other states and including non-registrants is important to measuring the relevance of this effect. These caveats aside, the data presented here indicate that beliefs about ballot secrecy may be an important and unrecognized, yet potentially remediable, barrier to political participation in the United States.

[20] Approximately six percent of Connecticut residents voted early (by absentee) in 2006, whereas the national average was roughly 22 percent (see http://elections.gmu.edu/early_vote_2010.html; complete data for 2010 are not yet available). Gerber et al. (2012b) shows that voters who cast ballots by mail are more concerned than polling place voters that officials can link their ballot to their name, a pattern that is not surprising given that vote by mail ballots are placed in envelopes bearing the voter’s name.

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