

**If You Ask, They Will Come (to Register and Vote): Field Experiments with State Election Agencies on Encouraging Voter Registration**

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### **Abstract**

We address the frequent critique that voter registration is a barrier to participation in the US. Institutional reforms to voter registration produce only small impacts on participation. We show the registration barrier can be reduced without changing laws or administrative processes using official communication seeking to change individual political behavior. In collaboration with state election agencies in two states, we conducted large-scale field experiments using low cost postcards aimed at increasing registration among eligible but unregistered citizens. The experiments find statistically and substantively significant effects on registration and turnout in subsequent elections. The research partnership with election officials is unusual and important for understanding electoral participation. Further, the population targeted for registration is broader than prior experiments on voter registration in the US. The results provide important insights about voter registration as a barrier to political participation, plus practical guidance for election officials to reduce this barrier.

### **Keywords**

voter registration, voter turnout, election reform, election official, field experiment

It is a cliché that “getting to the starting line” is often more difficult than running the race, and this sentiment seems applicable for many American citizens when it comes to voting: The requirement to register is a costly and time-consuming obstacle to casting a ballot. In the United States, the first step in voting is registering with the local election administrator. Unlike many democracies, the burden of registration in the United States rests on the citizen rather than the government. Previous research including both cross-national analysis (Blais 2006; Cancela and Geys 2016; Jackman and Miller 1995; Powell 1986) and cross-state analysis (Ansolabehere, Hersh, and Shepsle 2012; Ansolabehere and Konisky 2006; Burden et al. 2014; Erikson 1981; Gay 2012; Hall 2013; Highton 2000; Kelley, Ayres, and Bowen 1967; Leighley and Nagler 2013; McDonald 2008; Mitchell and Wlezien 1995; Neiheisel and Burden 2012; Squire, Wolfinger, and Glass 1987; Wolfinger and Rosenstone 1980) has found the requirement to register in advance of the election reduces turnout. Despite widespread normative concerns about low participation in US elections, we know little about how to encourage citizens register or whether overcoming this barrier will lead to subsequent voting.

One approach to reducing the registration barrier to electoral participation is administrative change. Reforms to voter registration requirements have been implemented, at least in part, as an attempt to reduce the registration barrier to electoral participation. Past reforms include allowing citizens to register and vote at the polls on Election Day or during early in-person voting, moving registration deadlines closer to Election Day, allowing citizens to mail in voter registration forms, and providing voter registration through public agencies (especially driver’s license offices). More recently, states have attempted to reduce the registration barrier by implementing online voter registration to make registration more convenient (Hicks, McKee, and Smith 2016) and automatic voter registration to shift the burden of registration from citizens to the government (Griffin et al.

2017). While these reforms have had some success at increasing turnout, many citizens remain unregistered. Furthermore, many US states have not implemented any of these registration reforms (and do not appear likely to consider them in the foreseeable future).

Another approach to reducing the registration barrier uses communication to change political behavior without changing laws or administration of elections. Many civic and political organizations seek to encourage voter registration by conducting voter registration drives to promote broad participation in democracy or participation by their supporters. Although details of voter registration programs vary, the core mechanism of these programs is reminding citizens to register and reducing the difficulty of finding, completing and returning voter registration applications. This paper focuses on the whether the communication approach to increasing voter registration can be successful.

The two large-scale field experiments in this paper test the effect of state election agencies encouraging eligible but unregistered citizens to register to vote using postcards. Institutional reforms have had only modest effects, so there is reason to be skeptical that a postcard is sufficient change the behavior of unregistered citizens. However, mailings have generated significant increases in voting by registered voters, so postcards might also be able to increase registration of unregistered citizens. The experiments in this paper find clear support for the hypothesis that simple and inexpensive communication to eligible but unregistered citizens significantly increases registering to vote and increases turnout in upcoming elections. The results provide insights about political behavior, political communication, and the impact of voter registration requirements. The findings also have practical implications for election officials.

Over the last two decades, field experiments have substantially increased our understanding of individual level voting behavior. However, field experiments have focused

almost entirely on getting *registered* voters to turn out to vote (see Green and Gerber 2015 for a review). Studying only registered voters leaves out a major step of the voting process and major portion of the eligible but non-voting electorate in the US. Only a handful of published experiments examine encouraging *unregistered* citizens to vote (Addonizio 2011; Braconnier, Dormagen, and Pons 2017; Bennion and Nickerson 2011, 2014, 2018; John, MacDonald, and Sanders 2015; Nickerson 2007, 2015; Sweeney et al. 2018). Green and Gerber's (2015) book on voter participation field experiments describes several additional voter registration experiments fielded by civic organizations. These past experiments have produced mixed results that may be due to the treatment types, experimental population, or election type. Thus, the two experiments reported here add considerably to the existing literature: targeting the full population of eligible but unregistered citizens enhances the generalizability of the findings. Studying two different states in two different elections is an important replication of the efficacy of the same mechanism in different contexts. Studying voter registration encouragement from state election officials is novel in the study of voter registration in the US and rare in the study of voter participation more broadly, so the experiments presented here provide both theoretical insight and practical lessons.

The next section of the paper discusses relevant theory and past research on how to nudge eligible but unregistered citizens into registering. We then state the hypotheses to be tested in our experiments and describe the two large field experiments conducted in partnership with state election agencies in Delaware in 2012 and Oregon in 2014. (The Oregon experiment was conducted prior to that state's adoption of automatic voter registration for the 2016 election). The purpose of the experiments is to test whether communication from election officials can increase registration and turnout among eligible but unregistered citizens. We find postcards to eligible but

unregistered citizens cause significant increases in registration and turnout in subsequent elections. The paper concludes with a discussion of the implications of the findings.

## **1 Registration as a Barrier to Voting**

In the United States, it is up to citizens to learn how, when, and where to register. Under the hyper-federalized administration of elections in the US, the challenge of registration is compounded because the process and deadlines vary by state – and to some degree by county or township within states. Past research points to several aspects of the voter registration process as barriers to participation (Leighley and Nagler 2013). Historically, requiring registration was used to disenfranchise minorities (Keyssar 2009). Although many egregious practices have been banned (e.g. poll taxes, literacy tests, and flatly discriminatory refusals), the registration process still presents hurdles for eligible citizens seeking to register. Scholars have established that requiring voter registration prior to Election Day reduces registration and turnout (Ansolabehere, Hersh, and Shepsle 2012; Ansolabehere and Konisky 2006; Burden et al. 2014; Erikson 1981; Gay 2012; Hall 2013; Highton 2000; Kelley, Ayres, and Bowen 1967; McDonald 2008; Mitchell and Wlezien 1995; Neiheisel and Burden 2012; Squire, Wolfinger, and Glass 1987; Wolfinger and Rosenstone 1980).

The requirement to register in advance of Election Day makes voter registration is a major obstacle to participation. Many citizens who are not politically active do not start paying attention to electoral campaigns until the last month before the election (Fournier, et al. 2004), but a large majority of states have registration deadlines three to four weeks prior to Election Day. Citizens are more likely to miss registration deadlines when the deadline is farther from Election Day (Highton 2004; Patterson and Caldeira 1983; Rhine 1995; Street et al. 2015; Timpone 1998). Turnout increases when the registration requirement is removed by allowing voters to register at

the polls on Election Day or during early in-person voting (Rhine 1996; Brians and Grofman 2001; Burden et al. 2014; Hanmer 2009; Highton 1997, 2004; Neiheisel and Burden 2012; c.f. Keele and Minozzi 2013).

Some potential voters are deterred from registering by the perceived complexity and difficulty of the task itself, because they perceive the process to be difficult and/or do not know where to get a voter registration form or where to submit it once completed (Alvarez, Hall, and Llewellyn 2007).

Policy reforms intended to lower the barrier of advance registration and mitigate the perceived (and real) complexity of the registration process, especially as part of the National Voter Registration Act of 1993, had impacts smaller than reformers had hoped for (Hess, Hanmer and Nickerson 2016; Hanmer 2009; Highton and Wolfinger 1998; Highton 2004; Piven and Cloward 2000). In addition to policy and administrative changes, reformers have begun to test behavioral interventions to increase registration among eligible but unregistered citizens, such as those presented here.

## **2 Field Experiments on Voter Registration**

Field experiments about increasing voter turnout among registered voters have explored a wide range of communication tactics and psychological mechanisms (for a review, Green and Gerber 2015), but field experiments on voter registration are scarce. Moreover, inconsistency in the results of these voter registration experiments suggests more research is needed to understand how to increase registration. Evaluating the impact of voter registration drives using door-to-door canvassing, Nickerson (2015) found that streets randomly assigned to receive face-to-face canvas visits to encourage voter registration had 4.4 percent more new voter registrations than streets in the control group. The turnout of new registrants on treated streets also increased by an estimated

24 percent in the subsequent election. In a similar experiment in France (where voter registration is also up to citizens), Braconnier, Dormagen and Pons (2017) found that a two-round face-to-face canvass in low-turnout precincts significantly increased voter registration and voter turnout in the subsequent election. Bennion and Nickerson (2016) find that classroom presentations about voter registration across 16 college campuses increased registration by 6 percentage points and subsequent turnout by 2.6 percentage points. Earlier experiments by Addonizio (2011) also found significant increases in registration and turnout from presentations in high schools. Field experiments using email to encourage registration have produced the full range of possible outcomes. An initial experiment using an email based treatment among young citizens produced a null result (Nickerson 2007), then Bennion and Nickerson (2011) found that e-mail encouragement to register to vote resulted in a negative effect on registration among college students. A recent study found that emails with links to online registration had a larger positive effect on both registration and turnout than a link to downloadable registration form (Bennion and Nickerson 2018).

The findings from experiments using mailings to encourage registration are similarly mixed. Green and Gerber (2015, Chapter 10) report that mailings encouraging registration from civic organizations are successful when targeting specific populations, but may have little to no effect on registration and turnout in other contexts.

In the time since our experiments were conducted, two recent experiments in the UK have examined how electoral registration officers (EROs) can increase response rates to the annual registration canvass (John, MacDonald, and Sanders 2015; Sweeney et al. 2018). These experiments are similar to ours in exploring the effect of mailings from election officials, although the UK's system is quite different from the US: UK local government officials are required to



update the electoral rolls annually using mailings and going door-to-door to ensure the canvass is as “accurate and complete as possible” (Sweeney et al., p.3). Notably, this list is used for a variety of public purposes in addition to determining eligibility to vote. John, MacDonald, and Sanders (2015) test treatment mailings offering a chance to win a lottery (£1000 or £5000) if citizens responded to the local council’s annual voter registration update prior to a deadline. Comparing the lottery letters to a placebo letter with the same deadline, they find significant increases in response to the lottery treatment: +1.5 percentage points ( $p_{ate} < 0.001$ ) for £1000 lottery and +1.9 percentage point ( $p_{ate} < 0.001$ ) increase for £5000), but the effects are not statistically distinguishable. While they have interesting findings about financial incentives, using the placebo letter as the baseline means they provide no insight about election officials simply encouraging registration (a required activity in the UK, but an unusual step for US election officials). Moreover, the financial constraints for election officials in the US and elsewhere make lucrative lotteries very unlikely to be widespread practice to encourage voter registration. Sweeney et al. (2018) test a variety of mailings to encourage response to the UK’s mandatory annual registration canvass. Simple changes to the treatment mailers resulted in both negative and positive effects ranging from -1.1 percentage points below the control group to 3.4 percentage points above the control group. The treatment that had the greatest positive effect at increasing response rates included removing all references to voting (i.e. focusing on non-voting reasons to reply to the UK canvass), an option that is not suitable in the US context when voter registration is a stand-alone process.

This lack of clarity from past research about the effect of mailings on voter registration, especially the limited number of experiments on mailings by election officials (none in the US), motivates the experiments in this paper.

### 3 Partnership with Election Officials

Nearly all field experiments on voting behavior are conducted either in partnership with civic or political organizations attempting to nudge citizens towards the desired political behavior or by researchers imitating communication from such organizations (Green and Gerber 2015). In prior voter registration field experiments, civic or political organizations delivered the encouragement to register. We are aware of only a handful of published field experiments on voting behavior conducted in partnership with US election officials (Gerber et al. 2013; Hess, Hanmer, and Nickerson 2016; Mann and Sondheimer 2013; Stein, et al. 2012), and none of these examined whether election administrators could impact voter registration among eligible but unregistered citizens.

We conducted two large-scale field experiments with two states, working with the respective state election officials: The Office of the State Election Commissioner in Delaware (2012) and the Office of the Secretary of State in Oregon (2014). The experiments were facilitated by a small administrative reform: In 2012, a consortium of state election officials created the Electronic Registration Information Center [ERIC] to provide a variety of data processing services to improve administration of voter registration.<sup>1</sup> One service is identifying likely *eligible but unregistered* citizens (EBUs) using a sophisticated matching procedure to compare voter registration rolls with the roster of driver's licenses and state identification cards for each ERIC member state. EBUs are either newly eligible because they reached voting age, moved from another state, never registered, or were removed because they failed to participate in at least two

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<sup>1</sup> Two other papers cover ERIC experiments similar to our research, but they are yet to be published. Insofar as the other experiments include similar treatments and findings, the results are consistent with the results we present here. For more information about those studies, see < Citations redacted >.

consecutive federal elections. Registered voters who move within the state and therefore need to re-register are also identified by ERIC and contacted by the states, but are not EBUs and thus not included in these experiments.

States who are members of ERIC are required to send mailings encouraging registration to EBUs, and our experiments are embedded within these mailings. Thus, in addition to scholars' interest in understanding electoral participation, the results of these experiments help inform election officials' implementation of this requirement.

A central mechanism in all the treatments in our experiments is information about how to register to vote. Unlike most voter mobilization treatments that seek to increase intrinsic or extrinsic motivation to participate, our treatments seek to lower the costs of political participation – especially for citizens unfamiliar with the voting process. In previous research utilizing outreach from election officials, Gerber et al. (2013) found that information about ballot secrecy from a state election official increased turnout among registered voters who had not previously turned out to vote. Stein et al. (2012) found that information from local election officials increased turnout when implementing new Election Day Vote Centers. Informational mailings from civic groups about new or unfamiliar voting procedures have also found increases in turnout (e.g. Citrin, Green and Levy 2014; Gerber et al. 2014; Mann and Mayhew 2014). This prior research suggests that mailings from election officials should be effective at influencing registration behavior.

In addition to providing information about the registration process, all treatments in these two experiments mention monitoring of pro-social behavior. The text explaining why the mailings are being sent by the state election agency indicates that the voting behavior of the recipient is being monitored: “[o]ur records indicate you may be eligible to vote, but do not appear to be registered to vote,” (from Delaware, see Supplemental Online Materials [SOM] Figure DE-1).

This monitoring does not add the heavy-handed explicit threat of social sanction in Gerber, Green and Larimer's (2008) voter mobilization treatment, but subsequent research on mobilizing registered voters has shown that registered voters are more likely to cast ballots even when monitoring of compliance with social norms about electoral participation is disguised (Mann 2010; Panagopoulos 2011). Since this implication of monitoring is embedded in a legally necessary description of ERIC's EBU identification process by the election agency on every treatment, we cannot evaluate its marginal contribution to the efficacy of the treatments.

A secondary aspect is examining the effectiveness of variations of the postcard treatment. Although wide array of mechanisms might be expected to increase registration and turnout, these experiments are only able to explore a narrow range of possibilities because US election officials are legally prohibited from utilizing many promising mechanisms (e.g. political partisanship, salient controversies and issues, candidate appeals, explicit many social pressure, financial incentives, descriptive norms referencing social identities). Some scholars may find these limitations on the range of theoretically interesting treatments frustrating, but the treatments in these experiments reflect the range of mechanisms available to US election officials. We hope that future research will explore a wider diversity of psychological mechanisms available to non-governmental civic and political organizations.

Both theory and past research suggest the mechanisms in the treatment variations could provide an additional nudge for eligible but unregistered citizens to register to vote, so we assess whether each increases the effectiveness of the postcards from election officials.

The first variation is conveying a sense of urgency to register to enable participation in the upcoming election. While anyone with cursory familiarity with consumer advertising recognizes this concept is not new (Buy now before the deal is gone!), applying and especially testing urgency

as a mechanism to increase voter registration is new. Many states require registration well in advance of Election Day (up to 30 days prior to the election), but many eligible but unregistered citizens do not appear to understand or be aware of the necessity to register well before Election Day (Street et al. 2015). To address this situation, the experiments test a message of urgency to register to vote, stressing the importance of remembering to register and that missing the deadline will prevent the ability to participate. This urgency message is aimed at increasing the likelihood of acting promptly on a latent propensity towards electoral participation (Arceneaux and Nickerson 2009), rather than seeking to change intrinsic motivation to participate in the political process. Conveying urgency should increase voter registration rates in the short-term, i.e. before an upcoming registration deadline. However, an increase relative to other treatments without the urgency messages may dissipate over time.

The second variation seeks to provide visual cues about completing the registration process. Simple visual cues can serve as a task reminder and help overcome the obstacle of perceived (or real) difficulty. A large body of research supports use of visual cues to encourage pro-social behavior in many facets of day-to-day life, e.g. handwashing in public restrooms, separating recycling and trash, workplace safety procedures. Any communication about socially desirable behavior will have a reminder effect, but these types of visual cues may have a larger effect by reducing the perceived (or real) difficulty of completing the process.

The third variation seeks to increase an individual's intrinsic motivation to vote by priming social norms about civic duty to participate in elections (Blais and Achen 2018; Carreras 2018; Goodman 2018). Voting has significant social rewards (Gerber et al. 2016). Research on mobilizing registered voters to cast ballots has found that leveraging social norms about voting is a powerful tactic (see Green and Gerber 2015 for an overview). While state election agencies are

not willing to use the heavy-handed social pressure that has proven effective in voter mobilization (e.g. Gerber, Green and Larimer 2008), voter registration efforts often remind eligible but unregistered citizens about civic duty. Since US election officials are not permitted to reference many politically powerful social identities (e.g. party, race, gender), we turn to (likely) weaker treatments attempting to leverage national or state civic pride in appeals to civic duty.

## 4 Hypotheses

We derived testable hypotheses from the theory and past research described above. For clarity, we state these hypotheses explicitly before describing the research design of the experiments used to test these hypotheses. The hypotheses are ordered from the primary questions to secondary propositions.

The basic premise of the mailings by state election administrators is that communication from election administrators to eligible but unregistered citizens increases voter registration.

*Hypothesis 1: Treatment will increase the voter registration rate compared to the control group that receives no mailings.*

We further expect that overcoming the registration barrier (H1) will lead to higher rates of voting in the subsequent election. Increases in turnout must be equal to or smaller than increases in registration because advance registration is a legal pre-requisite to casting a ballot in the states where our experiments are conducted. If there is no increase in registration, there can be no increase in voting. Citizens who register are not contacted again to encourage turnout, except normal correspondence with all registered voters (polling place notifications, sample ballots, etc.), so increases in turnout indicate the degree to which overcoming voter registration barriers is sufficient to increase voting.

*Hypothesis 2: Treatment will increase the voter turnout rate in the subsequent election compared to the control group.*

A secondary question is whether different versions of the mailings produce different treatment effects. The urgency treatments highlight the timing of registration (i.e. prior to the deadline to participate in the upcoming election), so we test whether this shifts the timing registration but may dissipate over time as the other randomly assigned conditions “catch-up” to the urgency treatment condition.

*Hypothesis 3: Urgency will prompt more registration in the short-term, but the treatment effect will dissipate over time.*

Since the range of mechanisms testable by election officials is narrow and past research involving other behaviors suggests each mechanism will have small incremental effects, we did not have strong prior expectations about relative magnitude. Thus, beyond Hypothesis 3, we have only a general expectation of difference across the treatments.

*Hypothesis 4: The treatments will generate different magnitudes of increased voter registration and turnout from one another.*

## **5 Research Design**

Our research design utilizes field experiments with random assignment to one of several treatments or an untreated control group. The two experiments were conducted with communication from state election agencies to eligible but unregistered citizens. Field experiments are valuable for studying political behavior because of the pernicious problem of unobserved heterogeneity in observational data and self-reporting biases in statements of intended behavior in survey and laboratory experiments. Our design addresses concerns about generalizability of the findings in two ways: First, the experimental population for each experiment is the entire population of eligible but unregistered citizens. While ERIC’s method of identifying the eligible but unregistered citizens is not perfect, it is a more comprehensive process for identifying the unregistered population than previously available to scholars, election administrators, or other

organizations. Second, we replicate the results with independent experiments during a Presidential and a mid-term election in two states on opposite sides of the country with very different voting rules and political cultures. Of course, future research should further replicate the results in different places and different elections to further boost confidence.

Field experiments about voting raise important considerations about research ethics because of the potential to impact both individual human subjects and society. Partnering with state election officials is a key feature shaping the ethical profile of these field experiments. Importantly, the societal and individual benefit outweigh the negligible risks. Without research, state election officials still would have sent postcards to voters as required by membership in ERIC. Thus, the increase in electoral participation that could (and did) occur and any potential risk to individuals from receiving the postcard (e.g. social sanction for not being a good citizen) would have happened *without* conducting research. The only alteration for research purposes was random assignment to a treatment or the small untreated control group. Since ERIC states are required to contact eligible but unregistered citizens for each federal election, the risk of non-treatment of the control group in a particular election is mitigated by contact in a future election rather than permanent exclusion. The risk from differential impact of the treatment versions is mitigated by the expectation of only small marginal effects across mechanisms tested. The societal benefit of informing government officials about how to maximize a public good (electoral participation) counter-balances these negligible risks. Beyond the researchers' judgment and review by Institutional Review Boards, ethics were carefully considered by the state election officials.<sup>2</sup>

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<sup>2</sup> Experiments were conducted under the supervision of the Institutional Review Boards at <redacted>.



We describe the common elements of both experiments' designs before detailing the specifics of each experiment.

## **5.1 Postcard Treatments**

Postcards are commonly used in election administration to deliver a variety of notifications, and they are economical to print and mail. All of the treatments were 5 x 8 inch postcards. One side of the postcard was identical across all treatments in each state. This side of the postcard provided several pieces of information (SOM Figures DE-1 & OR-1) including: why individuals were receiving the postcard, a brief summary of legal requirements for registering to vote, instructions on how to register, and the deadline for registering to participate in the upcoming election. It also displayed the official seal of the state election agency and the U.S. Postal Service's "Official Election Mail" logo. These indicators of official correspondence likely contributed to the credibility of the postcard, and thus increase the likelihood of response (Edwards, Dillman & Smyth 2014).

Each experiment had four separate versions of the treatment postcard plus the untreated control group. The postcards were selected by the staff in the respective state elections office from a set of options provided a professional mail firm, then revised by state election agency staff. The researchers consulted on the selection and editing of the postcards but did not have any control over the final content.

The most notable difference between the two experiments is that Delaware directed people to download (or otherwise obtain) a paper voter registration application and then mail it to the state election agency, while Oregon directed voters to the state's online voter registration system.

## **5.2 Data on eligible but unregistered citizens**

The largest impediment to conducting voter registration experiments is identifying citizens who are not registered to vote. Comparing the number of registered voters to Census data on

population reveals that there are many eligible but unregistered citizens, although exact estimates get bogged down in debates about quality of voter rolls and Census methodology. The Census Bureau asks respondents if they are registered, but this individual level data is not available to scholars, election administrators or anyone else. Other surveys are too small to provide a sizable number of unregistered individuals. Moreover, survey self-reporting about registration status does not match actual registration data (Ansolabehere and Hersh 2012).

Prior field experiments on voter registration have taken different approaches: Some experiments have used students because they are unlikely to be registered due to recent eligibility (turning 18 years old) and/or high residential mobility (thus requiring re-registration) (Bennion and Nickerson 2011, 2014, 2016; Addonizio 2011). Other experiments have randomly assigned geographic areas (streets or apartment buildings) and canvassers identified unregistered individuals in these geographic units by knocking on each door (Braconnier, Dormagen, and Pons 2017; Nickerson 2015).

The process of identifying likely eligible but unregistered population in these experiments has important advantages over prior approaches. It includes nearly all likely eligible but unregistered voters, not just newly eligible voters or those in an unrepresentative geographic area. In order to identify the broadest possible list of likely EBUs, ERIC compares a state's voter registration rolls to all individuals with state driver's license or identification card. ERIC cleans the data with the US Postal Service National Change of Address database, U.S. Social Security Administration's Death Master File, and other governmental data sources. Then ERIC produces a list of EBUs using a sophisticated data matching process (described at [www.ericstates.org](http://www.ericstates.org)). The main shortcoming in this approach is omission of eligible but unregistered people without a driver's license or ID card.

We believe the ERIC process is the best available to produce broadly generalizable estimates of the effects of treatments encouraging registration. We have no role in ERIC's process, only receiving the end product from the states in order to randomly assign the experimental conditions. Moreover, the random assignment ensures that any unobservable errors from the likely eligible but unregistered identification process are evenly distributed across the groups. Therefore, any errors in identifying eligible but unregistered voters will not bias results.

### **5.3 Random Assignment Procedure**

We conducted random assignment of the entire population of likely EBUs for each experiment after receiving the lists from the respective state election agencies. In order to prevent violations of the Stable Unit Treatment Value Assumption (i.e. cross-contamination of the experimental conditions) (Sinclair, McConnell & Green 2012), random assignment was conducted by unique address so that every record at a unique mailing address received the same assignment. We report the results of bivariate regression estimation that are equivalent to difference of means. The inclusion of individual level covariates in the regression models (reported in the SOM) does not alter the results, as expected.

The random assignments were conducted using the “re-randomize” procedure developed by Kennedy and Mann (2015) to ensure balance on observable covariates prior to the administration of treatments. Since the re-randomization procedure truncates the distribution of possible random assignment, hypothesis testing requires randomization inference to estimate  $p$ -values (Gerber and Green 2012). Using randomization inference tests the sharp null of no effect for any individual in the experimental population. The randomization inference procedure conducted 1,000 iterations of the randomization procedure used for the original random assignment to construct the distribution of potential outcomes. Using this distribution of potential outcomes, all  $p$ -values report two-tailed calculations of probability of the observed outcome under

the assumption of zero treatment effect. This procedure accounts for the clustering by unique address in calculating the randomization inference  $p$ -values.

Since we make compare four treatments to the control group in each experiment, correction for multiple comparisons is appropriate. Bonferroni corrections of critical values for statistical significance are reported in the SOM (e.g. the usual  $p < 0.05$  threshold is adjusted to  $p < 0.0125$ ). All of the treatment effects reported as statistically significant reject the null under this conservative approach to avoiding Type I errors.

#### **5.4 Measurement of Outcomes**

In each experiment, we measure voter registration effects at two different times plus voter turnout in the general election following the state's mailing. The first measure of voter registration is any voter registration transaction with a date on the state voter registration rolls between the state's mailing and the deadline to register in the upcoming election. The second measure of registration is defined as any voter registration transaction between the state's mailing and the final post-election data of the voter registration rolls we received from the state election agency.

While this is not a voter mobilization experiment, we are interested in whether encouragement to *register* will result in any increase *voting*. The voter turnout measure is whether an individual cast a ballot in the general election immediately following the mailing according to the state's administrative records.<sup>3</sup> While registration is legally required to vote and there is no way for voters to get around the requirement, using turnout among those who registered would lead to biased comparisons when the treatments lead to different rates of registration among the eligible but unregistered citizens, as occurs below. Therefore, we calculate voter turnout effects

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<sup>3</sup> We are unable to assess effects on registration and turnout in later elections. ERIC and the states have extremely strict data confidentiality policies so we no longer have access to identifiable records necessary to conduct these downstream analyses.

using the full experimental population to have unbiased estimates about how turnout was impacted by the voter registration treatment.

Identifying likely eligible but unregistered individuals is only the first half of the data matching for these experiments. The second half is matching the list of EBU individuals in the experiment to the voter registration rolls after the election to be able to measure registration rates and turnout. Since all types of errors in the post-election process should be evenly distributed across the randomly assigned conditions, there is again no risk of biasing the estimates. We note that we cannot estimate the uncertainty introduced by the matching process. It is impossible to estimate this uncertainty because we have no benchmark of the ‘true’ match, so we simply note that the reported hypothesis tests understate the full uncertainty around our treatment effects. However, even large adjustments for this unknown uncertainty would not alter the substantive inferences due to the large size of these field experiments.

## **5.5 Experiment 1: Delaware 2012**

Experiment 1 was conducted in partnership with Delaware’s Office of the State Election Commissioner prior to the 2012 General Election. Delaware was not a competitive state for the Presidency, but there were competitive races for congressional and state offices. Recipients of the treatment needed to download and print a paper registration application from the website on the treatment postcards or obtain the paper application elsewhere, then mail the application to the election office. This process is a non-trivial burden that civic and political voter registration efforts often seek to reduce by providing the voter registration application and assisting with return of the application during in-person visits or by mailing the application and a pre-addressed postage paid envelope to likely unregistered citizens. In addition, individuals could (and did) also register by other methods.

The four treatment versions in Delaware differed on the postcard's front side. The *Deadline-Urgency* treatment had the phrase "Important Voter Registration Information" across the center of the postcard, plus "Deadline Approaching" highlighted in red. This plain graphic presentation was intended to highlight the urgency of the deadline (see SOM Figure DE-2). The *Visual Cue-Urgency* treatment used an image of a voter registration application to prompt voters to think about registering. Further, it sought to convey urgency with an image of a Post-It note with "Important! Don't forget to register to vote." (see SOM Figure DE-3). The last two treatments were both intended to evoke social norms of civic duty to vote, but used two different civic identity cues. Both treatments used the same text, "Our democracy only works if you vote." The *National Civic Duty* treatment uses the American flag to evoke national civic pride plus the famous first three words of the U.S. Constitution "We the People". The *State Civic Duty* treatment uses the Delaware flag to evoke state civic pride (SOM Figures DE-4 & 5).

ERIC produced a list for Delaware with 30,247 likely eligible but unregistered individuals (28,687 households). The Office of the State Election Commissioner agreed to random assignment allocating 20% of the unique addresses to each of the 5 experimental conditions. SOM Table 1 provides descriptive statistics for each condition using the limited covariates available from ERIC (number of targets at mailing address, mailing zip code, and first initial of last name). The random assignment was balanced across these covariates, as expected.<sup>4</sup>

The data team in the Election Commissioner's office did the post-election matching to the voter registration rolls to obtain the outcome measures. The matching used name, address, and date of birth available to them on both datasets.

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<sup>4</sup> The experiment provided two weeks to complete the voter registration process. The postcards were sent on September 27, 2012 using first class mail. The deadline to register to be allowed to vote in the upcoming 2012 election was October 13, 2012.

## 5.6 Experiment 2: Oregon 2014

Experiment 2 was conducted in partnership with the Oregon Secretary of State's Office prior to the 2014 General Election. The 2014 mid-term election in Oregon featured a contested US Senate race, a contested gubernatorial race, a battle for majority control of the state Senate, and a marijuana legalization measure. Oregon allows any eligible citizen with a driver's license or state ID to register to vote online. Since the ERIC list is derived from people with a license or state ID, treatments directed recipients to the state's online registration portal. This online process is generally seen as less burdensome than paper applications or appearing in person (Atkeson 2014). Again, recipients could (and did) register via other methods. Oregon uses a postal voting system so all registered voters receive a ballot by mail (Mann 2014).

All four Oregon postcards have two features expected to increase registration (and thence turnout) compared to the untreated control group. Each postcard has a fake Post-It to convey urgency saying "Important! Don't forget to register to vote." The urgency message on all four treatments means we cannot make any inferences in Experiment 2 about the effect of urgency. The bolded phrase, "3 Minutes. Click. Done." appears on the back of each postcard to emphasize the convenience of using Oregon's online voter registration system. Again, since this convenience message is present on all four postcards, we make no inferences in Experiment 2 about the effect of convenience.

The four treatment versions in Oregon differed on the postcard's front side. The first treatment in Oregon is a *Placebo* treatment that is intended to elicit only the mechanisms shared by all of the mailings to isolate the marginal effect of other mechanisms. The Placebo treatment used a large version of the US Postal Service "Official Election Mail" logo on the front of the postcard (SOM Figure OR-2).

The remaining three treatments replicate and extend the mechanisms explored in Experiment 1. The *Visual Cue-Urgency* treatment in Experiment 2 was similar to this treatment in Experiment 1, although with a different image. In Experiment 2, the *Visual Cue-Urgency* treatment shows a clearly labeled Oregon Voter Registration Card (SOM Figure OR-3). The last two treatments in Experiment 2 are intended to distinguish the effect of civic duty from association with civic identity. The *Civic Duty* treatment used a simple clip-art image of the word “Vote” with the letter V transformed into a checkmark (SOM Figure OR-4). The image is typical of blandishments to vote from civic organizations and government agencies. This treatment is a weak and innocuous reminder to check-off one’s civic duty by voting. The *State Civic Duty* treatment uses an image of the iconic Oregon license plate with evergreen tree over a mountain silhouette (SOM Figure OR-4).

ERIC produced a list for Oregon with 769,686 likely eligible but unregistered individuals (549,748 households). Due to ERIC bylaws in 2014, the Oregon Secretary of State’s Office allowed the random assignment 4% of the unique addresses to the control group and 24% to each of the 4 treatment conditions.<sup>5</sup> SOM Table 2 provides descriptive statistics for each condition using covariates available from ERIC in 2014 (age, year of last DMV transaction, number of targets at mailing address, mailing zip code, and first initial of last name). The random assignment was balanced across these covariates, as expected.<sup>6</sup>

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<sup>5</sup> ERIC bylaws in 2014 required states mail voter registration information to at least 95% of EBUs. The Oregon Secretary of State permitted only a 4% control group to be inside this limit.

<sup>6</sup> Due to the large number of postcards, the Secretary of State’s printing vendor delivered the postcards to the US Postal Service in batches as they were printed during the week of September 19-24, 2014. Ideally, the mailings would have all been sent on the same day, but this small variation in timing between treatments being sent out is very unlikely to create significant variation in the treatment effects at the registration deadline on October 14.



In Experiment 2, we matched the ERIC list to updated voter registration rolls with individual turnout records after the election. Our matching procedure was a modified version of Ansolabehere and Hersh's (2017) AGDN method (see SOM for details). The results reported in Section 6 do not change substantively using alternative matching criteria. As discussed in Section 5.5, any errors will be evenly distributed across the randomly assigned experimental conditions, and therefore will not bias the estimated effects.

## 6 Results

Experiment 1 demonstrates that sending postcards to the eligible but unregistered citizens identified by ERIC significantly increases voter registration and turnout. Figure 1 reports the registration rate for the randomly assigned experimental conditions prior to the deadline to be able to vote in the 2012 election. The results find clear support for Hypothesis 1's expectation that postcards would cause a statistically significant increase in registration. All of the postcards increased registration above the 6.8% registration rate in the control group. The registration rates between mailing date and the registration deadline were: Visual Cue-Urgency = 9.4% (+2.6 percentage points,  $p_{ate} < 0.001$ ); Deadline-Urgency = 9.2% (+2.4 percentage points,  $p_{ate} < 0.001$ ); National Civic Duty = 8.6% (+1.8 percentage points,  $p_{ate} < 0.001$ ); and State Civic Duty = 8.6% (+1.8 percentage points,  $p_{ate} = 0.002$ ). These treatment effects are indistinguishable from one another, and the average registration rate among all treatments is 9.0% (+2.2 percentage points,  $p_{ate} < 0.001$ ) (see SOM Table 3 for full results).

**Figure 1: Registration Rate before 2012 Election Deadline in Delaware (Exp. 1)**

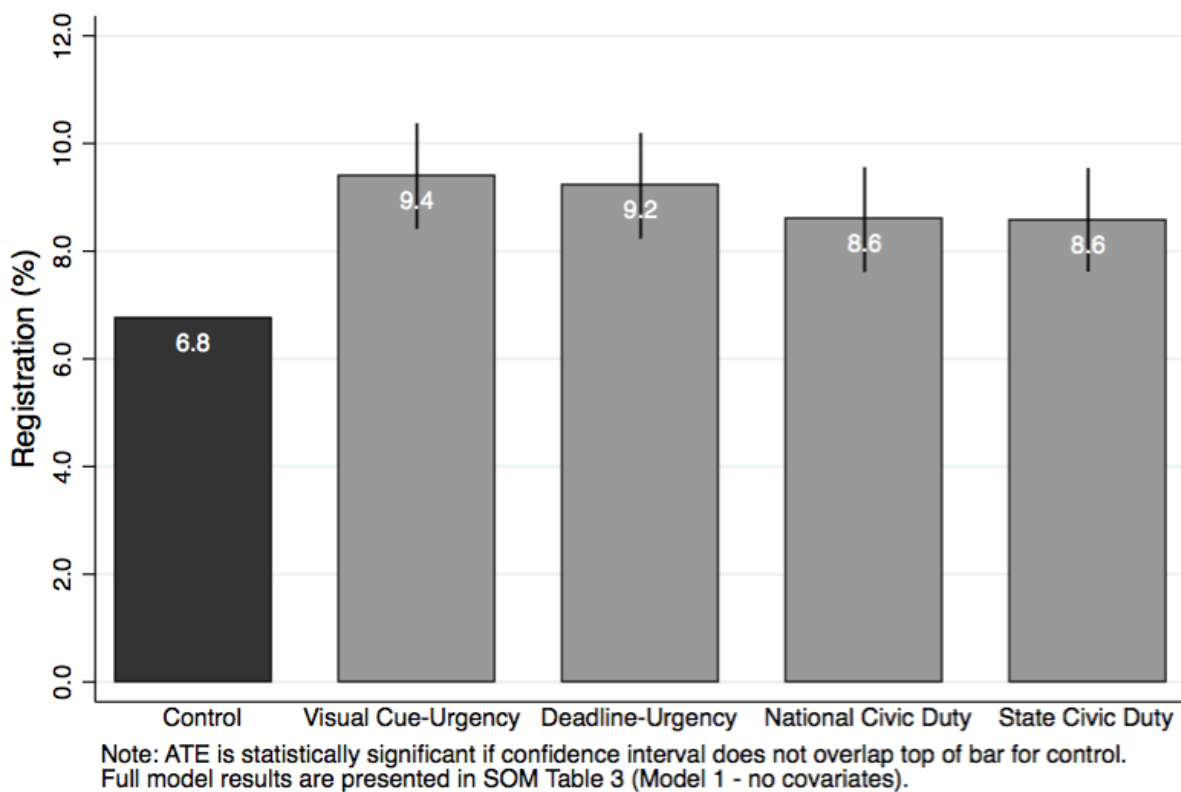
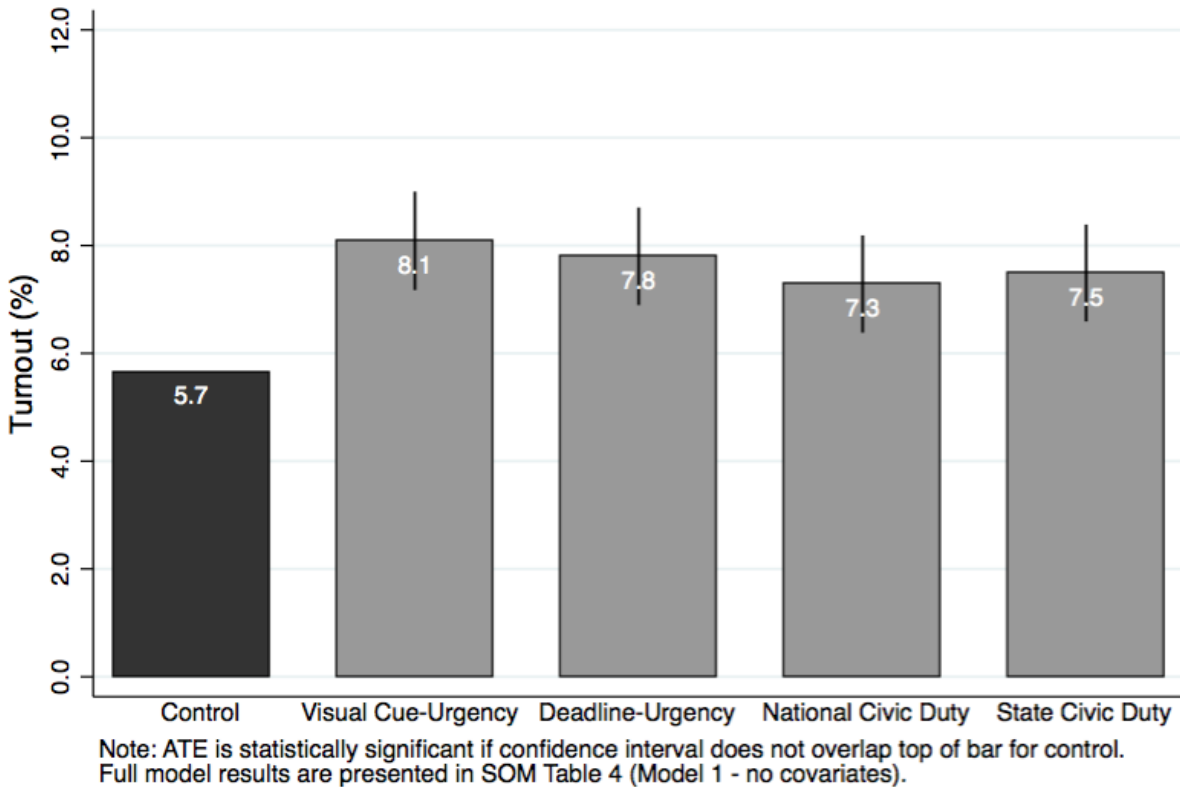


Figure 2 reports the turnout rate in the 2012 general election for all records assigned to each experimental condition. Experiment 1 also shows strong support for Hypothesis 2's expectation that the postcard treatments would increase turnout in the subsequent election. In Experiment 1, 91% of increase in registration is translated to an increase in turnout.<sup>7</sup> In the control group, turnout was 5.7% of all eligible but (previously) unregistered citizens assigned to this condition. The turnout among eligible but unregistered citizens assigned to the treatments were: Visual Cue-Urgency = 8.1% (+2.4 percentage points,  $p_{ate} < 0.001$ ); Deadline-Urgency = 7.8% (+2.1 percentage points,  $p_{ate} < 0.001$ ); State Civic Duty = 7.5% (+1.8 percentage points,  $p_{ate} = 0.001$ ); and

<sup>7</sup> In Experiment 1, an instrumental variables regression of the turnout effect using random-assignment-to-treatment as the instrument for voter registration estimates the turnout effect is 91.0% of the registration effect ( $p < 0.001$ ; s.e. 6.8 percentage points).

National Civic Duty = 7.3% (+1.6 percentage points,  $p_{ate}=0.001$ ). These treatment effects are indistinguishable from one another, and the average turnout among all treatments is 7.7% (+2.0 percentage points,  $p_{ate}<0.001$ ) (see SOM Table 4 for full results).

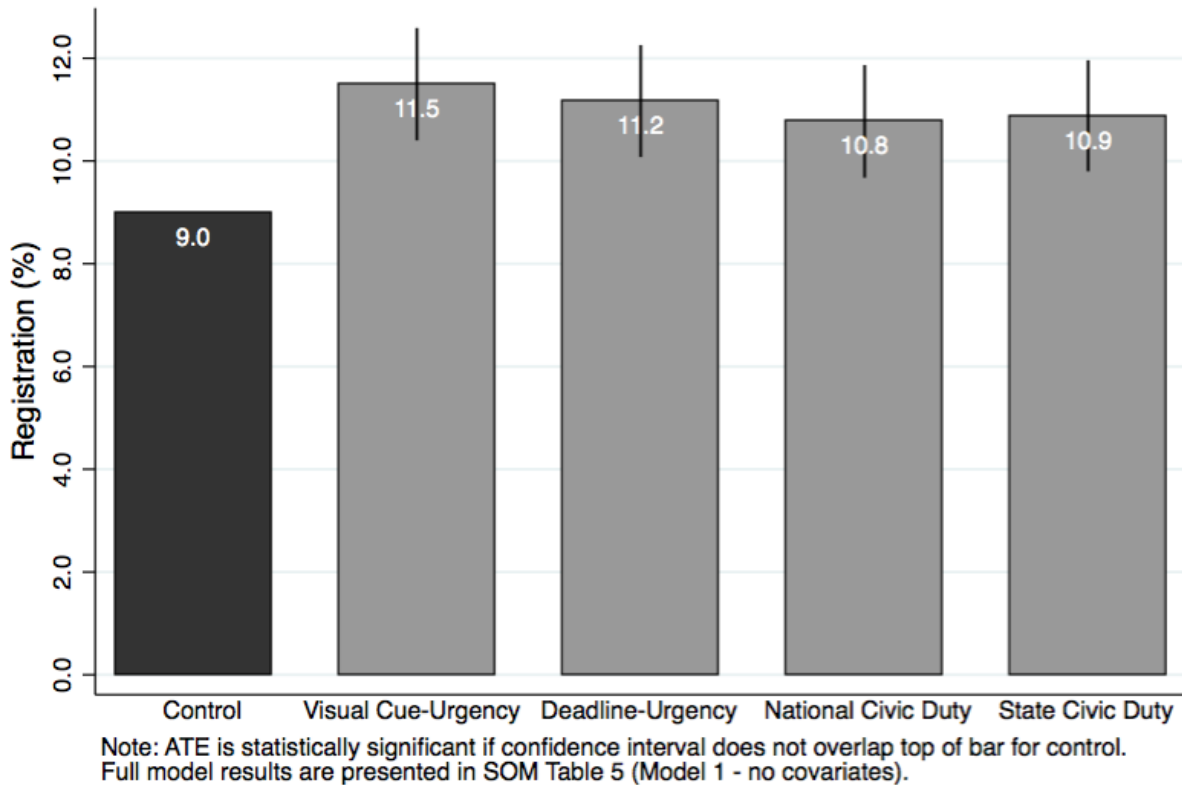
**Figure 2: Turnout in the 2012 General Election in Delaware (Exp. 1)**



Experiment 1 provides little evidence for Hypothesis 3’s expectation that stressing urgency will have a larger effect in the short-term but will be attenuated over a longer time. Figure 3 shows the registration effects as of December 26<sup>th</sup>, 2012, when the Office of the State Elections Commissioner provided final data for the analysis. Registration rates rose in all conditions but the treatment effects are essentially unchanged. The registration rate from the mailing to December 26, 2012 in the control group was 9.0%, and the turnout for the treatments were: Visual Cue-Urgency = 11.5% (+2.5 percentage points,  $p_{ate}<0.001$ ); Deadline-Urgency = 11.2% (+2.2 percentage points,  $p_{ate}<0.001$ ); State Civic Duty = 10.9% (+1.9 percentage points,  $p_{ate}=0.004$ ); and

National Civic Duty = 10.8% (+1.8 percentage points,  $p_{ate}=0.002$ ). These treatment effects are indistinguishable from one another, and the average registration rate among all treatments is 11.1% (+2.1 percentage points,  $p_{ate}<0.001$ ) (see SOM Table 5 for details).

**Figure 3: Registration Rate From Mailing Date to Dec. 26 2012 in Delaware (Exp. 1)**

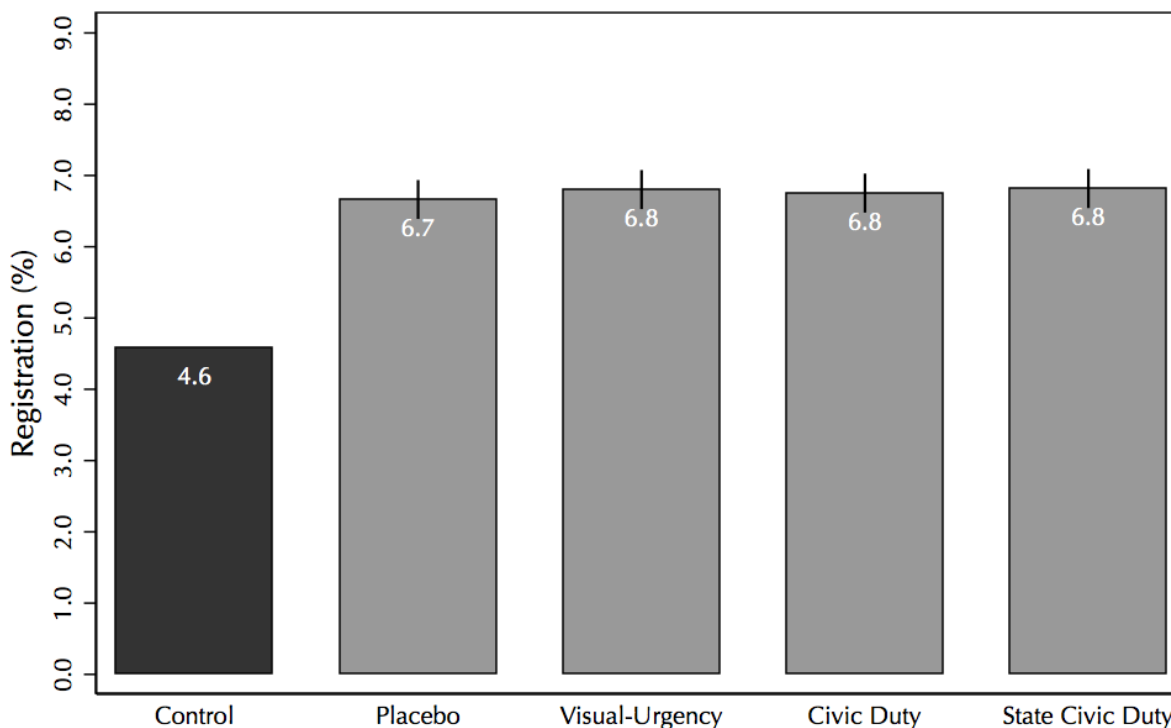


Experiment 1 provides no statistically significant support for Hypothesis 4’s expectation of differences between the treatments, since the four treatments are indistinguishable in Figures 1-3. However, the Visual Cue-Urgency treatment appears to have the largest effect, with the Deadline-Urgency treatment close behind, on voter registration, and a drop off to the two civic identity treatments. These results lend support to election officials’ prior beliefs about communicating urgency. For the applied decision made by election officials overseeing Experiment 2, these results were sufficient to indicate communicating urgency could be beneficial

and was very unlikely to be harmful so urgency is used in all treatments in Experiment 2 (and several other states' ERIC mailings).

Experiment 2 replicates the substantive findings from Experiment 1. Experiment 2 shows strong support for Hypothesis 1's expectation of increased registration. Figure 4 shows all four treatments significantly increase registration prior to the deadline to cast a ballot in the 2014 General Election. The registration rate in the control group was 4.6.%, and the registration rates for the treatments were: Placebo = 6.7% (+2.1 percentage points,  $p_{ate} < 0.001$ ); Visual Cue-Urgency = 6.8% (+2.2 percentage points,  $p_{ate} < 0.001$ ); Civic Duty = 6.8% (+2.2 percentage points,  $p_{ate} < 0.001$ ); and State Civic Duty = 6.8% (+2.2 percentage points,  $p_{ate} < 0.001$ ). These treatment effects are indistinguishable from one another, so again there is no support for Hypothesis 4's expectation of differences between the treatments. The average registration rate among all treatments is 6.8% (+2.2 percentage points,  $p_{ate} < 0.001$ ) (see SOM Table 6 for details).

**Figure 4:** Registration Rate before 2014 Election Deadline in Oregon (Exp. 2)



Note: ATE is statistically significant if confidence interval does not overlap top of bar for control. Full model results are presented in SOM Table 6 (Model 1 - no covariates).

Experiment 2 also shows strong support for Hypothesis 2's expectation of increased turnout. Figure 5 shows all four treatments significantly increase turnout in the 2014 General Election among eligible but unregistered citizens. In Experiment 2, 77% of the registration effect is translated into increased turnout.<sup>8</sup> The turnout in the control group was 2.0%. The turnout rates for the treatments were: Placebo = 3.7% (+1.7 percentage points,  $p_{ate} < 0.001$ ); Visual Cue-Urgency = 3.6% (+1.6 percentage points,  $p_{ate} < 0.001$ ); Civic Duty = 3.6% (+1.6 percentage points,  $p_{ate} < 0.001$ ); and State Civic Duty = 3.7% (+1.7 percentage points,  $p_{ate} < 0.001$ ). These treatment effects are indistinguishable from one another, and the average turnout rate among all treatments is 3.7% (+1.7 percentage points,  $p_{ate} < 0.001$ ) (see SOM Table 7 for details).

<sup>8</sup> In Experiment 2, an instrumental variables regression of the turnout effect using random-assignment-to-treatment as the instrument for voter registration estimates the turnout effect is 76.7% of the registration effect ( $p < 0.001$ ; s.e. 3.5 percentage points).

**Figure 5:** Turnout in the 2014 General Election in Oregon (Exp. 2)

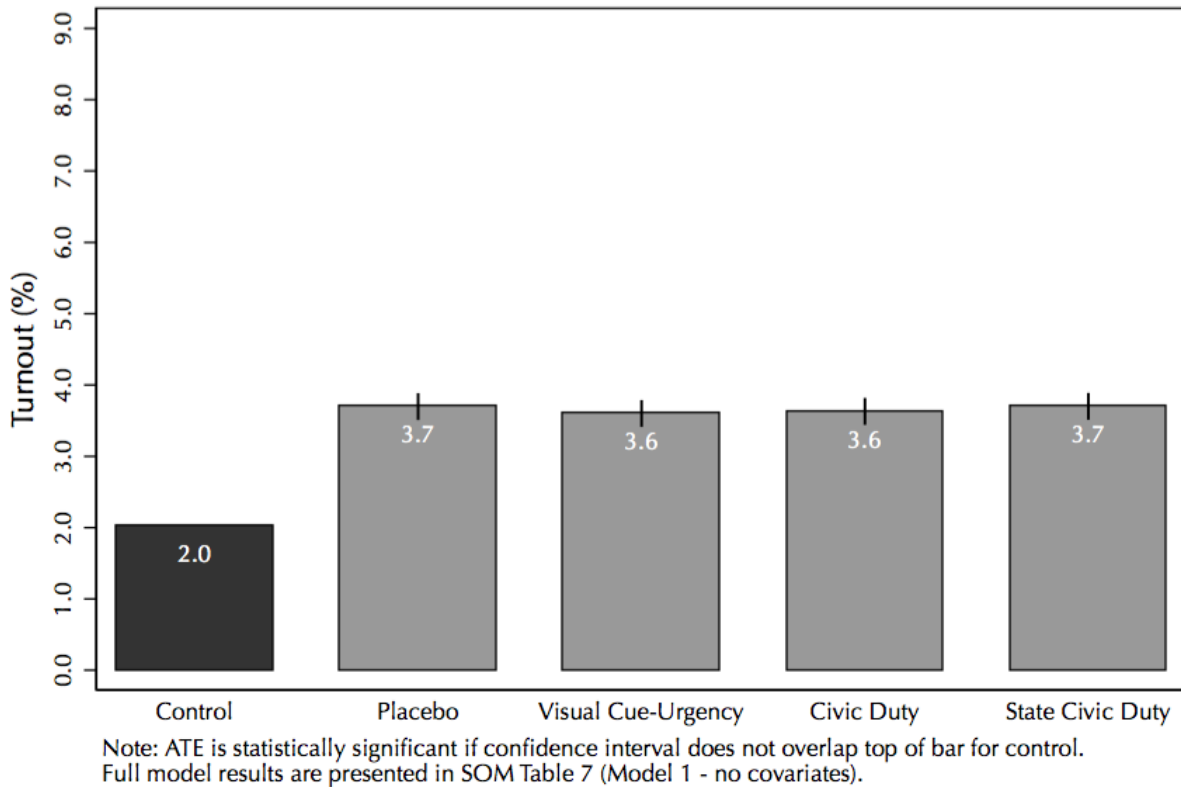
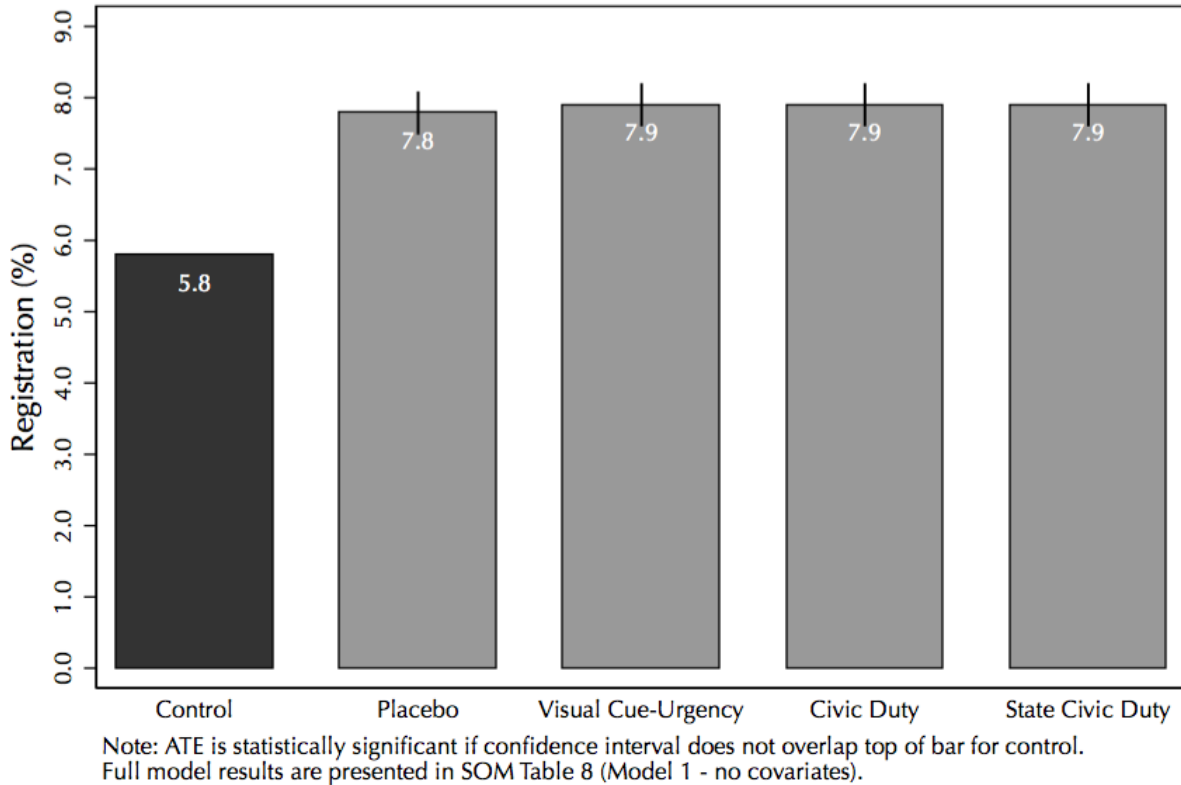


Figure 6 shows the increase in registration for all four treatments persists over the long term – and a much longer period than in Experiment 1. The treatment effects on registration in May 2015 remain essentially unchanged from Figure 4, although all conditions see increased registration over this seven-month period. Thus, there is no support for Hypothesis 3’s expectation about attenuation of the registration effect. The registration rate in the control group was 5.8%, and the registration rates for the treatments were: Placebo = 7.8% (+2.0 percentage points,  $p_{ate} < 0.001$ ); Visual Cue-Urgency = 8.0% (+2.2 percentage points,  $p_{ate} < 0.001$ ); Civic Duty = 7.9% (+2.1 percentage points,  $p_{ate} < 0.001$ ); and State Civic Duty = 7.9% (+2.1 percentage points,  $p_{ate} < 0.001$ ). These treatment effects are indistinguishable from one another, and the average registration rate among all treatments is 7.9% (+2.1 percentage points,  $p_{ate} < 0.001$ ) (see SOM Table 8 for details).

**Figure 6:** Registration Rate between Mailing Date and May 2015 in Oregon (Exp. 2)



## 7 Discussion

These experiments provide robust causal evidence that simple, low-cost postcards from election officials to encourage registration increase voter registration (Hypothesis 1) and turnout (Hypothesis 2) across the broadest available population of eligible but unregistered US citizens are effective. People concerned about civic engagement have long assumed that such encouragement increases voter registration and turnout in subsequent elections, but the mixed results of the few prior field experiments to measure the effect of registration efforts by civic and political organizations raised questions about this assumption. The positive impact on electoral participation provides important information for election officials and policy makers about the benefits of this policy choice.



Unfortunately, the experiments provide limited insight about mechanisms in the mailings causing additional marginal increases in registration and turnout. Neither experiment produces significant differences between the treatments (Hypothesis 4). The treatments were a limited range of possible mechanisms from social science but realistically reflect the range of options legally and ethically possible for election officials. Applying Ockham's razor, the most parsimonious mechanism is that all of the treatment mailings were a reminder to register. Information about the registration process also seems likely to contribute to the overall effect. The signal about monitoring of pro-social behavior is also a likely contributor since it has increased participation of registered voters. The marginal contribution made by appeals to civic duty, visual cues, or urgency is quite small and/or roughly equal. The absence of differences between the treatments is bad news for scholars seeking to understand nuances of registration and voting behavior. On the other hand, election officials will see good news in straightforward guidance about best practices for future mailings to increase registration.

The experiments also demonstrate the degree to which voter registration is a barrier to voting. Since the state election agencies did not have any contact with the experimental population beyond the postcard encouraging registration, the downstream effect on turnout comes from removal of the registration barrier. In the 2012 Presidential Election, the increase in registration in Delaware translated almost entirely into additional turnout. In the 2014 midterm election, about three-quarters of the increased registration translated to increased turnout. Further research is needed to determine whether these different ratios of converting increased registration to increased turnout are a systematic feature of electoral salience or some other difference in the experimental context (e.g. state political culture, voting method, etc.).

We have not reported on whether these ERIC EBU mailings generated complaints or problems because we lack systematic evidence. However, to the best of our knowledge, our partner state election agencies received few complaints. The absence of complaints bolsters research confidence about internal validity and suggests ERIC produces quality data for election officials.

The results from these two experiments are robust, particularly because of the replication of the treatment effect in states on opposite sides of the country, in different electoral contexts, using different voting systems. Nonetheless, replication in additional settings would be helpful to validate the generalizability of these findings.

The good news for democracy from these experiments is that inexpensive and simple communication from election administrators can increase registration and subsequent voting. The bad news is that despite a significant increase in registration and turnout, these treatments make only a partial reduction in non-participation by likely eligible but unregistered citizens. Future research is needed to explore whether repeated outreach can further increase registration and turnout. Repeated communication may have more impact through simple repetition or more serendipitous timing in contacting these eligible but unregistered citizens. Future research is also needed to explore other possible “nudges” from election officials (or civic and political organizations) to increase registration and voting.

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