Does email suppress turnout on election day? Experimental evidence from the 2016 general election*

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Abstract

Can inexpensive, impersonal methods of contact such as unsolicited email increase voter turnout? While email has potential advantages as a mobilization tool, existing experimental work suggests it is ineffective in most contexts. Here, we use public data to randomly expose a subset of Florida voters to email messages that encourage participation in the 2016 general election. No messages we tested increased turnout; instead we find that receiving email messages demobilized some voters. While the overall decrease in turnout amounted to less than 1 percent of the final margin, the demobilizing effect was particularly pronounced among minority voters. Black and Latino voters assigned to receive email messages were less likely to show up to the polls on election day than voters from the same group who received no message. These findings encourage both campaigns and researchers to think critically about the use and study of massive impersonal mobilization methods.

*This research was approved by the institutional review board of the `redacted for review`.

1 Email Mobilization

Do the low-cost methods of electronic communication used by political campaigns (Roose, 2018) actually encourage potential voters to turn out? One such method, email, has been the subject of experimental inquiry with mixed results (see Table 1). As social media reshapes the way voters communicate electronically (Anderson and Perrin, 2017), email's effectiveness as a campaign technology may also be changing.

Here, we test the impact of email messages that prime social norms about voting (Gerber and Rogers, 2009). The messages we send emanate from a publicly identifiable political science professor who is a member of our research team. We contact voters using a real identity to forestall concerns about deceptive messaging. While this experiment was designed to test the differential impact of injunctive and descriptive norms on turnout (`citation to pre-analysis plan redacted for review`), we find no clear evidence that voters react differently to different treatment messages. Instead, we were very surprised to observe a small, but persistent decrease in turnout among voters assigned to receive a treatment message.

Table 1: Mixed Evidence for Email Mobilization

No Effect	**Negative Effect**	**Positive Effect**
Nickerson (2007)	Bennion and Nickerson (2011)	Malhotra, Michelson and Valenzuela (2012)
Thirteen field experiments conducted in partnership with politcal campaigns find emails have essentially zero effect on voter registration or turnout.	University administrators' email encouragement of students to register to vote slightly decreases registration rates.	Email messages sent from the registrar of voters can have small, but significant positive effects on turnout.

When email has been effective the senders were a trusted, offical source (Malhotra, Michelson and Valenzuela, 2012). Outside of email mobilization there is evidence that some efforts meant to increase turnout can inadvertently have the opposite effect (e.g. McCabe and Michelson, 2015; Kousser and Mullin, 2007; Grose and Russell, 2008; Cornwall and Kessler, 2012).

Table 2: Comparison of Email Providers

	Registered Voters	*Provide Email*	*Assigned A Condition*	*Analytic Sample*
Proportion Democrat	0.51	0.51	0.52	0.51
Age in 2016	51.52	45.73	45.50	45.26
Proportion Female	0.53	0.52	0.52	0.52
Proportion non-White	0.29	0.31	0.32	0.30
Observations	12,339,702	629,738	503,859	328,181

Notes: SEs are omitted here but reported in the SI. *Proportion Democrat* is the proportion of registered Democrats among those registered as a Democrat or Republican. Column 2, *Provide Email* includes registered voters with email addresses that could not be reconciled to be valid, *Assigned A Condition*, includes only voters with a valid email. The *Analytic Sample*, used throughout the reported analysis, excludes voters who voted early in-person.

2 Experimental setting, approach, and data

We target voters who provide a valid email in the publicly available October 10, 2016 Florida Division of Elections voter roll. Random assignment to receive an email message was blocked on congressional district and self-reported race (See section 1 in the SI). Of the more than 12 million registered voters in Florida, 503,859 provide a valid email and are assigned to a condition.

Of all votes cast during the 2016 general election in Florida, 39.7% were cast early in-person, 29.4% were by-mail, and 30.5% were cast in-person on election day. Throughout the reported results, the analytic sample is the group of 328,181 Florida voters who provide a valid email address and did not vote early in-person. We include those who voted by mail, because some of these voters could have sent in their ballots after receiving a message. We exclude voters who cast ballots early in-person, because these votes were cast before the distribution of our treatment messages. The analytic sample is notably younger than the whole set of registered voters, but otherwise broadly similar in terms of party affiliation, gender identification, and ethnic/racial identification (Table 2, columns 1 & 4).

We contact voters via a single email sent from an active email address with a .edu top-level domain. All emails contain the subject heading "Please Remember to Vote Tomorrow", begin with a greeting, and close with a brief paragraph that directs questions about the voting process to the Florida Department of State.[1] In line with current campaign practices, we sent messages the morning of November 7, 2016 at 10am EST, one day before the election. Limitations on the sending infrastructure precluded the ability to measure whether a voter received or opened our emails (Hughes et al., 2019). All results are intent-to-treat effects.

3 Results

Outcome data are drawn from the June 14, 2017 official voter roll. Voters assigned to receive no contact – i.e. the control group – turned out to vote at a rate of 78.1 percent (see Table A3 in the SI).[2] Our main experimental finding, shown in Figure 1, is that those who were randomly assigned to receive any email message turned out at a 0.53 percentage points lower rate ($\bar{\tau} = -0.053$, robust $SE = 0.017$).[3]

We further examine the effect of email messages within groups of self-identified black, Latino, and white voters.[4] Among black voters, 64.0 percent assigned to the control group voted, compared to 61.8 percent in the treatment group ($\bar{\tau}_b = -2.168$, robust $SE = 0.585$). Among Latino voters, 73.9 percent assigned to the control group voted, compared to 73.0 percent in the treatment group ($\bar{\tau}_l = -0.970$, robust $SE = 0.414$). Finally, among white

[1] The SI contains additional details about message language, sending infrastructure, randomization, and data capture.

[2] At a target power of 0.8, this sample and design can detect a difference in the rates of turnout as small as 0.3 percentage points – roughly half the size of the established effect size of direct-mail (see SI section 3).

[3] All estimates use robust standard errors. Details of the estimation and accompanying regression results are provided in SI , section 6. We find no evidence of differential effects across message variants (see SI Table 5 for details). Throughout the main text, we compare the turnout of voters assigned to control to those assigned any message, and denote this treatment as $\bar{\tau}$.

[4] The analytic sample includes 204,054 white, 63,404 Latino and 36,518 black voters. See SI section 4.3.1 for details about estimation. See Table 7 for a test for heterogeneous response to treatment between racial/ethnic groups.

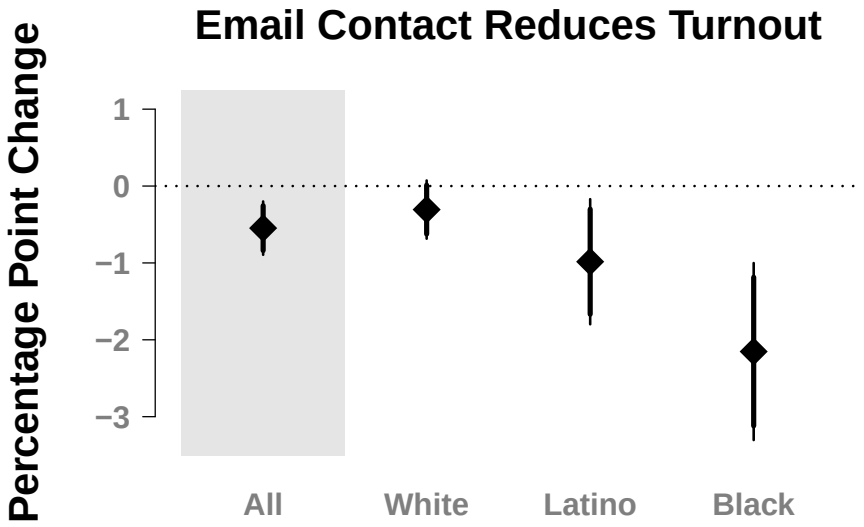

Figure 1: Reduction in rate of turnout among registered voters assigned to receive an email, compared to voters of the same race/ethnicity in the control group. All points are estimated in separate models, on data partitions noted on the x-axis. Thick lines are 1.64 times the robust standard error; thin lines are 1.96 times the robust standard error. See Table 4 and Table 6 in the SI.

voters, 84.0 percent assigned to the control group voted, compared to 83.7 percent in the treatment group ($\bar{\tau}_w = 0.309$, robust $SE = 0.193$). Plainly stated, these messages may have kept as many as 1,389 voters from going to the polls (95% robust CI $= [-2249, -531]$).

4 Discussion

Email mobilization has attracted interest in part because of its potential to deliver results at very low cost. Our results show that this low-cost investment for campaigns may yield zero or even negative returns for some groups of voters. Like all field experiments, ours has idiosyncratic context and design features that require care in extrapolating to both existing findings and future contexts (Bates and Glennerster, 2017). Specifically, our treatment messages were sent in the competitive 2016 election by a university professor with no personal or institutional connection to voters in the study. Perhaps this

approach primed legacies of experimentation or exploitation by researchers, especially among marginalized groups.

Ultimately, our design cannot support direct claims about why turnout was reduced. However, our preferred conjecture is that receiving a treatment message induced stress in some subjects. Approximately 700 voters chose to respond to our message; many of these people used worried or anxious language. Voters' decisions to reply occur after exposure to treatment, and so we are circumspect about the causal inferences permitted by these responses (Coppock, 2019). It is possible, however, that when voters perceive the political environment to be hostile or unfavorable the circumstances of voting cause stress, and unsolicited communication may reinforce those negative impressions and lower voters' internal motivation to turnout (Hassell and Settle, 2017). This perspective is consonant with careful observational work that demonstrates that black voters who perceive a lower chance of electing their preferred candidate are less responsive to mobilization efforts (McGowen, 2010).

The findings reported here provide a chance to reflect on our disciplines' ethical stance toward field experiments. When field experiments produce normatively appealing results there is little discussion about the ethics of experimentation (c.f., Desposato, 2015). Results like those we present here raise the specter that haunts experimental research, namely the possibility of doing harm (e.g. Michelson and Nickerson, 2011, p. 235-236). Our experimental intervention did not have even a minute impact on the election results, but it did lead to the normatively undesirable result of demobilizing some voters. While we strongly concur that no voter should be disenfranchised, we view the results of our research through a lens of transparency, and commitment to a process of scientific discovery. In the conduct of this research: we socialized our ideas with colleagues; preregistered our design and analysis; herein report the results; and, have made the code, compute, and data available to the community. Throughout this process, we have endeavored to strive toward scientific understanding within the commonly agreed upon

ethical constraint to minimize harm. As a result, we have generated what we believe will be useful information for researchers. We hope that, in the balance, the larger scientific enterprise will tend to further enfranchise all voters.

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