Field Experimental Evidence that Messages Designed to Increase Perceived Electoral Closeness Increase Turnout**

Daniel R. Biggers  
University of California, Riverside, Associate Professor  
Department of Political Science  
900 University Avenue  
Riverside, CA 92521  
daniel.biggers@ucr.edu

David J. Hendry  
Hong Kong University of Science and Technology, Assistant Professor  
Division of Social Science  
Main Academic Building  
Clear Water Bay, Kowloon  
Hong Kong SAR  
hendry@ust.hk

Alan S. Gerber  
Yale University, Professor  
Department of Political Science  
Institution for Social and Policy Studies  
77 Prospect Street, PO Box 208209  
New Haven, CT 06520-8209  
alan.gerber@yale.edu

Gregory A. Huber*  
Yale University, Professor  
Department of Political Science  
Institution for Social and Policy Studies  
77 Prospect Street, PO Box 208209  
New Haven, CT 06520-8209  
gregory.huber@yale.edu

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** The authors declare that they have no conflict of interest.  
* Corresponding author
Abstract

The decision-theoretic Downsian model and other related accounts predict that increasing perceptions of election closeness will increase turnout. Does this prediction hold? Past observational and experimental tests raise generalizability and credible inference issues. Prior field experiments either (1) compare messages emphasizing election closeness to non-closeness messages, potentially conflating changes in closeness perceptions with framing effects of the voter encouragement message, or (2) deliver information about a particular race’s closeness, potentially altering beliefs about the features of that election apart from its closeness. We address the limitations of prior work in a large-scale field experiment conducted in seven states and find that a telephone message describing a class of contests as decided by fewer, as opposed to more, votes increases voter turnout. Furthermore, this effect exceeds that of a standard election reminder. The results imply expected electoral closeness affects turnout and that perceptions of closeness can be altered to increase participation.

**Keywords:** Voter turnout; Election closeness; Voter mobilization field experiment; Voter pivotality
The canonical Downsian decision-theoretic model of turnout predicts that individuals will vote when they perceive the expected instrumental benefits of voting are larger than the expected costs of voting. These perceptions of the election’s stakes are derived from beliefs about the different outcomes depending on which candidate wins, multiplied by the chances that one’s vote is pivotal. This model implies that all else equal, individuals will be more likely to vote in a given election when they believe the probability that their vote decides that election increases.¹ Other accounts also accommodate expected election closeness as important in encouraging voting, for example in some group-utilitarian models of voting (Coate and Conlin 2004) and in accounts where voters weigh the social benefits of voting (Edlin et al. 2007, pg 303).

However, whether that prediction reflects actual behavior is unclear, as prior investigations of the direct effect of variation in perceived electoral closeness on participation generally suffer from a number of threats to credible causal inference. For example, studies that exploit variation in observed election closeness or survey assessments of perceived closeness fail to rule out the possibility that omitted factors correlated with an election being close or someone believing it will be close (such as increased campaign or media activity) also affect the decision to vote (see Blais 2000 and Enos and Fowler 2014 for reviews of prior work). In contrast, survey (e.g., Ansolabehere and Iyengar 1994) and lab (e.g., Duffy and Tavits 2008) experimental approaches address endogeneity concerns but may not accurately reflect behavior outside those settings. Field experimental tests alleviate both sets of concerns, but almost all prior field studies

¹ Subsequent extensions have made voters’ decisions fully dynamic (e.g., Palfrey and Rosenthal 1983) and incorporate returns to participation (e.g., civic duty) unrelated to electoral outcomes (e.g., Riker and Ordeshook 1968; see Feddersen 2004 for a discussion of subsequent models).
compare closeness messages to other types of messages (e.g., Dale and Strauss 2009; Enos and Fowler 2014), which makes it difficult to ascertain whether any observed turnout effects arise due to (1) the framing of the vote choice in terms of closeness or (2) changes in perceptions of how close the election will be. Moreover, even experimental treatments about election closeness may not solely manipulate perceptions of pivotality. This is potentially true of the single field experimental paper that implements multiple closeness treatments (Gerber et al. 2020), because those treatments convey information about the closeness of that particular race (via polling margins). Whether a specific race is close may also alter perceptions of other factors that affect the decision to vote (e.g., the importance of the contest or the quality of the candidates).

In this paper, we present results from a novel field experiment that addresses the limitations of prior observational and experimental tests. We contacted more than 16,000 registrants in 7 states by phone during the 2014 Congressional primaries and delivered two messages that differed only in how close they (accurately) describe past primary races across the country as being (7% of past contested races were decided by either less than 350 or less than 2500 votes). Importantly, these interventions depart from prior field experiments on election closeness in that they do not communicate information about the specific race (or district) in question (e.g., whether one candidate is leading in the polls) but instead convey general information about the likelihood a race will be close given past races of that type nationwide. In this way, we hold constant both campaign contact and the framing of the turnout decision in terms of closeness, and isolate variation in expected closeness from all other information that might be conveyed by describing that race and that could affect the decision to vote through other mechanisms.
We find that registrants assigned to receive a message stating that past elections have been closer—the 350-votes condition—are 1.6 percentage points (6.5%) more likely to vote than those assigned to receive a message that past elections have been less close—the 2500-votes condition (p=.02, two-tailed). That difference is consistent across states, electoral context (whether the respondent lived in a competitive primary district), and past patterns of voter participation (though power issues mean estimated differences in effects within subgroups are generally not statistically significant in isolation). Furthermore, this message is more effective than a standard message that reminds the recipient of an upcoming election.

In summary, our findings demonstrate that outreach emphasizing that a class of elections is more likely to be closer increases participation. Thus, contrary to arguments that the Downsian model and related accounts are of limited value in understanding individual participation (e.g., Green and Shapiro 1994), we provide direct experimental evidence that this perspective helps explain variation in electoral turnout. The results also show that the perception that “one’s vote doesn’t matter,” a frequently cited justification for abstention, is malleable and not simply correlated with other factors that also explain low rates of participation. As such, those views can be manipulated so as to increase participation, potentially highlighting a mechanism for further raising political engagement. In the conclusion, we expand upon these implications and discuss alternative theoretical accounts that are compatible with our findings.

**Election Closeness and Participation**

The Riker-Ordeshook model of participation posits simply that individuals participate when \( P \times B - C > 0 \), where \( 0 \leq P \leq 1 \) is the probability one’s vote decides an election, \( B \geq 0 \) is the potential benefit to having one’s more preferred candidate defeat one’s less preferred candidate, and \( C \) is the cost of voting (finding one’s way to the polls, deciding for whom to vote,
etc.). Given that for all individuals \( C > 0 \), the crucial determinant of whether voting is “worth the cost” is how much one cares about the election outcome, \( B \), and the likelihood that one’s vote decides an election, \( P \). One may care a great deal about an election outcome (i.e., one may have a large \( B \)), but if voting is perceived to have no effect on the outcome (\( P = 0 \)), that individual will abstain even if the value of \( C \) is close to 0. By contrast, as \( P \) gets closer to 1—that is, as one believes their individual vote may have a realistic chance of deciding the election—the perceived stakes of the election (\( B \)) relative to the perceived costs (\( C \)) come into play. While some have criticized the model as predicting that no one will vote (because \( P \) will nearly always be exceedingly small, making \( P \cdot B \) almost surely less than \( C \)), scholars such as Blais (2000) note that there is substantial variation in individual perceptions of \( P \), and many individuals appear to overestimate the likelihood that their votes will prove decisive. Furthermore, as game theoretic models show, individuals must condition their beliefs about \( P \) on the behavior of others, and how that conditioning transpires empirically remains an open question. Allowing for variation in how individual perceptions of \( P \) manifest, and fixing \( B > 0 \), for some individuals there may be a \( P \leq 1 \) such that \( P \cdot B > C \), in which case voting is rational.2

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2 Riker and Ordeshook (1968) address the concern that the model predicts mass abstention by including an additional term, denoted by \( D \), that represents the participatory benefits one receives that are independent of the election outcome (e.g., the additional utility gained by fulfilling a sense of civic duty or gaining respect from one’s peers). To the extent that including these non-instrumental benefits is (or at least can be) consistent with the rational choice framework, then the basic logic remains the same: fixing \( B > 0 \), for some individuals there may be a \( P \leq 1 \) such that \( P \cdot B + D > C \), in which case voting is rational.
Despite the simplicity of this theoretical perspective and related models that consider social or group returns to voting and also predict a relationship between expected closeness and turnout, tests of the effect of how expected closeness affects turnout are limited in their credibility as causal evidence. There are four main approaches in this work. The first is to examine the aggregate relationship between observed election closeness and levels of turnout. Blais (2000) characterizes this literature as providing strong reasons to believe that individuals are more likely to vote when the contest is close, while a subsequent meta-analysis by Cancela and Geys (2016) is similarly supportive of this expectation but less conclusive. However, as prior scholars note (e.g., Aldrich 1993; Cox and Munger 1989), the mechanism explaining this result is ambiguous because the increased campaign activity and media attention often assigned to close contests may influence participation aside from common perceptions of one’s own pivotality (see also Matsusaka and Palda 1993). More generally, close elections may be close because many people vote or because of some other omitted factor that affects turnout.

The second approach eschews aggregate election-level analysis in favor of individual-level data. One advantage of this approach is that it is possible to measure individual-level assessments of perceived election closeness along with other factors that might predict voting. Some studies find evidence that individuals who think elections will be closer are more likely to vote (e.g., Blais et al. 2000). Nonetheless, many of the same threats to inference arise in this context because the sources of individual-level variation in perceived closeness may also affect other factors that increase turnout, or may reflect existing individual-level differences in the willingness to vote. In the absence of a full accounting of all (potential) factors that explain variation in participation (or correlated measurement error), the threat of omitted variable bias remains large.
Scholars have turned to a third approach—namely lab- or survey-experimental tests of the effects of variation in electoral closeness on participation—to address these concerns about credible causal inference. Ansolabehere and Iyengar (1994), for example, find in a survey that manipulating the poll results embedded in a newscast (i.e., making the race appear more or less competitive) has no effect on intention to vote. By contrast, Kam and Utych (2011) find that races described as close spur cognitive engagement, with subjects undertaking efforts (e.g., seeking out more information) consistent with the actions of someone more likely to participate.

Work in the lab, in which the returns and costs to voting are experimentally manipulated, also provides some support for the effect of expected closeness on voting. Some of this work focuses on analyses in which pivotality is endogenous to others’ anticipated actions (i.e., as an equilibrium outcome of a game; see, e.g., Feddersen and Pesendorfer 1996, 1999; Palfrey and Rosenthal 1983, 1985).3 Duffy and Tavits (2008) show that in a lab experiment where the costs and benefits of voting are fixed, a higher perceived probability that one is pivotal increases the propensity to vote, although the relationship is not as sharp as predicted by theory in light of the parameters manipulated in the game (see also Levine and Palfrey 2007).

For both types of experiments, one important concern is external validity: Subjects participating in a survey or playing a laboratory game may behave differently than they would if exposed to similar stimuli outside of the laboratory. This may occur either because the decision

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3 Though see Feddersen and Pesendorfer (1996) for conditions under which turnout is not necessarily related to electoral closeness (i.e., the “swing voter’s curse”), as well as lab experiments that find a positive relationship between turnout and margin of victory as the number of informed voters increases (Battaglini et al. 2010).
to vote (or express an intention to vote) is not an accurate reflection of real behavior, or because the way people make decisions in the lab setting is different from how they would behave outside of it. Additionally, in the case of prior survey experimental work, manipulations of closeness may generate variation not just in expectations about closeness, but also in beliefs about factors like aggregate turnout, which may affect beliefs about election importance and other relevant factors.

Finally, a fourth approach involves field experiments that manipulate the salience of electoral closeness. We summarize this prior research in Table 1. These studies have the potential to address concerns about omitted variable bias and endogeneity, as well as the artificiality of the survey and lab setting. Those studies that explicitly test the “closeness” hypothesis find mixed evidence that stressing the competitiveness of the contest increases turnout, but almost all of these studies compare a closeness message either to no contact or to an alternative outreach message (see columns D, E, and F). Thus, they do not estimate the effect of directly manipulating perceptions of election closeness, assuming instead that discussing election closeness (apart from the expected closeness of any particular election) does not affect participation. With only a single closeness treatment, however, they cannot rule out this violation of the exclusion restriction (i.e., they do not vary how close the election was described conditional on discussing closeness).

Gerber and Green (2000), for example, conclude that asserting via door-to-door canvassing or direct mail (but not phone) that each year some elections are decided by only a handful of votes increases turnout compared to no contact, but the effects are not distinguishable from those of other messages. Similarly, Bennion (2005) finds no evidence that stressing in canvassing that many elections in the state “will be decided by only a handful of votes” has a
larger effect on turnout than a standard civic duty message, while Dale and Strauss (2009) determine that text messages stating that “elections often come down to a few votes” increase turnout in comparison to an uncontacted control group, but actually have a smaller effect on turnout than a standard civic duty message. Enos and Fowler (2014) show that raising awareness of one’s potential pivotality following a special election in which the original contest ended in a tie between the two major party candidates increases turnout in the follow-up election vis-à-vis an election reminder, but the difference is not statistically significant.4

The single exception to the comparison of a closeness message to a non-closeness message is the two experiments reported in Gerber et al. (2020). They report results from a pair of studies explicitly designed to test the effects of manipulating perceived election closeness by providing polling margins in a particular race. In a 2010 panel study with treatments delivered online, they find that providing subjects with a close poll (one in which the race is depicted as very close) increased perceptions measured in the same pre-election survey that the final race will be close relative to a poll that was less close. However, using the close-poll treatment as an instrument for perceived election closeness, they find no evidence that inducing differences in perceived closeness increased turnout as measured using administrative records among those who also completed a post-election survey.

A second experiment in Gerber et al. (2020) was a large-scale (N=approximately 126,000) field experiment with treatments administered using mailed postcards. The treatments

4 Other field experiments employ treatments that use the word “close” or mention the number of votes that might decide the contest (often as part of a longer message) but do not explicitly test the “closeness” hypothesis (see, e.g., Matland and Murray 2012; Nickerson 2006, 2007).
were manipulated along two dimensions: close versus not-close polls and large versus small electorate (turnout). The experiment yields a statistically insignificant .3 point increase in turnout associated with the close-polls treatment. One concern with these experiments is that the polling margin in a particular race may convey information not just about its expected closeness, but also about its candidates (i.e., the race may be close precisely because of something about the relative qualities of the incumbent and challenger). Thus, it is not clear that the treatment perturbs only expected election closeness.5

2014 7-State Field Experiment

We conducted our field experiment during the 2014 primary elections in 7 states [Massachusetts (MA), Michigan (MI), Minnesota (MN), Missouri (MO), New Hampshire (NH), Tennessee (TN), and Wisconsin (WI)] in which all registered voters can vote in at least one party’s primary election.6 We first obtained a complete list of registered voters in each state. Prior to treatment assignment we excluded records likely to be invalid or persons who could not be contacted by phone. In households with multiple registrants, one registrant was selected at random for inclusion in the sample. From this pool, subjects were then randomly assigned to one of four treatment groups described below—in brief, a placebo, a traditional election reminder, a close elections message, and a less-close elections message. Treatment assignment was stratified by

5 The direction of this bias is unclear. For example, close elections may indicate equal candidate quality (decreasing the stakes of voting) or a sharp division on policy (increasing it).

6 In MA and NH, affiliated voters cannot vote in another party’s primary, but unaffiliated voters can vote in any party’s primary. In the other five states in the study, all registrants can vote in any primary.
state, whether the registrant lived in a district with a competitive House race, and an individual’s past record of voter participation.  

Each message was delivered by telephone in the four days leading up to each state’s primary election by a professional survey vendor we hired. All interventions began with the same question asking whether the subject was a resident of his or her state. Subjects who answered in the affirmative were coded as contacted and treatments were then delivered. As this

7 Approximately 31% of our subjects were selected from competitive districts (listed in the online appendix) and about 69% from non-competitive districts. We partitioned subjects based on their turnout histories as recorded in the voter file for the years 2008-2012 into those who have voted in at least one prior primary election (primary election voters), those who have voted before but never in a primary election (general election voters), and those with no prior history of voting (never voters). We oversampled general election voters and undersampled primary election voters. See the online appendix for full details.

8 Complete treatment scripts are provided in the online appendix.

9 Treatment assignment rates differed slightly by state because of changes we made to the experimental protocol while the experiment was in the field for states with different primary dates. In MI, MO, and TN, 40% of registrants were assigned to the Placebo message and 20% were assigned to each of the remaining three treatments. In the other states, about 33% of registrants were assigned to the election reminder treatment and about 22% were assigned to each of the remaining three messages. These different rates reflect our desire to have a larger Placebo or election reminder sample for comparisons to unrelated treatments not analyzed here. We eliminated certain unrelated treatments after the experiment was administered in MI, MO,
question was asked prior to the portion of each script that branches into the assigned treatment group, we use this common (treatment-independent) definition of contact so that inclusion in the analysis is not potentially affected by variation in the subsequent treatment content. Voting in the 2014 primary was measured using turnout as recorded in updated state voter files obtained from our vendor in spring 2015. Individuals are coded as having voted if they are listed as having done so in the official record, and as not having voted otherwise.

Our core treatments were messages that emphasized the potential closeness of the election but that varied in the (accurate) information they conveyed about how close the race would be. Both messages began with an informational prompt and asked the registrant if they were aware of the upcoming primary. Following this, both scripts included the following message, after which the call concluded:

Because fewer people vote in most primary elections than in general elections, each vote matters more for deciding who wins. In fact, of the approximately 160 seriously contested primaries for the US House in 2012, more than 7% were decided by fewer than \([# \text{ OF VOTES}]\) votes. Think about how you will feel if you don't vote and it turns out the election was decided by only a few votes.

In the Closeness 350 treatment, the number of votes was 350. In the Closeness 2500 treatment, the number of votes was 2500. To arrive at these figures, we examined returns for the 2012 House primary elections and found that 162 had a margin of less than 25 points, and we coded these as being “seriously contested.” In these races, 12 (7.4%) were decided by fewer than

and TN in order to ensure sufficient contacts in each retained cell given our budget, and this caused us to change our assignment rates for the other treatments. Our subsequent analysis accounts for these different rates of assignment. See the online appendix for additional details on sampling and treatment assignment.
350 votes, and therefore were decided by fewer than 2500 votes as well. Importantly, therefore, by using this specific language we are able to avoid deception.\textsuperscript{10} Note that these two treatments do not mention anything about the particular primary contest in the respondent’s district and hold constant all features apart from how close 7\% of elections are. As such, we believe this makes it less likely that subjects infer features of their particular race from the variation in the closeness treatment (although they may infer something about the race in general from the fact that someone sent them a message at all, or react to framing voting in terms of closeness, reasons we compare outcomes across the two closeness messages). Additionally, the treatments communicate that turnout is generally low in primary elections, fixing expectations about average turnout across treatments.

Our third treatment was a standard script message asking the respondent whether they were aware of the upcoming primary election. It was similar to the opening script for the Closeness messages, but also mentioned that turnout was expected to be high. Finally, our fourth treatment was a Placebo message with no political content; after confirming a subject was a resident of their state, they were asked how often they went to the grocery store. To avoid simply terminating the placebo call after confirming state of residence because it might be awkward, we asked respondents about grocery store visits because it was an innocuous non-political question compatible with consumer marketing surveys.

\textsuperscript{10} The study was ruled exempt by the Institutional Review Board of [UNIVERSITY NAME DELETED TO PRESERVE ANONYMITY]. As such, the requirement of informed consent with respect to the experiment was waived.
We note that all comparisons are among a sample defined in a homogenous way: Those we can contact on the phone and confirm their state of residence. The identification of our control group in this manner allows us to compare turnout across conditions among registrants we are able to contact via phone—a subset of the entire population but one that is most important for assessing the effectiveness of mobilization efforts that take place using live phone calls. At the same time, this means that, as with all observational and experimental designs in which a subset of the population is not contacted, we must exercise caution in assuming treatment effects would be the same among those we cannot contact.

Our vendor contacted 8,453 registrants in the Closeness 350 condition, 8,402 in the Closeness 2500 condition, 11,591 in the election reminder condition, and 10,487 in the Placebo condition. Balance tests show that treatment groups did not vary materially on all covariates available in the voter file (age, year of registration, gender, race/ethnicity, and the number of times having voted in previous general, primary, and special elections).\footnote{The mean age of our respondents across conditions was between 62 and 63 years and the number of years since registration was between 16 and 17 (see online appendix Table A1), meaning that our sample is considerably older than the eligible electorate. This likely stems from the fact that the sample that is able to be reached by phone is considerably older than the total electorate, although older voters are typically over-represented in relatively low-salience contests such as midterm congressional primaries.}

\footnote{The chi-squared test from a multinomial logit model predicting treatment assignment based on these covariates is not significant (p = .95, see online appendix Table A1).}
Results

Our key empirical test is whether turnout is higher among those in the Closeness 350 treatment than the Closeness 2500 treatment. This analysis appears in Table 2.\textsuperscript{13} As the first row of the table shows, for the entire pooled sample, 26.0% of respondents contacted for the Closeness 350 treatment voted, compared to 24.5% of those contacted for the Closeness 2500 treatment. The difference in these proportions, shown in column (3), is 1.6 points (\(p=.02\), two-tailed). That difference-of-proportions test does not accommodate weights or covariates, so in column (4) we present a regression estimate of the effect of the Closeness 350 treatment relative to the Closeness 2500 treatment. Complete OLS regression results with robust standard errors appear in Table A2 of the online appendix and control for assignment strata (State x Vote History x District Competitiveness) and state interacted with all of the pre-treatment covariates for which we assess balance.\textsuperscript{14} Cases are weighted to account for different rates of assignment across treatment strata and state, which is necessary to avoid bias when treatment rates vary by strata (Green and Gerber 2012).\textsuperscript{15} The regression estimate is 1.2 points (\(p<.05\), two-tailed test),

\begin{itemize}
  \item \textsuperscript{13} Table A5 in the online appendix presents the proportion of our sample that voted in each experimental condition by the same state, district competitiveness, and vote history subsets as presented in Table 2.
  \item \textsuperscript{14} To avoid information loss of cases through listwise deletion, we imputed the mean value of each measure when missing and added an indicator for whether the measure was missing. Furthermore, the New Hampshire voter file did not have exact dates for original registration, instead only indicating whether the measure was missing or not. Therefore, we imputed the mean value of years since registration for all subjects from New Hampshire.
  \item \textsuperscript{15} Results are nearly identical when weights are not applied (results available upon request).
\end{itemize}
meaning that turnout is about 5% higher among those who receive the Closeness 350 message than the Closeness 2500 message. (In a model excluding covariates, the estimated effect is 1.1 points, \( p=.05 \), two-tailed, showing that as expected in a large experimental sample covariates do not meaningfully affect treatment effect estimates; see online appendix Table A2). As column (5) shows, this estimate is based on 16,855 completed calls.

The remainder of the table shows the consistency of this result for different states, electoral contexts, and past voter history. Generally, the regression estimates are indistinguishable from the 1.2 point estimate for the entire sample. (The estimates for the subsamples are not usually individually statistically significant, reflecting the smaller samples.) Focusing on the regression estimates (which are less sensitive to potential imbalance created by sampling variability), we estimate that the Closeness 350 message is more effective than the Closeness 2500 message in 6 of 7 states. Additionally, it appears equally effective in districts with or without a competitive House primary (estimated effect of 1.0 and 1.2 percentage points, respectively). Finally, the message appears effective for all partitions of past voter history (while the point estimate for never voters is noticeably larger than for primary election and general election voters, that effect is derived from significantly fewer cases and is not statistically distinguishable from the effects for those other types of voters).

These results show that otherwise identical messages that differ only in how close they describe a past similar election as being can increase turnout when those previous elections are

\[16\] Inferences from OLS remain unchanged if we instead use logistic regression (see online appendix Table A4).
closer. This is direct evidence that a message designed to create an expectation that an election will be closer will bring more people to the polls.

Additionally, our experimental design also allows us to assess the comparative effectiveness of the closeness messages. For this analysis, we compare turnout in four conditions: Those who received each Closeness message, those who received the standard election reminder, and those who received the Placebo message. Figure 1 displays the comparative effectiveness of each treatment in increasing participation relative to the placebo message (the 95% confidence interval for each estimate is indicated with the black capped lines). These estimates are derived from a regression model similar to that used in the Table 2 analysis (see Table A3 of the online appendix). Compared to the placebo condition, those who received the Closeness 350 message are 3.0 points more likely to vote, which represents a proportional increase in turnout of 13% compared to the 22.7% turnout rate in the placebo condition. The Closeness 2500 and election reminder messages are both more effective than the Placebo message (by 1.8 and 2.0 points, respectively), but neither is as effective as the Closeness 350 message. Thus, the evidence indicates that providing information designed to heighten perceptions that the election is close increases turnout compared to an otherwise identical message that makes the election seem less likely to be close, and the Closeness 350 message is more effective than a standard election reminder script (p=.05, two-tailed test).

Conclusion

Does variation in expected election closeness, a core factor in the canonical decision-theoretic turnout calculus and other related accounts, explain voting? We provide experimental evidence in the field setting that communication manipulating the expected closeness of a class of elections increases participation. Some individuals are more likely to participate in an election
when they are informed that their individual votes are more likely, as opposed to less likely, to be decisive. Importantly, we compare across closeness treatments, fixing the framing of the decision to vote, and deploy treatments that provide information about expected election closeness that is independent of the expected margin in a specific race. These treatments are particularly novel compared to prior work, because polling margins in a specific race may convey information beyond expected closeness (e.g., about expected turnout and incumbent and challenger characteristics) that might on its own influence the decision to vote.

We note that while these effects appear robust and our treatments are designed to hold constant other factors that may also affect voting, our design does have some important limitations. One potential concern is the generalizability of the treatment effects. On the one hand, the fact that our experimental sample consisted of voters who could be reached by phone means that they are an unusual type of voter compared to the general electorate. It is not clear what the implications of this fact are for the ability to generalize the effects of mobilization efforts emphasizing election closeness. On the other hand, such voters are likely to be older (as, indeed, our sample is), and hence have a higher baseline propensity to turn out and a more muted reaction to any mobilization effort. This latter fact might lead us to speculate that the treatment effects found here are actually conservative estimates of how similar treatments would impact the wider electorate. This constitutes an interesting avenue for future research.

Furthermore, we do not directly show that our treatments increase turnout by increasing the perceived instrumental returns to voting or how exactly those benefits are understood (e.g., in individual or group terms). These results show that election closeness does appear to matter, and that all else equal, closer elections do drive greater participation. It is possible though that the treatments also perturb other relevant and likely consequential causal pathways—beliefs about
civic duty (an intrinsic motivation to vote), expectations about peer behavior and evaluations (social norms), or the returns to a political group (group utilitarian perspectives), for example. Pairing our tests with survey data (to ascertain whether the treatment affects the theoretical construct we designed it to alter and whether it affects arguably unrelated concepts) is another valuable area for future work.

Additionally, while we frame our inquiry largely in terms of the Downsian perspective, we note that our empirical results are compatible with other theoretical perspectives as well. For instance, it may be that the expectation that a class of elections will be closer triggers a general psychological engagement with the current race. Alternatively, it may be that the expectation of a closer race triggers more discussion with peers about politics, and that this political engagement is the mechanism that leads to turnout (see Edlin et al. 2007 for an extended discussion).

Whatever account one prefers, our design is, we believe, the first to successfully isolate this closeness mechanism in a field setting.

Those limitations aside, our findings open new avenues for identifying messages that successfully mobilize citizens. Contrary to frequent claims in prior survey work that many individuals believe that their decisions about whether to vote will not affect an election outcome, perceptions of election closeness (and anticipated pivotality) appear malleable, and we now have initial evidence that altering them increases the propensity to vote. Future work should examine how (and for whom) messages stressing electoral closeness can best be leveraged to bring potential voters to the polls.
References


Figure 1: Comparative Effectiveness of Different Treatments

Note: Among those contacted, N = 38,933. Placebo group turnout is 22.7%. Bar heights are point estimates; capped lines are 95% confidence intervals.
Table 1. Prior Field Experiments Examining the "Closeness" Hypothesis

<table>
<thead>
<tr>
<th>Study</th>
<th>Mode of Contact</th>
<th>Closeness Language</th>
<th>Compare between &quot;Closeness&quot;?</th>
<th>Compare to Other Treatment/Control?</th>
<th>Results</th>
<th>Sample Size</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gerber and Green (2000)</td>
<td>Door-to-Door</td>
<td>“Each year some election is decided by only a handful of votes. Who serves in important national, state, and local offices depends on the outcome of the election, and your vote can make a difference on election day.”</td>
<td>N</td>
<td></td>
<td>Compared to Civic Duty Message (ATT): .030</td>
<td>Control = 23,586 Neighbors = 1881 Civic Duty = 1985 Closeness = 1928</td>
</tr>
<tr>
<td>Gerber and Green (2000)</td>
<td>Phone</td>
<td>Mirrored language from door-to-door canvassing script, but exact language not reported.</td>
<td>N</td>
<td></td>
<td>Pooled treatments compared to No Contact (ATT): -.035 Closeness compared to other types of contact not reported</td>
<td>Control = 22,626 Treatments combined = 6754 Sample size by message type not reported</td>
</tr>
<tr>
<td>Gerber and Green (2000)</td>
<td>Direct Mail</td>
<td>“This year many elections will be decided by only a handful of votes—will yours be the deciding vote?”</td>
<td>N</td>
<td></td>
<td>Pooled treatments compared to No Contact (ATT): .002* Closeness compared to other types of contact not reported</td>
<td>Control = 14,661 Treatments combined = 14,719 Sample size by message type not reported</td>
</tr>
<tr>
<td>Bennion (2005)</td>
<td>Door-to-Door</td>
<td>“In an election, anything can happen. This year many Indiana elections will be decided by only a handful of votes—will yours be the deciding vote?”</td>
<td>N</td>
<td></td>
<td>Compared to Civic Duty Message: -.017 Pooled treatments compared to No Contact: .006 Closeness compared to No Contact not reported</td>
<td>Control = 1089 Closeness = 544 Civic Duty = 544</td>
</tr>
<tr>
<td>Dale and Strauss (2009)</td>
<td>Text Messages</td>
<td>“Elections often come down to a few votes—so please vote!”</td>
<td>N</td>
<td></td>
<td>Compared to Civic Duty Message: -.006 Compared to No Contact: .027*</td>
<td>Control = 4046 Closeness = 1974 Civic Duty = 2033</td>
</tr>
</tbody>
</table>
Enos and Fowler (2014) Phone

“The reason that there is a special election is that the last election ended in an exact tie. Had one more or one less person voted in the last election, your candidate would have won. The special election on Tuesday is likely to be close again, so there is a high chance that your vote could make a difference.”

Gerber et al. (2020) Internet Survey

“Below are the results of one recent poll about the race for governor. The poll was conducted over-the-phone by a leading professional polling organization. People were interviewed from all over the state, and the poll was designed to be both non-partisan and representative of the voting population. Polls such as these are often used in forecasting election results. Of people supporting either the Democratic or Republican candidates, the percent supporting each of the candidates were: <CANDIDATE NAME> <##>%. <CANDIDATE NAME> <##>%”

Gerber et al. (2020) Direct Mail

“Below are the results of one recent poll about the race for <office> in <state>. The poll was conducted by a leading professional polling organization. People were interviewed from all over <state>, and the poll was designed to be both non-partisan and representative of the voting population. Please keep in mind that this is just one poll. Polls such as these are often used in forecasting election results. Of people supporting either of the two leading candidates, the percent supporting each of the candidates was: <CANDIDATE NAME>-<PARTY> <##>%. <CANDIDATE NAME>-<PARTY> <##>%”

Gerber and Green (2000) do not report turnout rates across treatments for mail and phone experiments, only that the effects are not statistically distinguishable from each other. Gerber and Green (2000) direct mail effect estimate derived from dividing treatment effect by number of mailings. Other field experiments employ treatments that use the word “close” or mention the number of votes that might decide the contest (often as part of a longer message) but do not explicitly test the “closeness” hypothesis (see, e.g., Matland and Murray 2012; Nickerson 2006, 2007). *p<.05.
<table>
<thead>
<tr>
<th>Sample</th>
<th>Proportion Voting, 350 Votes Treatment</th>
<th>Proportion Voting, 2500 Votes Treatment</th>
<th>Difference of Proportions (350 votes - 2500 votes) [Standard Error]</th>
<th>Regression Estimate of Difference (350 votes - 2500 votes) [Standard Error]</th>
<th>Number of Observations (350 votes, 2500 votes)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Entire Sample</td>
<td>0.260</td>
<td>0.245</td>
<td>0.016 [0.007]</td>
<td>0.012 [0.005]</td>
<td>(8453,8402)</td>
</tr>
<tr>
<td>State=Massachusetts</td>
<td>0.272</td>
<td>0.251</td>
<td>0.021 [0.017]</td>
<td>0.013 [0.014]</td>
<td>(1335,1336)</td>
</tr>
<tr>
<td>State=Michigan</td>
<td>0.233</td>
<td>0.222</td>
<td>0.012 [0.019]</td>
<td>-0.005 [0.014]</td>
<td>(1050,993)</td>
</tr>
<tr>
<td>State=Minnesota</td>
<td>0.178</td>
<td>0.165</td>
<td>0.013 [0.011]</td>
<td>0.010 [0.010]</td>
<td>(2295,2372)</td>
</tr>
<tr>
<td>State=Missouri</td>
<td>0.376</td>
<td>0.343</td>
<td>0.033 [0.028]</td>
<td>0.033 [0.024]</td>
<td>(585,568)</td>
</tr>
<tr>
<td>State=New Hampshire</td>
<td>0.321</td>
<td>0.306</td>
<td>0.015 [0.030]</td>
<td>0.034 [0.024]</td>
<td>(474,484)</td>
</tr>
<tr>
<td>State=Tennessee</td>
<td>0.429</td>
<td>0.414</td>
<td>0.015 [0.028]</td>
<td>0.011 [0.022]</td>
<td>(653,636)</td>
</tr>
<tr>
<td>No Competitive House Primary</td>
<td>0.250</td>
<td>0.235</td>
<td>0.016 [0.008]</td>
<td>0.012 [0.007]</td>
<td>(5867,5881)</td>
</tr>
<tr>
<td>Either House Primary Competitive</td>
<td>0.282</td>
<td>0.268</td>
<td>0.015 [0.012]</td>
<td>0.010 [0.010]</td>
<td>(2586,2521)</td>
</tr>
<tr>
<td>Ever Voters (Have Voted Before)</td>
<td>0.267</td>
<td>0.252</td>
<td>0.015 [0.007]</td>
<td>0.011 [0.006]</td>
<td>(8168,8106)</td>
</tr>
<tr>
<td>Have Voted in Primary</td>
<td>0.524</td>
<td>0.515</td>
<td>0.009 [0.013]</td>
<td>0.013 [0.011]</td>
<td>(3164,3034)</td>
</tr>
<tr>
<td>Have Voted, but Never in Primary</td>
<td>0.105</td>
<td>0.095</td>
<td>0.010 [0.006]</td>
<td>0.010 [0.006]</td>
<td>(5004,5072)</td>
</tr>
<tr>
<td>No Prior History of Voting</td>
<td>0.056</td>
<td>0.044</td>
<td>0.012 [0.018]</td>
<td>0.033 [0.017]</td>
<td>(285,296)</td>
</tr>
</tbody>
</table>

Note: The estimates in column (4) were generated from regression models including strata (state × vote history × district competitiveness) fixed effects and state interacted with indicators for age, year of registration, sex, race/ethnicity, and the number of times voted in general, primary, and special elections (complete model results are reported in online appendix Table A2).