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ONE IN A MILLION:
FIELD EXPERIMENTS ON PERCEIVED CLOSENESS OF THE ELECTION AND VOTER TURNOUT

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ABSTRACT

A common feature of many models of voter turnout is that increasing the perceived closeness of the election should increase voter turnout. However, cleanly testing this prediction is difficult and little is known about voter beliefs regarding the closeness of a given race. We conduct a field experiment during the 2010 US gubernatorial elections where we elicit voter beliefs about the closeness of the election before and after showing different polls, which, depending on treatment, indicate a close race or a not close race. We find that subjects update their beliefs in response to new information, but systematically overestimate the probability of a very close election. However, the decision to vote is unaffected by beliefs about the closeness of the election. A follow-up field experiment, conducted during the 2014 gubernatorial elections but at much larger scale, also points to little relationship between poll information about closeness and voter turnout.

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An online appendix is available at http://www.nber.org/data-appendix/w23071

1 Introduction

A core question in political economy is why do people vote. In classic instrumental models of voting, such as the private values model introduced by Downs (1957) and Riker and Ordeshook (1968) and the common values setting of Feddersen and Pesendorfer (1996), natural assumptions lead to the prediction that individuals are more likely to vote when they believe the election to be close. Moreover, even in some of the leading alternative models such as the "ethical voter" framework of Feddersen and Sandroni (2006) or the signalling model of Razin (2003), where pivotality does not directly influence the decision to vote, turnout may still influenced by beliefs about the margin of victory.

Researchers have employed two main approaches to test the prediction that increases in the perceived closeness of the election increases turnout. The first vein, as surveyed by Cancela and Geys (2016), uses observational data from real-world elections, and shows that turnout tends to increase in measures of actual or predicted closeness across elections. However, as noted by Shachar and Nalebuff (1999) and Shachar (2007), it is hard to interpret any estimated effects as supporting theory, as numerous other factors are correlated with an election being close (e.g., greater voter mobilization by elites and greater media coverage). Further, observational closeness may be correlated with information asymmetries (Battaglini et al., 2010). The second vein (e.g., Levine and Palfrey, 2007; Duffy and Tavits, 2008; Großer and Schram, 2010; Agranov et al., Forthcoming) uses lab experiments to more cleanly identify the causal effect of beliefs or to study the impact of polls. However, these experiments abstract from the context of real-life elections and so may fail to account for the various factors that are salient outside the lab. Perhaps in part due to these challenges, recent empirical work on voter turnout has often focused on testing non-instrumental models, e.g., that turnout reflects confidence (Ortoleva and Snowberg, 2015), social incentives (DellaVigna et al., 2017), habit (Fujiwara et al., 2016), or the media (Gentzkow et al., 2011; Spenkuch and Toniatti, 2016).

To provide a cleaner test of theory and to understand how voters form beliefs about the closeness of elections, we combine the two approaches. We conduct two large-scale field experiments in the US that exogenously shift voters' beliefs about the election being close. In both experiments, we find no evidence that believing the election is close raises turnout. This suggests that, for the case of large US elections, beliefs about the closeness of an election are not a main driver of voter turnout.

The first experiment was conducted during the 2010 U.S. gubernatorial election cycle and included over 16,000 voters. As described in Section 3, using computer surveys in 13 US states, we asked potential voters to predict the vote margin, as well as their beliefs about the chance that the governor's race would be very close (e.g., decided by less than 100 votes). Exploiting variation in real-world poll results prior to the election, we divide subjects into groups. We informed the "Close" group of the results of a poll indicating the narrowest margin between the two candidates, whereas the "Not Close" group saw a poll indicating the greatest gap between the candidates. (In addition, there was a third group ("Control") who received no poll information and did not get surveyed.) After the election, we used administrative data to determine whether people actually voted. Using the 6,700 voters for whom we have data on beliefs, we obtain three main findings, which we present in Section 4:

1. Prior to being exposed to polls, most subjects overestimate the probability of a very close election. The median probabilities that the gubernatorial election would be decided by less than 100 or less than 1,000 votes were 10% and 20%, respectively, much higher than the historical averages. While such overestimation of low probabilities has been widely observed in other contexts, we are the first to precisely estimate its magnitude in the context of voting.

2. Both in terms of margin of victory and the probability of a very close race, voters strongly update their beliefs about the closeness of the election in response to polls. For example, as a result of receiving a close poll, there was a 2.5 percentage point increase in the perceived probability the election would be decided by less than 100 votes, which represents a 25% increase relative to the pre-treatment median. Conditional on updating at all, there was a 7.3 percentage point increase in the perceived probability the election

would be decided by less than 100 votes.

3. Most importantly, these changes in beliefs do not translate into behavior as predicted by instrumental voting models (even if individuals misperceived probabilities about closeness). Although many models imply that belief changes translate into changes in turnout, we find no such connection—voter turnout is statistically independent of beliefs about closeness.

While the 2010 experiment is able to establish that the effect of beliefs on turnout is small (if any), a larger sample is required to confidently establish whether the effect is approximately zero or merely small. To address this, we conducted a second large-scale field experiment during the 2014 gubernatorial elections, described in Section 5. We randomly mailed postcards to about 80,000 households (about 125,000 individuals) where we again provided information from the most close or least close poll. Including the control households that didn't get postcards, we have a sample size of over 1.38 million voters. In this much larger sample, we find results consistent with the 2010 experiment. Relative to the "not-close poll" postcard, there was no significant impact of the "close poll" postcard on turnout. Based on our confidence intervals, we can rule out that a close poll (vs. a not-close poll) increases turnout by more than 0.8 percentage points. (For the remainder of the paper, we abbreviate percentage points by "pp.")

Section 6 presents additional evidence that helps rule out alternative explanations. We show that our null result is robust to analyzing a person's immediate voting intentions, thereby helping address the concern that our null finding is driven by belief convergence after the intervention. Our null result is robust to sub-samples that might seem more conducive for finding impacts of closeness beliefs on turnout. In our preferred specification combining data from the 2010 and 2014 experiments, with 95% confidence, we can rule out that more than 13% of the observed relationship between actual closeness and turnout is driven by perceptions of closeness. Thus, the two experiments together provide substantial statistical precision.

Overall, our results are inconsistent with an electoral calculus whereby voters compute the expected benefit of voting (perhaps incorrectly) and then adjust turnout and voting behavior accordingly. Rather, the results seem to suggest that elite mobilization efforts and/or non-instrumental considerations (e.g., expressive voting) may be important for voter turnout in large elections (though we are at pains to stress that we have no direct evidence of these alternative considerations). We view this as an important contribution, as models that incorporate instrumental and pivotal motives are still very popular in top journals.[1]

A common approach to testing models of turnout is to use observational data. Broadly consistent with instrumental models, turnout tends to rise in elections that are closer or have smaller electorates.[2] But there are many confounds in comparing turnout across elections. Close elections tend to have more campaign spending (Cox and Munger, 1989; Matsusaka, 1993; Ashworth and Clinton, 2007), more party contact (Shachar and Nalebuff, 1999; Gimpel et al., 2007), more campaign appearances (Althaus et al., 2002), and more news coverage (Banducci and Hanretty, 2014). Like sporting events, tight races may be more interesting to monitor and discuss than walkovers, and may spur greater attention from one's friends. Close elections may spur elites to increase social pressure to vote (Cox et al., 1998); alternatively, potential impacts of electoral closeness on turnout, even if small, may be amplified by peer effects in voting (Bond et al., 2012) or social pressure (Gerber et al., 2008). Thus, it is very hard to tell whether greater turnout occurs because individuals believe they will have a higher probability of influencing the election or because of other reasons correlated with the election being close (Cox, 1999, 2015).

One way to try to address these confounds is to consider types of elections where confounds seem less likely. For example, in important articles, Coate and Conlin (2004) and Coate et al. (2008) study small-town liquor ban elections and Hansen et al. (1987) study school ref-

[1] Appendix Table C1 provides a non-comprehensive list of such papers published in "Top 5" economics journals in 2000-2015. There are 40+ papers listed, with thousands of Google Scholar citations among them, thus indicating that instrumental voter models are **not** a "straw man" with no place in frontier research (Spenkuch (2017) also makes a similar point). While some of these papers are motivated primarily by committees and other small elections, many are motivated by trying to explain behavior in large elections.

[2] Foster (1984) and Matsusaka and Palda (1993) provide surveys of the literature on turnout. Based on meta-analysis of 83 studies, Geys (2006) concludes that "Turnout is higher when the population is smaller and the election closer." Most papers measure closeness using ex post / realized closeness, but Shachar and Nalebuff (1999) and Bursztyn et al. (2017) show that turnout is also higher when predicted closeness is higher.

erenda, all finding that turnout decreases with the size of the electorate. However, it is hard to fully overcome the concern that there could have been greater attempts at mobilization in races with a smaller electorate (or in closer races). Another promising direction is to exploit differences in the availability of poll information, e.g., whether a region votes before or after exit polls are known (Morton et al., 2015) or whether poll information is available in different regional newspapers (Bursztyn et al., 2017), with both papers finding results consistent with instrumental models. However, it is hard to rule out that elites may respond to the presence of poll information; that newspapers may be more likely to provide polls when there is greater local interest in a race; or that observed effects of poll-predicted closeness may be largely driven by social pressure or peer effects (given that treatments are not at the individual level) as opposed to individual perceptions of closeness.

A complementary approach to examine whether closeness affects turnout is to use lab experiments. Though samples are generally small, they can rule out mobilization responses and other confounds. Duffy and Tavits (2008) elicit subjects' perceived chance of being pivotal in lab elections, showing that a higher perceived chance of being pivotal is associated with a higher probability of turning out. Similarly, Levine and Palfrey (2007) find strong evidence of higher turnout in smaller elections and when the election is closer. Großer and Schram (2010) and Agranov et al. (Forthcoming) expose lab voters to different polling information regarding the distribution of their induced preferences, showing that turnout is higher when the expected margin of victory is lower.[3]

While lab experiments have the advantage of full experimental control, the benefit of field experiments is to capture the context of real-life elections. To our knowledge, our experiments represent the first large-scale field experiments that randomly assign polls to voters so as to examine the impact on turnout.[4] In addition, we are aware of very few studies that seek

[3]Duffy and Tavits (2008), Großer and Schram (2010), and Agranov et al. (Forthcoming) vary whether people are randomly assigned to receive polls, which is ideal for examining whether the presence of polls affects turnout. In contrast, we additionally randomly vary whether the polls received are close or not close, allowing us to examine how shocks to beliefs affect turnout. Lab experiments have also been used to test particular theories of voting, including swing voter theories (Battaglini et al., 2010) and expressive theories (Tyran, 2004; Shayo and Harel, 2012; Kamenica and Brad, 2014). See Palfrey (2009) for an overview.

[4]Ansolabehere and Iyengar (1994) randomly assign one of two polls to around 400 voters. They find that

to measure or influence voter beliefs about electoral closeness.[5] In removing the confounds in observational data, our paper provides arguably the first direct, large-scale test of the closeness-turnout comparative static in the literature (economics or political science). Of course, closeness beliefs may still be important in small elections.

Arguably most related to our paper is a contemporaneous field experiment by Enos and Fowler (2014), who study a special Massachusetts state house race that ended previously in a tie. The authors randomly informed some voters by phone both that the previous election ended in a tie and that the new election is likely to be close, and, consistent with our findings, find no impact of the intervention on turnout (except perhaps among a subgroup of voters with high typical turnout). Our paper goes beyond Enos and Fowler (2014) in several respects. First, our study directly measures voter beliefs about closeness, allowing us both to characterize voter beliefs (which is a contribution in itself) and to directly measure how beliefs affect turnout.[6] Second, our sample size is much larger in both of our experiments (Enos and Fowler (2014) had 936 contacted persons in their data), allowing us substantially more statistical power. Third, we provide evidence from 20 elections instead of 1 election, thereby providing greater external validity. Fourth, we consider how our results relate to a broad range of voting theories.[7]

the closer poll does not affect whether people intend to vote (measured with a 0/1 variable), consistent with us, but that it does affect vote choice preferences. Besides being much smaller, this study does not measure actual turnout, nor does it measure voter beliefs about the probability of a very close race or about predicted margin of victory (they asked voters, who do you think will win?). Kendall et al. (2015) measure and randomly shock voters' subjective beliefs regarding candidate valence and policies (instead of regarding election closeness).

[5]There is a small literature on "probabilistic polling" that measures voters' beliefs about the chance they will turn out or vote for particular candidates (e.g., Delavande and Manski, 2010). However, to our knowledge, this literature does not measure beliefs about electoral closeness, nor does it experimentally manipulate the beliefs. Although they do not measure beliefs, Blais and Young (1999) conduct an experiment where they randomly teach students about the "paradox of voting," finding that the experiment decreases turnout by 7pp. However, they interpret their results as operating by affecting respondents' sense of duty.

[6]This is important because it enables us to measure how different aspects of beliefs affect turnout, including the predicted vote margin and the probability of a very close election.

[7]In addition, beyond Enos and Fowler (2014), Gerber and Green (2000) study the effects of different messages in canvassing, telephone calls, and direct mail on turnout. One message is: "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." They find no differential impact of this "close message" on turnout compared to other messages. However, because their close message does not provide any information about whether the current race is close, it may have no impact on voters' beliefs about the closeness of the current race (and there is no way to know if such wording affects closeness beliefs because beliefs are not measured). Thus, Gerber and Green (2000) do not provide evidence on how

2 Theoretical Considerations

Our main empirical exercise is to study how exogenous changes in beliefs about election outcomes affect turnout. This section describes verbally to what extent different theories of voting predict a testable prediction (Prediction 1): that seeing a close poll leads to higher turnout. Accompanying Section 2, Appendix D shows formally how different classes of voting models, in conjunction with a generalized version of Bayes' Rule, generate Prediction 1.

In sum, Prediction 1 (abbreviated "P1") is generated by many instrumental voting models, but many non-instrumental models will fail to produce the comparative static.

Prediction 1 (P1): *All else being equal, observing the close poll, compared to the not-close poll, leads to a higher chance of voting (versus abstaining).*

P1 most clearly emerges from the classic private values instrumental voting model of Downs (1957), and later extended by Ledyard (1981), Palfrey and Rosenthal (1983), and others. In such models, individuals compare the costs and benefits of voting, where the benefits are proportional to the probability of being decisive. Thus, individuals become more likely to vote when they believe the election to be closer.

A more general approach contemplates that voters have both ideological and valence elements to preferences, as in Feddersen and Pesendorfer (1997). Here, voters receive (private) signals about the valence (i.e., quality) of candidates and vote based on their assessment of ideology, candidate quality, and the chance of affecting the outcome. Observing a poll showing one candidate leading strongly then has two effects—it potentially informs voters about quality differences and about the likelihood of being decisive. The former effect raises the value of voting, as voters are now more certain of the quality of the leading candidate. The latter effect reduces the value of voting, since one vote is less likely to be decisive. So long as ideology dominates valence in the mind of the voter, and we consider only individuals who support the

perceived closeness affects turnout. In follow-on studies to Gerber and Green (2000), Bennion (2005) and Dale and Strauss (2009) also find no differential impact of very similar messages that elections have the general potential to be close.

minority candidate, then P1 continues to hold.[8]

A separate strand of the instrumental voting literature views voting as a means of signaling, either to other voters or to those in power (Razin, 2003; Piketty, 2000). Such signals presumably affect the policy chosen by the election winner. Thus, even if a vote is unlikely to change the candidate chosen, the effects on policy might still motivate a voter to come to the polls. In principle, signaling and decisiveness might operate in opposition to one another; however, under the assumption that policies are more sensitive to vote share in close elections than landslides, P1 holds: a voter observing a close poll sees that a vote for their preferred candidate has more impact on the desired candidate than does a distant poll.[9]

The leading alternative to instrumental voting models are ethical models. Starting with Riker and Ordeshook (1968), scholars argue that voters are motivated to turn out by a sense of duty, thus deriving utility from the act of turnout separate from the consequences of the vote. Later work sharpens this idea to consider utility derived from the joint event of turning out and voting for a particular candidate (Fiorina, 1976). P1 does not hold in such models as the election outcome, and hence the perceived closeness of the election, is unimportant.[10]

A richer view of ethical voting is developed in Feddersen and Sandroni (2006), where the force and direction of ethical motives depends on instrumental factors (i.e., the likelihood that the vote will affect the outcome). They posit that would-be voters follow a rule-utilitarian strategy, i.e., they vote under the hypothesis that all others sharing their ideology follow the same strategy. Ethical payoffs derive from adhering to this strategy, or not. This model predicts a tight relationship between the distribution of voters' preferences in society (a dis-

[8]This is because a close poll implies few A supporters are planning on voting, indicating that B should be preferred according to valence. The opposite would be true for a not-close poll. And so both valence and pivotality motives shift behavior in the same direction for B voters. More generally, as we discuss in Appendix D even if ideology does not dominate valence for all voters, we can restrict our analysis to individuals, whose preferences do not shift because of the poll results. These individuals then conform to the private values case discussed above. We examine additional predictions of this class of models in Section 6.3.

[9]Whether the conditions on the sensitivity of policy to vote share hold is, of course, debatable. Nonetheless, even when these conditions fail to hold, predictions can still be obtained, as described in Appendix D, and examined in Section 6.3.

[10]Some models (e.g., Morgan and Várdy, 2012) combine both motives. It may be readily seen that, in large elections, instrumental motives essentially vanish leading to the same prediction as when such motives are ruled out entirely.

tribution proxied for by polls) and the decision to turn out to vote. If an election is unlikely to be close, it would be wasteful for voters on the winning side to ask members of their group with high voting costs to turn out, so turnout is depressed. A close poll, on the other hand, suggests a need for large turnout among voters on a given side. Here, P1 should hold.

Recently, several "social" models have emerged to explain voting. Some studies (e.g., Gerber et al., 2008) emphasize the power of conformity. They hypothesize that individuals exposed to information about high turnout in their neighborhood will be more likely to turn out themselves. A separate strand (e.g., Harbaugh, 1996; DellaVigna et al., 2017) hypothesizes that voting occurs in anticipation of future interactions—if someone is likely to be asked whether they voted, they are more likely to vote. Such models are not directly concerned about the relationship between the perceived closeness of the election and turnout.[11]

As mentioned earlier, Shachar and Nalebuff (1999) posit a model based on elites where closeness affects the decision of individuals to vote, but via an indirect mechanism: closer elections encourage party leaders to exert effort to get their voters to turn out. Because our experiment only affects a very small subset of voters' perception of the closeness, we would expect this mechanism to predict a zero effect of our treatment on turnout.

Not only do different voting models make different qualitative predictions, but they also differ quantitatively, depending on various factors including the distributions of voting costs and voting benefits; beliefs about closeness; and any aggregate uncertainty. Appendix D.6 calibrates a very simple instrumental voting model, and we discuss it later in Section 6.

3 Methods and Data for 2010 Experiment

We conducted the experiment in states with gubernatorial races in 2010, a year where there was no presidential election. Our goal in doing this was to select highly visible elections

[11] Nonetheless, they could, in principle, rationalize outcomes consistent with P1. For instance, if exposure to a close poll leads an individual to believe she is more likely to be asked about her vote, then turnout should increase. But the reverse is also consistent with these models: An individual whose neighborhood is known to favor a given candidate might conclude that neighborhood turnout is high on seeing a distant poll result.

that would be salient to voters and avoid complications from the electoral college. Since US voters often vote on many races at one time, we wanted to choose elections that would be the most "top of mind" for voters. We avoided conducting our study with presidential elections as the electoral college makes the election differ substantially from basic theory. We chose a "midterm" (i.e., non-presidential) year to avoid having the governor races eclipsed by presidential elections. Political science research shows that governors are the second most recognized elected officials in the US (after the President), with substantially more visibility and media exposure than senators (Atkeson and Partin, 1995; Squire and Fastnow, 1994), suggesting that voters likely view gubernatorial races as significantly more important than senate races. For example, in Squire and Fastnow (1994), 79% of voters could recall their governor's name, compared to only 52% who could recall their senator's name.

The experiment was administered by Knowledge Networks, a large online survey company. The Knowledge Networks KnowledgePanel is a panel of individuals that agree to take several online surveys per month. Members are invited to join via random digit phone dialing. Members receive surveys by email and complete them over PC or WebTV.[12] Members receive various rewards and prizes for participating in surveys. Knowledge Networks collects demographics for all members, and the panel is designed to be roughly nationally representative of US adults along these characteristics (Liebman and Luttmer, 2015).

In choosing our sample of states, we excluded CO, MA, ME, MN, and RI, as these were states where there was a major third party candidate. In addition, we restricted our sample to states (1) where the was a poll within the last 30 days indicating a vote margin between the Democrat and Republican candidates of 6pp or less and (2) where there were two polls that differed between each other by 4pp or more. This left us with 13 states: CA, CT, FL, GA, IL, MD, NH, NY, OH, OR, PA, TX, and WI. In each state selected, we used KnowledgePanel members who were registered voters. From the KnowledgePanel registered voters in these states, we had 5,413 subjects assigned to Close Poll and 5,387 subjects assigned to Not Close

[12] For individuals without computer/WebTV or internet, Knowledge Networks provides access for free. The KnowledgePanel has also been used in leading economics research on unrelated topics (e.g., Fong and Luttmer, 2009; Liebman and Luttmer, 2015; Rabin and Weizsacker, 2009).

Poll (plus an additional 5,543 subjects assigned to receive nothing and not get surveyed). We used poll information from FiveThirtyEight.com and RealClearPolitics.com.

First Survey. Subjects were first sent the survey on Wednesday, October 20, 2010 (13 days before the election), and subjects could complete it up to midway through election day (Tuesday, Nov. 2). The order for the first survey was as follows (see Appendix Figure C1 for a visual timeline and see Appendix E.1 for screenshots with question wording):

1. The survey began with asking people whether they had already voted. Those who answered yes were removed from the survey.

2. Subjects answered three political knowledge and interest questions.

3. Subjects were asked for their prediction of the vote shares between Democrat and Republican.

4. Subjects were provided with a standard "explanation of probabilities" developed in the pioneering working of Charles Manski and used in Delavande and Manski (2010).

5. We then asked subjects about the chance that they would vote; their chance of voting for the different candidates; and the chance the election would be decided by less than 100 or 1,000 votes.[13] We decided to ask subjects about the event of the election being decided by less than 100 or 1,000 votes instead of the outright event of being decisive, as some political scientists and psychologists we spoke to believed that such questions would be easier for subjects to comprehend. In addition, as emphasized by Mulligan and Hunter (2003), vote totals within some range of an exact tie often trigger recounts in US elections; elections are then oftentimes decided by courts (e.g., recall the 2000 Presidential Election in Florida). Thus, having an election decided by less than 100 votes

[13]To avoid any issues of anchoring or voters trying to make their answers consistent across questions, voters were randomly assigned to be asked about *either* the chance the election would be decided by less than 100 *or* less than 1,000 votes.

may be roughly equivalent to a 1 in 100 chance of being pivotal. All belief questions were administered without any incentives for accuracy.[14]

6. We then provided the information treatment, described below.

7. Immediately after the information treatment, subjects were again asked their prediction of the Democrat/Republican vote share and the questions from #5 (in the same order). To ensure the treatment was strong, we continued to display the two poll numbers at the bottom of the screen as subjects re-answered questions.[15]

We decided to ask the same questions immediately after treatment so as to detect if there was any immediate impact on voting intentions, given the possibility (discussed further in Section 6.1) that belief impacts could conceivably attenuate between the survey and the date of the turnout decision. The median amount of time on the survey was 4 minutes (25th perc=3 mins, 75th perc=7 mins).

The survey had a 62% response rate, reflecting that some people invited to take the survey didn't take it. The rate was 62% both among those assigned to receive the Close Poll treatment (3,348 out of 5,413) and those assigned to receive the Not Close Poll treatment (3,357 out of 5,387). It is unsurprising that the treatment didn't affect the response rate because the treatment was only provided halfway through the survey. Given the paper's

[14] We decided not to use incentives for accuracy after a political scientist colleague informed us that doing so may be illegal, possibly constituting either gambling on elections or potentially even being a form of paying people to vote (for the question that asks people about their intended voting probability). Field experiments that have randomized incentives for accuracy often find little impact of using incentives on beliefs (Hoffman and Burks, 2017). Especially given the wide range of backgrounds, ages, and education levels in our sample, we suspect that adding financial incentives for accuracy via a quadratic scoring rule would not have reduced elicitation error (and might have even increased it). While most of our variables are binary, for the continuous variable of predicted vote margin, we did not elicit subject's uncertainty (see Kendall et al. (2015) for an example that does), doing this for simplicity and time/financial constraints from the survey company.

[15] Although it is quite common in information provision field experiments (e.g., Armantier et al., 2016; Armona et al., 2016), one potential concern with asking questions twice (and doing so while continuing to display poll numbers) is that it could lead to potential "Hawthorne Effects," e.g., where subjects feel pressure from the experimenters to update their beliefs based on the information provided. We take comfort from the fact that, as we document later, beyond updating on expected vote margin, subjects update on the probabilities of less than 100 or 1,000 votes, on which no direct information was provided. Moreover, our conclusions about closeness and turnout are unchanged if we restrict attention only to measuring beliefs using the less than 100 or 1,000 vote belief measures (instead of predicted margin).

focus on beliefs about electoral closeness, we perform our analyses restricting to these 6,705 individuals who did the survey, as belief data are only observed for those taking the survey.

Selection of Polls and Information Treatment. Poll choices were finalized on October 17, 2010. To select the polls, we identified the poll during the 40 days prior to the start of the experiment (which started October 20) with the greatest margin between the Democrat and Republican candidates. This served as our not-close poll. We then selected the poll that was most close, conditional on the same candidates being ahead and behind. If two polls were tied for being least close or most close, we selected the poll that was most recent. In the experiment, the language we used to present the poll was as follows:

Below are the results of a 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:
Jerry Brown (Democrat): 50%
Meg Whitman (Republican): 50%[16]

Appendix Table C2 lists the poll numbers we provided. Across the 13 states, the average margin of victory was 2.3% in the close polls and was 16.3% in the not close polls. For simplicity, subjects were not informed about the number of people in our study, but subjects likely understood that our sample size was small relative to the population because it consisted of people from the KnowledgePanel. On the Friday before the election, subjects who had already done the survey were sent a brief email reminding them of the poll numbers they saw (see Appendix E.2 for wording). Of those emailed, 3,900 people (or 62%) opened the email.[17]

[16]Poll numbers were calculated using the share of poll respondents favoring the Democratic (Republican, respectively) candidate out of the total respondent favoring either the Democratic or Republican candidate (and rounded to the nearest whole number). Our goal in doing this was to avoid having different interpretations of undecided voter shares represent a confound for our analysis, as well as create an experimental environment that best corresponded to the simple environment in theory models.

[17]The number of people opening the email each day was: 1,558 (Fri), 1,443 (Sat), 418 (Sun), 404 (Mon), and 97 (Tue, as of 12pm PST). A small share of people did the pre-election survey between Friday and Tuesday, and they were not sent a reminder email, as a reminder would be unnecessary for them given they received the poll quite close to election day.

Post-election Survey and Voting Data. The post-election survey was sent out on November 19, 2010, 17 days after the election, and subjects completed the survey until November 30, 2010. Subjects first completed a simple laboratory task designed to measure a possible bias in probabilistic thinking. We then asked subjects whether they voted and whom they voted for, among a few other questions (screenshots in Appendix E.3).

The laboratory task is taken from Benjamin et al. (2013), which is based on Kahneman and Tversky (1972). The task measures the extent of subjects displaying non-Bayesian beliefs, specifically, "non-belief in the law of large numbers" (abbreviated NBLLN). Subjects were asked the following question: "Imagine you had a fair coin that was flipped 1,000 times. What do you think is the percent chance that you would get the following number of heads." Subjects typed in a number corresponding to a percentage in each of the following bins: 0-200 heads, 201-400 heads, 401-480 heads, 481-519 heads, 520-599 heads, 600-799 heads, 800-1,000 heads. Our intent in asking this question was that NBLLN could potentially help rationalize turnout by explaining why individuals have excessive probabilities regarding a close election. Appendix A.2 discusses how person-level correlations between NBLLN and perceived closeness of an election support that our belief data are sensible.

We obtained administrative voting data on the voters in the sample for the last 10 years. Specifically, we worked with a "vote validation firm" that collects administrative records on whether people voted from the Secretaries of State in different US states.

Randomization and Summary Statistics. Randomization was carried out by Knowledge Networks by sorting individuals by several characteristics (state, education, self-reported voting in 2008, gender, race, age, and a random number), thereby stratifying by these characteristics. Details are given in Appendix B.1.

The goal of the 2010 experiment is to examine how voter beliefs affect turnout. Thus, the main individuals of interest are people who were assigned to the close poll or not-close poll groups and who responded to the survey. Table 1 shows that across most variables, respondents from the Close Poll group and Not Close Poll group have similar characteristics.

There is only one characteristic which differs across the two groups at the 5% level. Specifically, voters in the not-close group had a slightly higher pre-treatment belief that the election would be decided by less than 100 votes (but not for less than 1,000 votes or Predicted Margin). To address this imbalance, we will often control for the pre-treatment belief about less than 100 votes.

Even though we are using an online survey, the sample is broadly diverse both demographically and ideologically. The sample is 61% female, is 53 years old on average, and has a significant share with a master's or PhD degree. Appendix Table C3 gives summary statistics, including on outcome variables. The voting rate based on administrative data is 72% (71.9% for close poll, 72.1% for not close poll), which is sizably lower than the post-election self-reported voting rate of 84%. Such misreporting of turnout is present in many studies (e.g., DellaVigna et al., 2017) and highlights the importance of having administrative turnout data. Because of this, we do not use the self-reported information on whether someone voted.

4 Experimental Results for 2010 Experiment

4.1 Beliefs about whether the Election will be Close

Figure 1 shows subjects' pre-treatment predictions about the margin of victory, both overall and state by state. People tend to believe in closer margins of victory in states that end up being closer, a correlation we confirm with controls in Appendix Table C4.

Figure 2 shows subjects' subjective probabilities that the election is decided by less than 100 or less than 1,000 votes. There is a large amount of mass at 0%, 1%, or 2%, with many voters predicting that a very close election is unlikely. However, there is also a large mass of voters who are not 2% or less. As in many studies of subjective beliefs (e.g., Zafar, 2011), there is significant bunching at "round numbers" such as 10%, 20%, and 50%. The median belief for less than 100 votes is 10% and the median for less than 1,000 votes is 20%., i.e., most voters overpredict the probability of a very close election.

How do we know that this is an overestimation? The simplest evidence is to look at history. In the last six decades, there have been very few gubernatorial general elections decided by less than 100 or 1,000 votes: during 1950-2009, there were nine races decided by less than 1,000 votes (RI in 1956; VT in 1958; ME, MN, and RI in 1962; ME in 1970; AK in 1974; AK in 1994; and WA in 2004) and only one race decided by less than 100 votes (MN in 1962). In 835 contested gubernatorial general elections since 1950, the shares with margins less than 1,000 and 100 votes were about 1% and 0.1%, respectively (and 0.6% and 0% after 1970). Appendix B.3 gives further details on these calculations.

Alternatively, individuals might rely on models of voting to assess the chance that the election will be close. For example, suppose individuals have a simple model of voting where election outcomes are binomially distributed with a rate equal to the actual election outcome proportion and the number of draws equal to the number of voters. Stated beliefs would be an over-estimate in such a model. Even with the smallest electorate (New Hampshire, where roughly 450,000 votes were cast) the ratio of support between the candidates would have needed to be between 0.9934 to 1.0066 to generate even a 1% of the election being decided by less than 100 votes (0.9887 to 1.022 when considering less than 1,000 votes). This excludes not only the actual New Hampshire ratio (1.17), but also all realized ratios in our data (the ratio closest to 1 occurred in Oregon, where it was 1.03).

One reaction to Figure 2 is that many voters do not have advanced education and may not fully understand probabilities. To address this, Appendix Figure C2 restricts to the roughly 1,400 voters with Master's or PhD degrees. Even among these well-educated voters, the median perceived probabilities of less than 100 and less than 1,000 votes were 5% and 10%, respectively. Thus, the median belief is smaller among well-educated voters, but still quite high.

While pre-treatment closeness beliefs are very high, they seem sensible in several ways. First, Appendix Table C4 shows that the actual *ex post* vote margin in a state is a positive predictor of perceived vote margin, as well as a negative predictor of the perceived probability of a very close race (i.e., less than 100 or 1,000 votes). Second, this finding is consistent with Duffy and Tavits (2008), who find that students substantially overestimate the probability of being pivotal in 10-voter lab elections. Third, as we discuss in Section 6.1 and Appendix A.2, observed beliefs are consistent with other data and models in economics where subjects consistently overestimate small probability events.

Moreover, our identification strategy is driven by changes in individual beliefs, not the level. Thus, although individuals' beliefs may be off in terms of the level, so long as the close poll and not close poll differentially affect beliefs, we have the necessary experimental variation. As the next sub-section shows, our treatment leads to differential updating.

4.2 Belief Updating in Response to Polls

Table 2 provides non-parametric evidence that voters update in response to the experimental poll information. It tabulates whether voters increase, decrease, or did not change their beliefs, showing impacts on predicted vote margin, probability decided by less than 100 votes, and probability decided by less than 1,000 votes. The poll information was given to them in terms of vote margin, so it is perhaps unsurprising that voters would update on this metric. But there is also clear updating on the less than 100 or 1,000 vote margins, even though they were not directly manipulated by our experiment. Consider, for example, the probability the election would be decided by less than 1,000 votes. About two-thirds of voters are not changing their beliefs at all, a percentage which is in-line with other information field experiments (e.g., Armantier et al., 2016; Armona et al., 2016). However, for the share that do change, far more do so in the expected direction. Thus, despite being off by orders of magnitude, beliefs appear to incorporate information, much like a pure Bayesian.

Tables 3 and 4 confirm the same results using a regression. We regress post-treatment beliefs about the closeness of the election on the randomized treatment status and controls. Tables 3 uses predicted vote margin as the outcome variable, whereas Table 4 analyzes the perceived probability of an election being decided by less than 100 or less than 1,000 votes.

Table 3 shows that receiving the close treatment leads the average voter to decrease their predicted vote margin by about 2.8pp, which represents a very sizable 28% decrease in predicted margin relative to the pre-treatment median (or 16% relative to the mean). In addition, consistent with theory, we see that voters who are less informed update more. We measure how informed voters are using their self-expressed interest in politics (1-5 scale), whether they could correctly identify Nancy Pelosi as the Speaker of the House, and the share of the time they voted in the previous 5 elections. For example, a voter who identifies as having very low interest in politics updates by 4.7pp, whereas a voter with a very high interest in politics updates by only 1.8pp.[18]

Table 4 shows that receiving the close poll treatment increased the perceived probability that the vote margin is less than 100 or 1,000 votes. Both probabilities increased by about 2.5pp after receiving the close poll treatment. Column 1 shows an insignificant effect because, as discussed earlier in Table 1, people randomly assigned to the Not Close Poll group happened to have higher initial beliefs about the margin less than 100 votes. However, results become stronger once one controls for pre-treatment beliefs.[19] For the subjective probability of less than 100 votes, the coefficient in column 3 represents roughly a 25% increase in the believed probability relative to the pre-treatment median (or about 10% relative to the mean). For the subjective probability of less than 1,000 votes, the coefficient in column 6 represents roughly a 12% increase in the believed probability relative to the pre-treatment median (or about 7% relative to the mean). Thus, these represent quite sizable impacts on beliefs.

Appendix Table C7 shows even larger impacts on beliefs when restricting to individuals who update their beliefs at all in either direction.

Figure 3 graphs the average reaction of beliefs to our treatments. Appendix Figure C3

[18]In Appendix Table C5, we repeat the analysis using a continuous version of the treatment, namely, the vote margin in the randomly shown poll. Column 1 has a coefficient of 0.42, whereas once controls are added in column 2, the coefficient shrinks to 0.22. This occurs because states with actual wider vote margins tend to have polls with wider vote margins. Even though our treatment is randomly assigned within the state, the level of the poll vote margins is not randomly assigned across states.

[19]Repeating the analysis using the continuous treatment (vote margin in the poll) instead of the close poll dummy, Appendix Table C6 shows that each additional 1pp drop in the margin in the randomly assigned poll led to a 0.14pp increase in the probability of less than 100 or 1,000 votes.

graphs how the treatments affect the distribution of beliefs.

4.3 Electoral Closeness Beliefs and Voting

In our empirical analysis of voter turnout, we usually present results controlling for an individual's past voting history. As argued by McKenzie (2012), when an outcome variable is highly persistent (such as voting, where some people always vote and others never vote), there are often significant gains in statistical power by controlling for pre-treatments records of the outcome variable.[20]

Believed Closeness and Turnout, OLS. Table 5 performs OLS regressions of turnout on different measures of beliefs about the closeness of the election. Columns 1-3 study margin of victory, columns 4-6 study perceived probability of less than 100 votes, columns 7-9 study perceived probability of less than 1,000 votes, and columns 10-12 study the perceived probability of less than 100 or less than 1,000 votes. The coefficients are multiplied by 100 for ease of readability, as they are throughout the paper when the outcome is whether someone voted. We see little relationship between closeness beliefs and turnout. Column 1 implies that a 5pp decrease in predicted margin of victory is associated with an increase in turnout of 0.15pp.

To get a better sense of magnitudes and to see whether standard predictors of turnout are operative in our setting, Appendix Table C9 shows a regression of turnout on demographic characteristics in detail. We focus primarily on column 1 of Table C9, which shows results without past voting controls. Consistent with the past literature, older, more educated, and richer people are more likely to vote. Although our sample is not a random sample from the US population, these basic voting trends suggest that our sample is not especially atypical. Furthermore, the estimated coefficients are much larger than the closeness coefficients estimated in Table 5. For example, all else equal, being aged 75+ is associated with being 43pp

[20] In Appendix Table C10, we present our main IV results without controlling for past voting history, and obtain the same conclusions (though with less precise standard errors). The past voting variables mostly reflect whether someone chose whether to vote, but there is a small share of individuals in the data who were too young to be eligible to vote in past elections (see Appendix B.1 for details).

more likely to vote (relative to being under 25), and having household income over $100,000 is associated with being 15pp more likely to vote (relative to having household income under $25,000). The coefficient on being aged 75+ is roughly 200 times larger than that associated with a 5pp decrease in predicted margin of victory.[21]

Beyond the factors already discussed in the Introduction, the OLS results may be biased for multiple additional reasons.[22] First, measurement error in beliefs could attenuate results toward zero. Second, causation could run in the opposite direction, e.g., people who intend to vote may develop self-serving beliefs, justifying their intention to vote by coming to believe (or reporting) that the election is close. Third, there could be additional unobserved factors affecting both beliefs and turnout.

Believed Closeness and Turnout, IV. Table 6 shows IV regressions of turnout on beliefs instrumenting with our experiment (the dummy for whether the recipient received the close poll or not), showing that exogenously affected beliefs do not affect turnout. We estimate by 2SLS. In column 1, the coefficient of -0.12 means that for every 1pp decrease in the believed vote margin (i.e., the election becomes more close), turnout increases by 0.12pp. The F-stat on the excluded instrument is high, significantly above the rule-of-thumb of 10 often used to designate weak instruments (Stock et al., 2002). Table 6 also presents the exact first stage results in the final row.[23]

Columns 4-6 study the perceived probability of the margin of victory being less than 100 votes. In column 4, the F-stat on the excluded instruments is less than 1—this reflects the earlier discussed initial imbalance between the Close and Not Close groups in terms of initial

[21] The coefficient on margin of victory in column 1 of Appendix Table C9 is ≈ 0.04, and $\frac{43}{5 \cdot 04}$ is over 200.

[22] Papers in the literature often regress turnout on *ex post* closeness across elections (in our case, an election is a state). In contrast, in the OLS results here, we regress individual turnout on individual-level believed closeness while controlling for state fixed effects. However, it seems likely that many of the influences mentioned in the Introduction (e.g., media, social pressure, campaigning) could still bias OLS estimates conditional on state fixed effects. Suppose that a person has friends who are pressuring them to vote. This might make them more likely to vote, as well as more likely to believe the election is close (compared to someone whose friends are not pressuring them). Despite possible bias, we still believe, though, that it is of some interest to know the correlation between individual level beliefs and turnout (as opposed to overall closeness and turnout as in the literature).

[23] These results are slightly different from those in Table 3-4 because we include past voting controls. For reduced form results, see Appendix Table C24.

perceived probability of less than 100 votes. In column 5, we control for pre-treatment beliefs and the instrument becomes strong again. In column 6, when full controls are added, the coefficient is -0.19, meaning that a 1pp increase in the perceived chance of a very close election (margin < 100) actually slightly *decreases* voting (though it is not statistically significant).

Among columns 4-12, we have the most power in column 12. There the coefficient is 0.08 (se=0.33), leading to a 95% CI of [-0.58, 0.73]. The point estimate of 0.08 means that 5pp increase in the perceived probability of a very close election increases turnout by only 0.4pp. The 0.73 upper limit of the 95% CI means we can rule out that a 5pp increase in the perceived probability of a very close election would increase turnout by more than about 3.6pp. When considering models of instrumental voting, we might expect that the probability of an election being decided by less than 100 or 1,000 votes proxies for pivotality much more tightly than predicted margin of victory, so the statistical zero in column 12 is more noteworthy.

We can thus rule out that a 5pp decrease in perceived margin of victory or 5pp increase in the perceived probability of a very close election is anywhere near as important as that of other voting predictors like age, education, and income, where we saw relations on the order of 10-40pp. Even though the IV estimates have standard errors that are roughly 10 times larger (or more) than those from OLS, our estimated "zeros" still have a reasonable amount of precision.[24] Section 6.2 returns to the question of precision for our 2010 study.

One seemingly non-standard feature of Table 6 is that we use the same instrumental variable to instrument different closeness variables one at a time. Our view is that the different closeness variables likely represent different forms or constructs of a person's underlying perception of election closeness. To the extent that they represent different underlying constructs, Appendix A.1 shows that any resulting inconsistency in the IV estimates is in the direction away from 0, making the true impact of each closeness variable an even tighter zero than the one we estimate.

[24] It is hard to know the exact source of the difference between our OLS and IV estimates, but we suspect that measurement error in the OLS is quite important. The reader should also recall that the IV results reflect the treatment effect among compliers, in our case, individuals who would update their beliefs in response to close polls. We do not have strong priors as to whether treatment effects among compliers would differ from those among the general population.

Actual Closeness and Turnout. Having examined the relation between electoral closeness beliefs and turnout, we now attempt to "replicate" the past literature on actual closeness and turnout using our 2010 data. Appendix Table C8 regresses turnout and the actual margin in an election using our 2010 data. In keeping with a lot of the literature on closeness and turnout, columns 1-2 collapse the data by election (i.e., by state) and present election-level regressions. Columns 3-5 do individual level regressions. In column 3, a 10pp decrease in the vote margin is associated with 2.6pp higher turnout. This relation decreases in size when controls are added for a voter's past turnout decisions—while the column 5 coefficient is statistically significant when clustered by state, it is insignificant according to a block bootstrap or wild bootstrap p-value (13 clusters). While the strength of inference varies depending on the method of clustering, the 2010 experimental data provides suggestive evidence supporting a correlation between actual closeness and turnout. In contrast, it provides no evidence for a causal relation between perceived closeness and turnout.

5 Follow-up Experiment in 2014

Our 2010 experiment shows that changes in the perceived closeness of an election do not affect turnout. But it leaves some questions unanswered. First, while the first experiment established that the effect of beliefs on turnout is small (if any), there remains an open question whether the effect is small or approximately zero. Second, the population we chose is a population of online survey-takers. Although they have a broad range of demographics and have been used in leading economic research, there is a question of whether they could respond differently than a fully representative population (including people who are not willing to do online surveys). Third, although we worked hard to provide the information in a simple way, a skeptical reader could argue that providing a poll-based predicted margin could be nonideal when some people polled are undecided about for whom to vote. Could other types of information about closeness (such as information about the expected size of the electorate) matter?

We did a second large-scale experiment during the 2014 gubernatorial elections that enables us to answer these three questions. First, we treated roughly 125,000 voters (instead of the roughly 6,700 voters from before), increasing our sample size by a factor of roughly 20, thereby allowing us to see whether the effect is small or approximately zero. Second, we draw on the population of registered US voters, as opposed to online survey-takers, allowing us to see whether our results hold with a fully representative population. Third, in addition to providing the close vs. not close polls to treated individuals, we also crossed this with a high or low electorate size prediction treatment. The number of voters is a common regressor in empirical studies of turnout (Coate and Conlin, 2004; Coate et al., 2008; Hansen et al., 1987; Shachar and Nalebuff, 1999). Using predicted number of voters provides another way of communicating information about an individual voter's chance of being decisive, but one that does not involve vote margins.

Set-up. The set-up for the 2014 experiment was very similar to the 2010 experiment, with the main exception being that we conducted the experiment using postcards instead of an online survey. As in 2010, there was no presidential race, and we focused on states with gubernatorial races. As in 2010, we restrict to states with gubernatorial elections, excluding states with a major thirty party candidate. In addition, we restricted our sample to states (i) where there was a poll within the last 30 days indicating a vote margin between the Democrat and Republican candidates of 6pp or less and (ii) where there were two polls that differed between each other by 4pp or more.[25] We obtained poll information from RealClearPolitics.com. We also limited ourselves to states where we had access to the voter file from the Secretary of State. This left us with 7 states: AR, FL, GA, KS, MA, MI, and WI.

In the 2010 experiment, the main treatment was Close Poll vs. Not Close Poll. However, there are other potential aspects of the election that affect one's chance of being decisive besides the margin. Thus, we cross information about the closeness of the poll with information about the predicted electorate size from election experts.[26] We implemented a 2x2 design of

[25] The postcards were finalized on October 18, 2014, so it had to have been within 30 days of that date.

[26] We sent emails to 15 election experts, and asked them to predict turnout in each state. Seven election

Close Poll vs. Not Close Poll, and Large Electorate vs. Small Electorate. In each state, we randomly selected roughly 11,500 households to receive a mailing, equally allocated among the 4 combinations (Close Poll&Large Electorate, Close Poll&Small Electorate, Not Close Poll&Large Electorate, Not Close Poll&Small Electorate). In addition, in each state, among households not receiving a postcard, we randomly selected roughly 115,000 households (10x number of treated households) to obtain their voting records and serve as a control group.

The postcard's wording was very similar to the 2010 online survey, with a few exceptions (the postcard is shown in Appendix E.4). First, to make the postcard look like a "regular" sort of election material, we added short standard voting participation messages to the top and bottom of the postcard (Gerber and Green, 2016).[27] Second, we added the source of the poll for the polls, with the idea that when someone receives something in the mail, it would add credibility to see the source of the poll. Third, out of an abundance of caution, we added a sentence for respondents to recognize that the information we are sending them is only from one poll. While we can't observe beliefs changes directly for the 2014 study, our goal in making the 2014 wording very similar to the 2010 wording is to ensure that the 2014 experiment would also have sizable effects on beliefs. Furthermore, while we cannot observe whether people read the postcard, a large literature (discussed further below when we discuss magnitudes) finds that mailers (including postcards) tend to significantly boost turnout, sometimes by as much as 5-8pp (Gerber and Green, 2016). Such effects are clearly impossible if people don't read postcards. So long as our experiment is similar to postcards in past experiments (and we designed the postcard to be similar), we would expect substantial readership of our postcard.

The randomization was conducted using voter file records provided by an anonymous vote validation company.[28] We stratified by state, and attempted to stratify by whether

experts responded.

[27]These were "THE ELECTION ON NOVEMBER 4 IS COMING UP" at the top and "We hope you decide to participate and vote this November!" at the bottom. Although such standard language may make individuals receiving a mailing more likely to vote relative to those who received no mailing, it seems unlikely that it would interact with our treatment of interest: receiving a mailer showing a close poll relative to a mailer showing a not close poll.

[28]The vote validation company used a number of sample restrictions to create voter file lists for our experiments, most notably that an individual had not yet requested a ballot or voted as of Oct 16-17, 2014, when the voter file lists were extracted (see Appendix B.2 for details).

someone voted in the general elections of 2008, 2010, and 2012.[29] We restricted attention to households with three or fewer registered voters (to increase the chance that most voters would see or hear about the postcard), as well as to households with a valid address. For each household, we randomly selected one registered voter to have their name listed on the postcard. As is common in the literature, we consider all registered voters within a household to be treated, and our unit of analysis is a person. Our results are qualitatively similar if we restrict attention to individuals to whom the postcard is addressed (Appendix Table C23).

The postcards were mailed out on Friday, October 24. The bulk of the mail would have arrived on Monday (10/27) and Tuesday (10/28), and nearly all of the mail would have arrived no later than the Wednesday (10/29) before the election. This accomplished our goal of making the postcards arrive very close to the election while still having all postcards arrive before the election.

Appendix Tables C11 and C12 show that the samples are well-balanced in terms of observables, indicating a successful randomization. In our sample, the average margin of victory was 0.3% in the close polls and was 7.7% in the not close polls, and the "large electorate" electoral size prediction was 25% larger on average than the "small electorate" prediction (see Appendix Table C2).

The 2014 voting rate among people getting close poll postcards was 53.8%, and was 53.5% among people getting not close poll postcards. The 2014 voting rates were 53.5% and 53.7% for smaller electorate postcards and larger electorate postcards, respectively. These voting rates are substantially lower than the voting rate of 72% in our 2010 experiment. This likely reflects several factors, including that people in our 2010 internet sample may have a relatively high rate of voting, that the 2014 elections had historically low turnout, and that

[29] Given the possibility of vote or not in three elections, this means there are 8 strata based on past votes per state. In our data, there are several possible codings of the means by which someone voted. In doing the stratification for the randomization, a research assistant coded the string "unknown" as corresponding to a person not voting as opposed to a person voting but whose means of voting was unknown. Thus, the actual stratification is not exactly based on whether someone voted in the past, but on whether someone voted in the past where the means of voting (such as early, polls, etc.) is known. About one quarter of people's voting record fields in 2008, 2010, and 2012 have the "unknown" string. However, the treatment groups are still very well-balanced in terms of past voting rates. In our regressions, we control for dummies for turnout in 2008, 2010, and 2012, as well as dummies for the randomization strata.

we have different states across the two years.[30]

Results. Table 7 regresses turnout on treatment dummies. We do this (instead of an IV regression of turnout on beliefs instrumenting with treatment dummies as in the 2010 results) because beliefs are not measured in the 2014 experiment. Column 1 indicates that the close poll appeared to increase turnout by about 0.29pp relative to the not-close poll, but this is not statistically significant. Results are similar with further controls.

Column 3 compares the close and not-close poll treatments relative to getting no poll. Relative to the control, the close poll increased turnout by 0.34pp, which is marginally significantly different from 0. However, the not-close poll also increased turnout by 0.05pp. In our view, the main statistical comparison of interest is not whether the close poll affected turnout relative to the control, but rather whether it increased it relative to the not-close poll, as simply getting a postcard related to the election could lead someone to be more likely to vote. An F-test fails to reject that the treatment effects from the close and not-close polls are the same.

Column 4 further adds the cross treatment on whether the number of voters was expected to be small (or large). Information that the electorate is likely to be smaller decreased turnout by 0.17pp. This is in the opposite direction of what would be predicted by instrumental voting models (where smaller electorate means higher chance of being decisive), but it is not statistically significant. We can rule out with 95% confidence that our small electorate treatment would increase turnout by more than 0.31pp.

How tightly estimated is our "zero result" from the close poll treatment compared to the not close poll treatment? Based on the 95% confidence interval in column 4, we can rule out a treatment effect of more than 0.77pp. This is small relative to many (but not all) non-partisan interventions in the turnout literature (Gerber and Green, 2016). For example, Gerber et al. (2008) show that two mailer social pressure treatments increase turnout by 5-

[30]To investigate this further, we can exploit the fact that we have 3 states in both the 2010 and 2014 datasets: FL, GA, and WI. In the 2010 elections in these three states, the 2010 sample had a voting rate of 71%, whereas the 2014 sample had a voting rate of 52% (conditioning on being age 22 or older in 2014). This suggests that most of the difference is likely due to the internet sample having a relatively high rate of voting.

8pp. Experiments with one get-out-the-vote postcard on Asian-Americans (Wong, 2005) and Indian-Americans (Trivedi, 2005) increased turnout by 1.3pp and 1.1pp, respectively. Gerber and Green (2000) do an experiment where voters got 0-3 mailings with different messages, showing that each additional mailing increases turnout by 0.6pp. Surveying the literature on mail experiments, Gerber and Green (2016) report that "Pooling all mail studies together shows that sending a piece of mail to a voter increases the subject's turnout by about 3/4 of a percentage point"; thus, our 95% confidence interval means we can roughly rule out the average size effect of a mailing.[31]

We can also compare the precision of the 2014 results vs. the 2010 results. To do this, we need the 2010 results in an analogous form. Appendix Table C24 shows analogous reduced form regressions using the 2010 data. The standard errors are 3.25 times smaller for 2014, which is unsurprising given that the 2014 sample size is many times larger. Further, the 2010 and 2014 reduced form point estimates are similar (≈ 0.2 for 2010 vs. ≈ 0.3 for 2014).

6 Robustness: Alternative Explanations and Additional Investigations

6.1 Alternative Explanations

Could the intervention have been "washed away"? One alternative explanation for our zero impact on turnout result is that while the experiment may have affected voting tendencies, other events may have occurred in the several days before the actual election that would have over-ridden our impact on beliefs. Individuals could have forgotten the polls we

[31] While this summary statement was about all mailers (postcards and non-postcards), it seems that us that postcards would be likely to be at least as effective as other mailers, given that they don't need to be opened. The 95% CI top-end of 0.77pp is based on comparing a close poll postcard vs. a not close poll postcard. For comparing with the literature, we can also consider the impact of a close poll postcard vs. nothing. Based on column 3, we can rule out that getting a close poll postcard increases turnout by more than 0.69pp relative to nothing, which is less than the 0.75pp literature rough benchmark. The pooling of studies by Gerber and Green (2016) include a significant number of partisan mailer studies, which generally have a negligible effect of turnout (Gerber and Green, 2016)—without these studies, the average effect would be higher than 0.75pp.

gave them. Or, individuals who saw a poll could have taken the time to look up other polls or could have been exposed to other polling information in the media. What presumably matters in the theory is voter beliefs at the time of the actual turnout decision (for many potential voters, on election day), not voter beliefs at the time of the experiment.

We were quite conscious of this potential concern in designing our experiment. We tried to provide the polling information as close to the election as possible, while still providing time logistically for the information to arrive. To deal with subjects forgetting the polls, we sent them a follow-up email reminding them about the polls we showed them (and 62% of people opened the email we sent them). But that still doesn't fully address potential concern regarding washing away.

To assess this concern quantitatively, the 2010 experiment asked respondents about their intended probability of turning out three screens after providing the polls (Appendix E shows screenshots). In this very short span of time between initial information provision and post-treatment voting intention, it is very unlikely for additional information to have leaked in.[32] Relative to the not close poll treatment, the close poll treatment had no impact on post-treatment intended probability of voting (see Appendix Table C13). These IV results are even more precise than the IV results on administrative voting, with standard errors roughly one-quarter smaller. For example, in column 6, with the regressor of post-treatment belief about less than 100 votes, the 95% CI is [-1.07, 0.24]. The 0.24 upper limit means we can rule out that a 5pp increase in the perceived probability of a less than 100 vote margin would increase intended turnout probabilities by more than about 1.2pp.

While intended probability of voting is clearly conceptually different from actual administratively recorded voting, the two are quite correlated, with a correlation coefficient of 0.46. The average post-treatment intended probability of voting is 87.8% for the close poll group and 88.0% for the not close poll group. The 0 effect in Appendix Table C13 is consistent with the simple non-parametric evidence in Table 2, which shows that 88% of voters do not change

[32]Subjects did the 2010 experiment at home and it is theoretically possible they could have gone looking online at other polls immediately after seeing the polls we provided. However, we can see from the screen time data that few people lingered excessively after seeing the polls we provided, so it is unlikely that this occurred.

their intended probability of voting after receiving poll information.[33]

Another way to shed light on the "washing away" concern is to examine whether the treatment affected information acquisition, e.g., to start following all the polls and to more closely follow the election in general. Such voters might have discovered that we provided them with the most close recent poll, and might discard the information content once they learn this. However, 80% of people in the post-election survey said their attention to the campaigns didn't change after taking our pre-election survey, and the close poll treatment had no impact on self-reported tendency to pay greater attention relative to the not close treatment (Appendix Table C14). One concern might be that people would naturally start to pay more attention to the race in the last week or so after taking our survey, but only 12% of people said that they started to pay more attention to the campaigns after taking our survey.

Further, because people took the 2010 survey on different days, we repeated our 2010 results weighting by the day of the survey, with the idea that any washing away of beliefs would be lessened for those taking the survey last. Recall that survey responses began on Wed., Oct. 20, 2010 (day 1) and continued through Tue., Nov. 2, 2010 (day 14). The average day of survey response was day 3 (Fri., Oct. 22), and the standard deviation is 3 days. Weighting by day of survey response, the results are qualitatively similar (Appendix Table C15), though we caveat here by noting that day of survey response is chosen by respondents.

Last, while we could not measure beliefs a second time before the election, when information field experiments on unrelated topics have measured beliefs on multiple occasions, they tend to find that experimentally-induced belief changes are quite persistent, on the order of 50-90% two months after a one-time treatment.[34] Especially given that we sent an email reminding voters of the poll information that we provided them, we hypothesize that most of

[33]Given that intended probability of voting involves intention instead of actual behavior, some readers could be concerned about Hawthorne Effects. However, it seems to us that if Hawthorne Effects were biasing our result on intended probability of voting, they would bias toward finding a positive effect of closeness on turnout instead of a zero.

[34]In Armona et al. (2016), two months after providing information on past house price changes, the main coefficient for 1-year price updating is about 3/4 as large as it was immediately after treatment (columns 1-2 of row 5 in Table 8). In Bleemer and Zafar (2015), two months after providing information on returns to college, 88% of the treatment impact on college attendance expectations persists. In Cavallo et al. (Forthcoming), about half the impact of past inflation data on inflation expectations persists two months after treatment.

our experimentally-induced belief changes would have persisted until the time that subjects decided whether to vote.

Is observed belief updating genuine? A potential concern for the 2010 experiment is that voters do not actually update their beliefs at all, but rather appear to change their beliefs as a result of a Hawthorne Effect, i.e., changing their stated (but not true) beliefs to please the experimenter. While we cannot fully rule out this possibility, we provide a couple reasons why we believe it to be unlikely to explain our results. First, as discussed previously, voters update strongly both on the margin of victory (on which they were provided information) and the probability of a very close election (on which they were not provided information). If voters were simply telling the researchers what "they wanted to hear," it is not clear that they would update on both. Second, as noted earlier in Section 4.2, the amount of updating is negatively correlated with political interest and information, i.e., less informed / politically interested people update more. A pure Hawthorne effect seems unlikely to deliver this result (unless, of course, for some reason the people who are less informed, controlling for observable characteristics, are also the ones who are more prone to Hawthorne effects).

That people update closeness beliefs in response to one poll may be surprising to some social scientists who eagerly follow election polls online and may be very familiar with what polls have been taken. However, our results are consistent with evidence in political science that many voters are relatively unsophisticated and uninformed (Carpini and Keeter, 1997; Fowler, 2016). Indeed, the correlation across all voters between the pre-treatment predicted Democrat vote share and actual Democrat vote share is only 0.37 (though this is still highly statistically significantly different from 0).[35] Of course, we do not claim that all of our sample was uninformed (and indeed, as shown earlier in Table 2, a significant share of voters in the 2010 study do not update at all); rather, the evidence is consistent with many voters not being informed about polling.

Do the Zero Results Mask Important Heterogeneity? While we have presented

[35] The correlation between post-treatment predicted Dem vote share and actual Dem vote share is 0.42.

our results as being indicative of no effect on turnout, it is possible that significant effects could be observed for some types of voters. For example, there is a significant share of individuals in our data who are always observed as having voted in past elections. Such individuals may vote out of duty, habit, or other forces that make them much less susceptible to how close the election is. However, as seen in Appendix Tables C16 (2010 experiment) and C17 (2014 experiment), our zero result is robust to restricting to individuals who don't always vote.[36]

Given that many people do not update beliefs (and that our experiment involves exogenously "shocking" beliefs), one question is whether our results are robust to restricting to cases where a person updates their beliefs. Appendix Table C18 shows our results are robust to this restriction.

Turning to a different issue, one may believe that closeness considerations would be most important for "partisan" or "ideological" voters, as such people may be thought closer to voters in a private values instrumental framework. However, Appendix Table C19 shows no impact of beliefs on turnout when restricting to voters who rate themselves as having strong political ideologies.[37]

Another important possible source of heterogeneity is the size of the election. Closeness considerations may be thought to be more important in smaller elections. While all the elections we study are quite large (compared to, say, the vote of a business committee), we can restrict attention to the elections in our sample with smaller electorates. Dropping the "larger elections" in our sample (defined here as the ones with above median electorate size in our samples), we find little evidence that closeness considerations affect turnout, as seen in Appendix Tables C20 (2010 experiment) and C21 (2014 experiment).

Are Belief Levels "Sensible"? A contribution of the paper is to document that sub-

[36]If we additionally drop people who never vote, there is also no relation between closeness and turnout (though standard errors become larger, particularly for the 2010 experiment).

[37]We also did the analysis restricting to "middle of the road" voters (i.e., those who are not the ideological voters defined in Appendix Table C19) and obtained the same conclusions. Likewise, impacts of turnout might be larger among voters with low interest in government (as such voters might be less likely to know about polls). One could also tell a story that impacts would be larger among those with high interest in government (because these people would care about the polls). In either subpopulation, there is no relation between closeness beliefs and turnout.

jects significantly overestimate the probability of a very close election, at least relative to the historical evidence. However, subjects' tendency to overestimate beliefs may raise questions for some readers about whether the beliefs data are "sensible." Although our experimental treatment is exploiting changes in beliefs (as opposed to belief levels), we believe it is still useful to try to understand whether elicited belief levels are likely to represent subjects underlying expectations (as opposed to measurement error in the belief elicitation). For brevity, we discuss in detail in Appendix A.2. We argue that subjects' beliefs are consistent with literature in behavioral economics and we document empirically that subjects who seem more "behaviorial" as measured on our non-belief in the law of large numbers task have greater over-estimation. In addition, using data on the number of seconds that each subject spent on each question, we document that subjects took significant time to answer the various belief questions, suggesting they took the questions seriously.

Was the experiment "strong" enough to matter theoretically for turnout? Though we find strong 1st stage effects of our treatment on beliefs, could it have been that the changes in beliefs would not be large enough to affect turnout theoretically? This question is difficult to answer because it depends on the particular model of voting one is considering (recall that several classes of models predict that perceived closeness should increase turnout), as well as numerous assumptions about key parameters (e.g., distribution of voting costs, distribution of voting benefits, current beliefs about closeness, and any aggregate uncertainty). Nevertheless, Appendix D.6 examines the theoretical consequences of increasing beliefs in the context of a simple instrumental model. We find that the observed 2010 increases in perceived chance of less than 100/1,000 votes would predict sizable changes in turnout (on the order of 5-7pp), which is substantially higher than our reduced form estimates in Appendix Table C24. This suggests that our experiment was strong enough to matter theoretically in the context not only of the simple model considered, but also, as discussed in Appendix D.6, in variants and enriched versions of the model.

Were the gubernatorial races eclipsed in importance by senate races? One concern is that closeness may not have affected turnout because the elections we studied were not as important to voters as senate races. As mentioned above in Section 3, we chose to study gubernatorial elections because past research indicates that governors are substantially more visible to voters than senators. Still, it is possible that some people in our samples would have been more interested in the senate races than the gubernatorial races. In 2010, the only state in our sample without a senate election was TX, whereas in 2014, only FL and WI didn't have senate elections. If we re-do the 2014 results restricting to FL and WI, the results are qualitatively similar. Further, the closeness of the senate race is not a confound because we randomized within state and control for state fixed effects, which control for differences across states in non-governor races such as senate races.

6.2 The Relationship between Actual Margin and Voter Turnout: Perceived Closeness vs. Other Factors

How much precision do our estimates give us in assessing the importance of individual perceived closeness for explaining the relationship between actual closeness and turnout? Let B represent the impact on actual turnout of increasing the margin by 1pp. Let s denote the share of B that is driven by perceptions of closeness, whereas $1 - s$ represents the share of B that is driven by other factors, such as elite mobilization and social pressure. We found no evidence of $s > 0$ in our analysis above, and the analysis here shows how we can rule out $s > 0.12$ (in our preferred specification), thereby demonstrating that we have a precise "null result." See Appendix A.3 for details on the discussion in this subsection.

Set-up. Let T be voter turnout, P be perceived closeness, and E be other factors that affect turnout such as elite mobilization. Both P and E are functions of actual closeness, C.

That is, $T = T(P(C), E(C))$. We differentiate turnout with respect to actual closeness:

$$\overbrace{\underbrace{\frac{\partial T}{\partial C}}_{\text{Table } C8}}^{B} = \overbrace{\underbrace{\frac{\partial T}{\partial P}}_{\text{IV coefs}} \cdot \underbrace{\frac{\partial P}{\partial C}}_{\text{Table } C4}}^{sB} + \overbrace{\frac{\partial T}{\partial E}\frac{\partial E}{\partial C}}^{(1-s)B}$$

Consider moving from Election X to Election Y where the margin of victory decreases by 10pp. Appendix Table C8 provides estimates of B. To better correspond with much of the literature on closeness and turnout, we use the simple cross-election regression in column 1; the estimate of $B = 0.34$ implies that turnout would go up 3.4pp in response to a 10pp decrease in margin.

For the 2010 experiment, to estimate s, note that according to Appendix Table C4, a 10pp drop in actual margin is associated with decrease in perceived margin by 4.8pp, an increase in perceived chance of less than 100 votes by 1.4pp, an increase in perceived chance of less than 1,000 votes by 4.1pp, and an increase in perceived chance of less than 100/1,000 votes by 2.8pp. We multiply these changes by our IV estimates of the turnout impact of such changes, obtained from columns 3, 6, 9, and 12 of Table 6. Then, we divide by B to get s.

For the 2014 experiment, we can rule out that the postcard increases turnout by more than 0.77pp. In Tables 3-4, we estimate roughly that our close polls treatment decrease perceived margin by 2.8pp, increased the perceived chance of less than 100 votes by 2.5pp, increased the perceived chance of less than 1,000 votes by 2.3pp, and increased perceived chance of less than 100 or 1,000 votes by 2.4pp. We do not observe beliefs in the 2014 experiment. Instead, in order to derive estimates of s from the 2014 experiment, we assume that the 2014 postcards affect beliefs to the same extent as the 2010 online survey, a quite strong assumption that we discuss in Appendix A.3. Then, by the logic of the two sample IV estimator (Angrist and Krueger, 1992), we can divide our 2014 point estimate by the degree of the 1st stage 2010 treatment effect (just identified case) to obtain the 2014 estimate of how beliefs affect turnout.[38]

[38] We calculate two sample IV standard errors using the Delta Method, as in, e.g., Perez-Truglia and Cruces (Forthcoming). Details are in Appendix A.3.

Results. As seen in column 4 of Table 8, our estimated s values are mostly small and are all not significantly different from 0, with an average of 0.13 across the 8 fully filled-in rows. To combine the results from the different belief variables together, we calculate a weighted sum of estimated s values using predicted vote margin, Pr(Marg <100 votes), and Pr(Marg <1,000 votes), where each value is weighted according to the precision (i.e., inverse variance) of the estimate. We use the Delta Method to form a standard error for this weighted sum. For the 2010 results, our combined estimate of s using all three belief variables is 0.005, whereas for the 2014 results, our combined estimate of s is 0.06.

Last, in Panel C, we pool the 2010 and 2014 data together to run a reduced form regression, which allows us to estimate s using both experiments at once. We estimate $s = 0.05$, with a 95% confidence interval of $[-0.02, 0.13]$. A value of $s = 0.05$ implies that 95% of the relationship between actual closeness and turnout is driven by factors other than perceived closeness. Further, the 95% upper limit of 0.13 means we can rule out that more than 13% of the relationship between actual closeness and turnout is driven by perceived closeness.

It is important to note that this exercise partially relies on non-experimental variation (as well as the experimental variation). In particular, we do not randomize actual closeness across elections, so our estimates of $\frac{\partial T}{\partial C}$ and $\frac{\partial P}{\partial C}$ rely on observational data, where concerns about potential unobserved variables and choices about particular specifications may be more important than in experiments. Thus, the results of this exercise should be viewed as more tentative, as least compared to our main experimental estimates. Still, the exercise suggests that our experimental results seem inconsistent with s being more than a very modest level.

6.3 Did Information Change Preferences Over Candidates?

One potential concern is that subjects' preferences over candidates were also affected by the polls—for example, we may observe a "Bandwagon Effect, where observing that a candidate is further ahead makes someone more likely to want to vote for them, or the opposite.[39] We

[39]The fact that polls may lead to changes in preferences has been discussed extensively in both the theoretical and empirical literature. This literature, beginning with Simon (1954), Fleitas (1971), and Gartner

examine bandwagon effects by regressing a dummy for (self-reported) voting for the Democratic candidate on the perceived Democratic vote share, instrumenting with the Democratic vote share in the randomly assigned poll. Our main finding is that we fail to find causal evidence of bandwagon effects with respect to actual voting (Appendix Table C26). Furthermore, our main IV results on perceived closeness and turnout are robust to restricting to people whose intended probabilities of voting for each candidate do not change after receiving polls (Appendix Table C22). For brevity, these findings are detailed in Appendix A.4, along with further discussion of how our results relate to past findings.

7 Conclusion

In many models of turnout, voters are more likely to vote when the election is close because they are motivated to help decide the election. To test this prediction, we conducted large field experiments in 2010 and 2014 with US voters. In both cases, we fail to find evidence supporting the idea that believing an election is close causes individuals to be more likely to vote, even though the 2010 data indicates that the polls strongly affected beliefs.

Like all experiments, each of the two experiments has limitations. Even though the 2010 experiment was large, we did not know whether effects whether small or approximately zero, and some readers could have concerns about external validity due to the internet sample. The 2014 experiment was very large and used a broad national sample, but instead faced the limitation of not being able to measure beliefs, and instead relies on the assumption that the close poll information delivered with very similar wording via postcard would also affect closeness beliefs (given the evidence of this occurring in the online experiment). We believe, however, that the experiments complement one another very well, and together support that the impact on turnout of believing the election to be close is approximately zero. Of course,

(1976), suggests that polls may lead to Bandwagon Effects, making poll winners win with even greater leads than predicted. Most experimental studies find that majority supporters vote with greater propensities than minority ones (Duffy and Tavits, 2008; Großer and Schram, 2010; Kartal, 2015). Cason and Mui (2005) find that the participation rates of the majority are higher than the participation rates of the minority.

as mentioned previously, the size of any predicted relationship varies depending on a variety of unobservable variables (e.g., voting costs, aggregate uncertainty, voting benefits). While we cannot rule out that our treatments have any effect (e.g., we cannot reject that the close poll treatment in the second experiment could increase turnout by 0.8pp), our results suggest that beliefs about closeness are not more than a very modest determinant of turnout in US gubernatorial elections.

An advantage of our study is that we test a prediction shared by many theories. While some readers may not be surprised that our results are inconsistent with a plain vanilla "pivotal voting model," our results also speak to many other models and concepts of voting.

Our results seem broadly supportive for non-instrumental models of voting. These include expressive models of turnout (e.g., Morgan and Várdy, 2012; Hillman, 2010; Hamlin and Jennings, 2011), as well as models based on social norms (e.g., Gerber et al., 2008; DellaVigna et al., 2017). While active research is already underway on non-instrumental voting models, we hope that our results spur even greater interest (both theoretical and empirical) in studying such models for large elections. Our results also suggest that the observed closeness-turnout relationship in the literature is likely mostly driven by elite mobilization and other endogenous features of close elections as opposed to believed closeness making individuals directly more likely to vote. In our data, we can rule out that more than 13% of the observed closeness-turnout relationship is driven by perceptions of closeness.

Our conclusions are specific to the type of election we study. For example, in much smaller elections, it is possible that closeness beliefs may affect turnout. We would also speculate that beyond the size of the electorate, the context of an election might be important. US gubernatorial elections are often quite ideological; it could be that closeness beliefs may matter for other types of elections such as referenda. Further, our results do not rule out that closeness beliefs might be important for non-politics elections such as union certification elections (Farber, 2015) or shareholder votes, and these may also be smaller elections.

Our results are relevant for policy-makers or political parties interested in boosting turnout (in general or for particular groups). In particular, our results suggest that increasing a person's belief the election will be close is unlikely to affect the person's turnout decision. However, political parties and other elites may still find it useful to focus campaign efforts in close elections, due to the turnout effects that campaigning can have separate from altering beliefs about electoral closeness.

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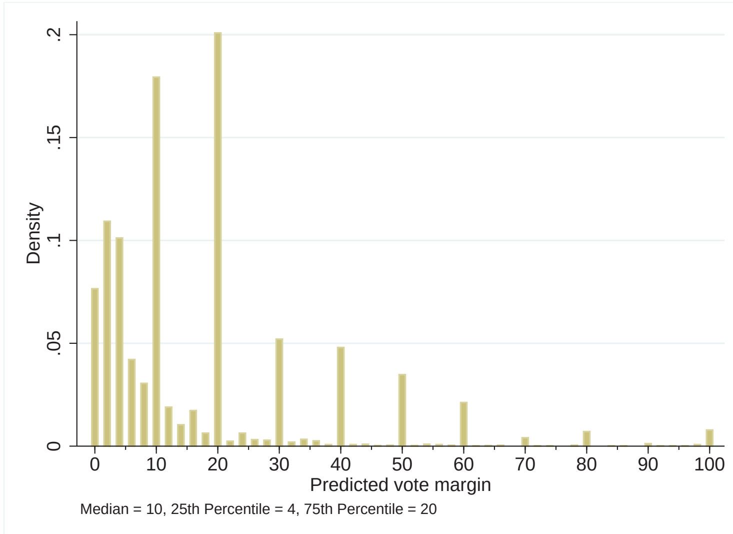
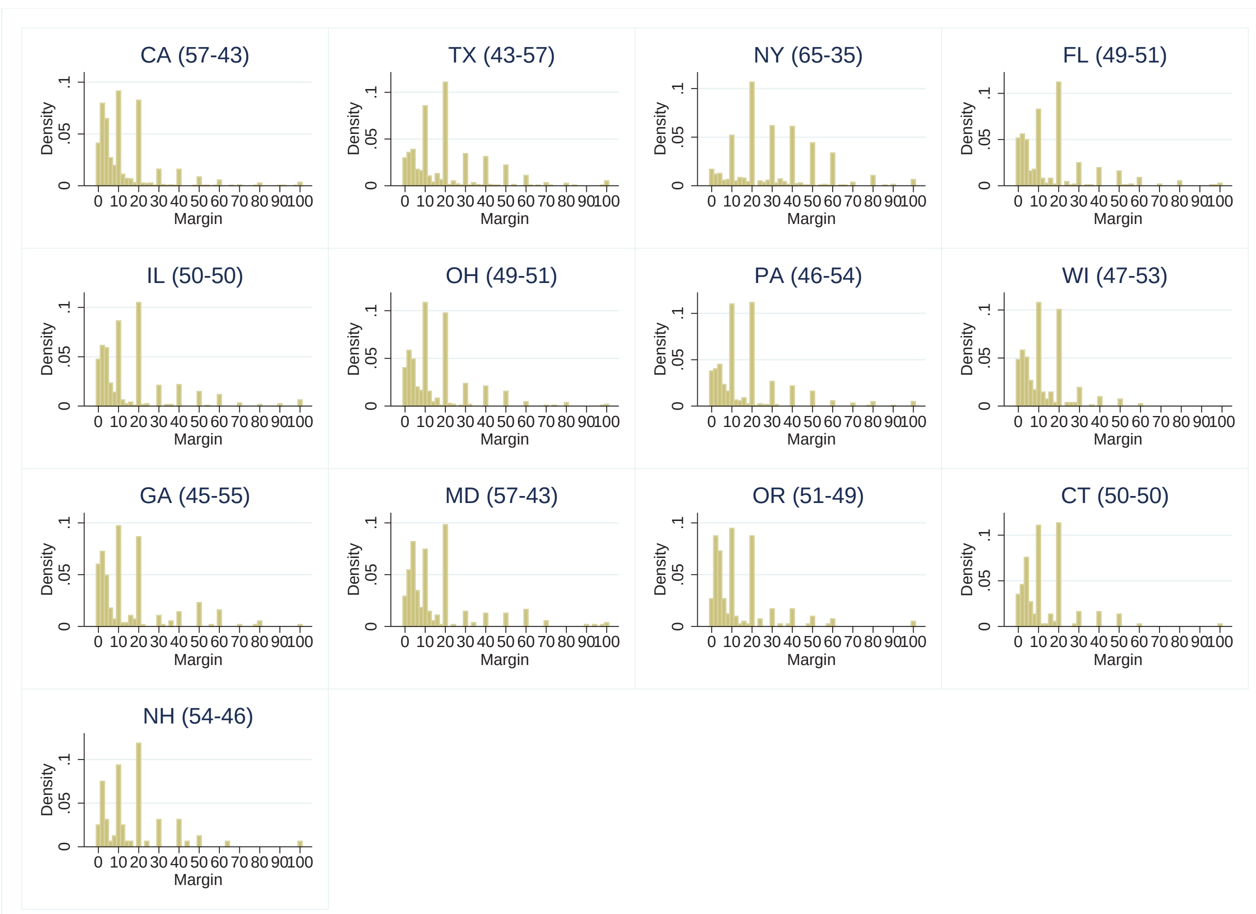
Figure 1: Distribution of Predicted Margin of Victory, Before Treatment (2010 experiment)



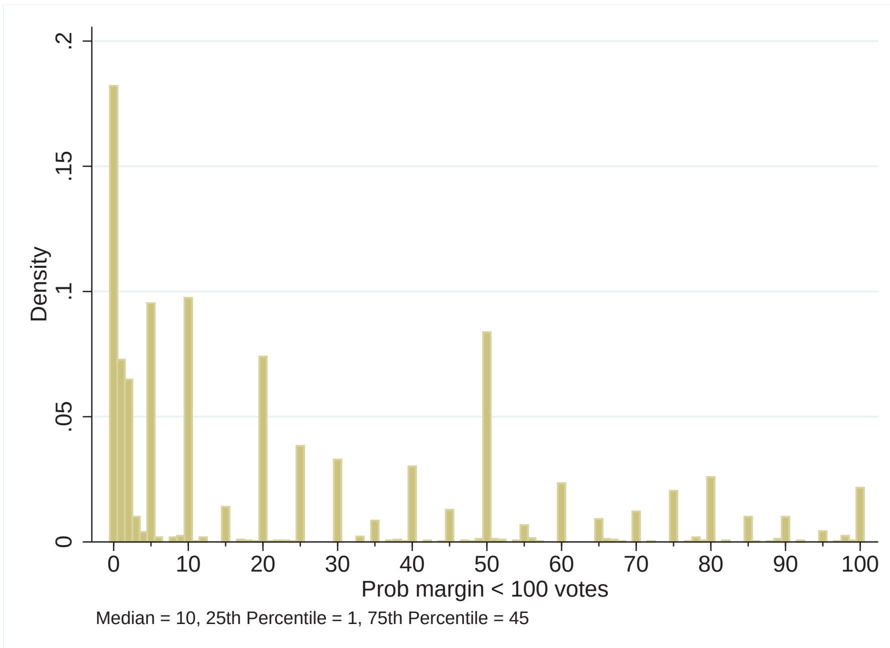
(a) Overall



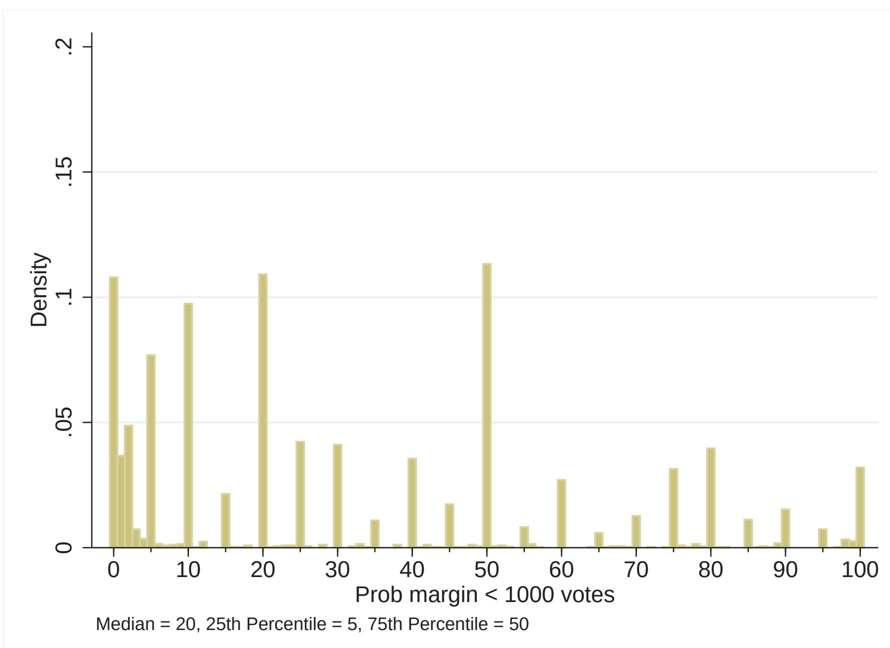
(b) Across States

Notes: This figure presents the pre-treatment distribution of subjects' beliefs about the margin of victory among the two leading candidates. Panel (a) presents the predicted margin of victory combining all states. The margin of victory is the difference in vote shares rounded to the nearest integer, i.e., a 50/50 election corresponds to 0 margin, a 51/49 election corresponds to a margin of 2, and so on. Panel (b) presents the same information broken out by state. The numbers in parentheses for each state represent the actual vote shares among the two leading candidates (the Democrat share is first). Data are from the 2010 experiment.

Figure 2: Subjective Probabilities that Gubernatorial Election Will be Decided by Less than 100 Votes or 1,000 Votes, Before Treatment (2010 experiment)



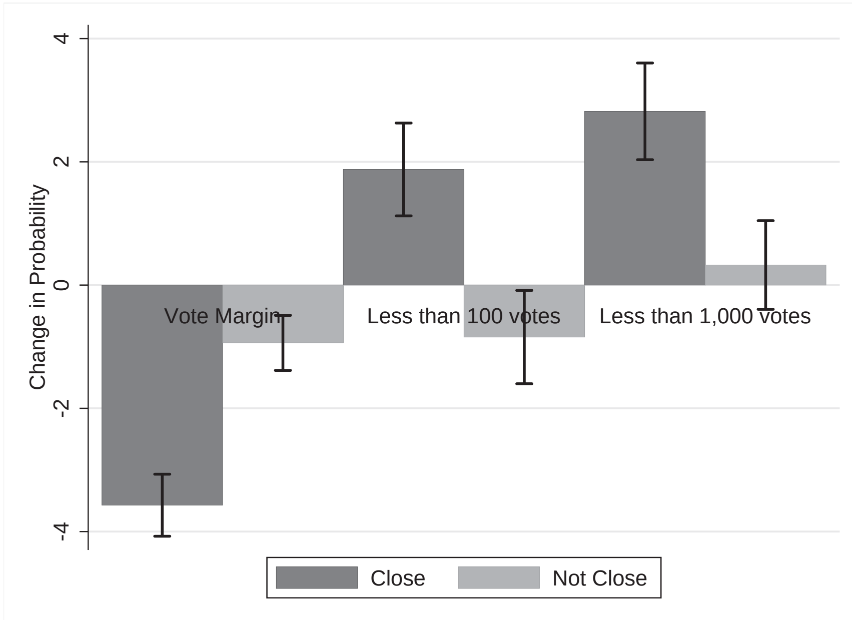
(a) Less than 100 Votes



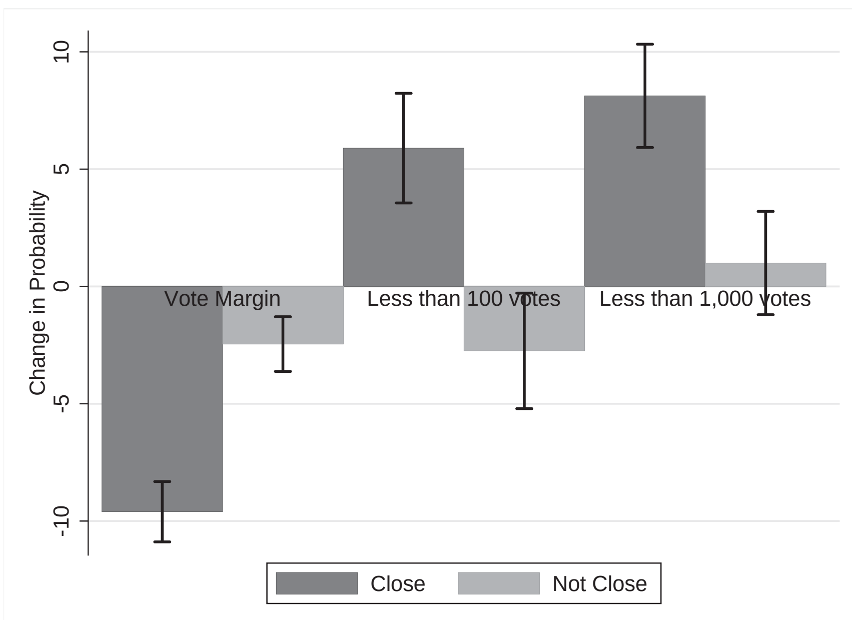
(b) Less than 1,000 Votes

Notes: These graphs plot the distribution of answers to the question asking for the probability the election in the respondent's state would be decided by less than 100 votes or less than 1,000 votes. These subjective beliefs were elicited before the poll information was provided. The data are from the 2010 experiment.

Figure 3: Belief Updating in Response to Polls (2010 experiment)



(a) Overall impact on beliefs



(b) Impact on beliefs among those who change their response

Notes: These graphs analyze the impact of the experiment on voters' beliefs. Each bar represents the average change in beliefs for those receiving either the close or not close poll treatments. They are calculated via a person-level regression of changes in beliefs (i.e., post-treatment beliefs minus pre-treatment beliefs) on a constant using robust standard errors. Whiskers show the 95% confidence interval for the coefficient estimate (i.e., plus/minus about 1.96 standard errors). Thus, the whiskers reflect a confidence interval on each bar in absolute terms (and not for a comparison of the close bar versus the not close bar). The differences between the close and not close bars are highly statistically significant, as indicated in Tables 3-4.

Table 1: Comparing Means between People Getting Close Treatment and People Getting Not Close Treatment: 2010 Experiment

	Close ($N = 3,348$)	Not Close ($N = 3,357$)	t-test
Demographics:			
Male	0.39	0.39	0.91
Black	0.08	0.08	0.87
Hispanic	0.06	0.06	0.67
Other	0.03	0.03	0.96
Mixed race	0.02	0.02	0.34
Age	53.21	53.45	0.49
Less than high school	0.03	0.03	0.56
High school degree	0.14	0.13	0.54
Some college or associate degree	0.34	0.34	0.96
Bachelor's degree	0.29	0.29	0.76
Master's or PhD	0.21	0.21	0.91
Household income $25k-$50k	0.22	0.24	0.09
Household income $50k-$75k	0.24	0.23	0.14
Household income $75k-$100k	0.18	0.17	0.31
Household income $100k and up	0.24	0.25	0.32
Political variables:			
Registered Democrat	0.47	0.49	0.44
Registered Republican	0.36	0.36	0.78
No party affil/decline state/indep	0.14	0.13	0.53
Other party registration	0.03	0.02	0.79
Identify Nancy Pelosi as Speaker	0.82	0.83	0.23
Interest in politics (1-5 scale)	3.73	3.7	0.31
Affiliate w/ Democrat party (1-7)	4.23	4.24	0.87
Ideology (1-7 Scale, 7=Ext Liberal)	3.89	3.87	0.65
Predicted vote margin, pre-treat	17.05	17.1	0.91
Prob margin < 100 votes, pre-treat	23.44	25.44	0.04
Prob margin < 1,000 votes, pre-treat	31.93	31.46	0.65
Prob voting, pre-treatment	87.08	87.04	0.95
Prob vote Dem, pre-treatment	49.71	50.17	0.67
Prob vote Republican, pre-treat	41.46	41.53	0.95
Prob vote underdog, pre-treat	40.79	41.52	0.49
Share voted previous 5 elections	0.65	0.65	0.92

Notes: This table compares means across the close and not close poll individuals in the 2010 experiment. "Close" refers to individuals receiving the close poll treatment. "Not Close" refers to individuals receiving the not close poll treatment. The numbers in the "t-test" column are the p-values from a two-sided t-test. The sample is restricted to individuals who respond to the survey. To avoid any anchoring effects, voters were asked about either the probability of margin less than 100 votes or probability of margin less than 1,000 votes, so the sample is only roughly half as large for those two questions. The number of non-missing observations is less than 6,705 for some of the other political variables, particularly for party registration which is non-missing for 3,823 people (non-missing for 1,902 people in Not Close group and for 1,921 in Close group). See Appendix Table C3 for exact observation counts.

Table 2: Nonparametric Evidence on Changes in Beliefs and Voting Intentions After Treatment (2010 Experiment)

	N	Decrease	Same	Increase	N	Decrease	Same	Increase
	Not Close Treatment				Close Treatment			
Predicted margin of victory	3,311	19.0%	61.8%	19.2%	3,301	30.1%	62.8%	7.1%
Prob margin < 100 votes	1,601	18.2%	69.3%	12.6%	1,681	11.3%	68.2%	20.5%
Prob margin < 1000 votes	1,749	18.4%	67.3%	14.3%	1,657	10.4%	65.3%	24.3%
Intended prob of voting	3,350	3.4%	88.3%	8.3%	3,347	3.7%	88.0%	8.4%
Intended prob voting for underdog	3,357	6.1%	87.7%	6.3%	3,348	5.7%	88.2%	6.1%

Notes: This table describes how voters' perception of the vote margin, their perception the election is decided by less than 100 or 1,000 votes, their predicted probability of voting, and their intended probability of voting for the underdog candidate (the candidate behind in the polls) change under the two information treatments (close poll and not close poll). Note that it is possible to change predicted Democrat vote share without changing predicted margin of victory (i.e., a 52D-48R prediction changes to a 48D-52R prediction).

Table 3: The Effect of the Close Poll Treatment on Vote Margin Predictions (2010 Experiment)

Dep. var.:	b_{post} (1)	b_{post} (2)	b_{post} (3)	Δb (4)	b_{post} (5)	b_{post} (6)	b_{post} (7)	b_{post} (8)
Close poll treatment (0=Not Close, 1=Close)	-2.80***	-2.79***	-2.68***	-2.62***	-2.72***	-5.45***	-3.83***	-4.66***
	(0.39)	(0.36)	(0.29)	(0.34)	(0.36)	(1.44)	(1.00)	(0.78)
Pred vote margin, pre-treat			0.54***					
			(0.02)					
Close poll*Interest in politics (1-5 scale)						0.73**		
						(0.36)		
Close poll*Identify Nancy Pelosi as Speaker							1.35	
							(1.07)	
Close poll*Share voted previous 5 elections								2.98***
								(1.01)
Interest in politics (1-5 scale)					-0.03	-0.38	-0.03	-0.01
					(0.21)	(0.27)	(0.21)	(0.21)
Identify Nancy Pelosi as Speaker					-1.59***	-1.60***	-2.27***	-1.60***
					(0.54)	(0.54)	(0.78)	(0.54)
Share voted previous 5 elections (administrative)					-1.16**	-1.15**	-1.17**	-2.66***
					(0.56)	(0.56)	(0.56)	(0.77)
Mean DV if not close poll=1	16.15	16.15	16.05	-0.938	16.02	16.02	16.02	16.02
State FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,650	6,650	6,612	6,612	6,529	6,529	6,529	6,529
R-squared	0.01	0.14	0.45	0.02	0.14	0.14	0.14	0.14

Notes: In all columns, the dependent variable is the post-treatment predicted vote margin, except in column 4 where the dependent variable is change in predicted vote margin (i.e., post-treatment predicted vote margin minus pre-treatment predicted vote margin). Robust standard errors in parentheses. We use robust standard errors because the randomization is at the person level. Demographic controls are gender, race (Black, Hispanic, other, mixed), 10-year age bins (25-34, 35-44, 45-54, 55-64, 65-74, 75+), education dummies (less than high school, some college/associate degree, bachelor's degree, master's/PhD), and $25k household income bins (25k-50k, 50k-75k, 75k-100k, 100k+). The treatment variable is discrete, i.e., it is a dummy for getting the close poll (versus getting the not close poll). * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: The Effect of the Close Poll Treatment on the Perceived Likelihood of the Election Being Decided by Less than 100 or Less than 1,000 Votes (2010 Experiment)

Dep. var.:	Prob < 100 votes			Prob < 1,000 votes			< 100 or 1,000 votes		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Close poll treatment	0.80	2.47	2.54	2.93	2.55	2.33	1.67	2.47	2.43
	(1.01)	(0.53)***	(0.53)***	(1.04)***	(0.53)***	(0.52)***	(0.73)**	(0.38)***	(0.37)***
Prob <100 votes, pre-treat		0.87	0.85						
		(0.01)***	(0.01)***						
Prob <1,000 votes, pre-treat					0.88	0.86			
					(0.01)***	(0.01)***			
Prob <100 or 1,000 votes, pre-treat								0.88	0.86
								(0.01)***	(0.01)***
Mean DV if not close poll=1	24.54	24.55	24.55	31.79	31.79	31.79	28.32	28.33	28.33
State FE	No	No	Yes	No	No	Yes	No	No	Yes
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes
Observations	3,286	3,282	3,282	3,407	3,406	3,406	6,693	6,688	6,688
R-squared	0.00	0.73	0.73	0.00	0.74	0.75	0.00	0.74	0.75

Notes: The dependent variable is a voter's post-treatment belief that the election will be decided by less than 100 votes or less than 1,000 votes. Voters were either asked about 100 votes or about 1,000 votes. The data are pooled in columns 7-9. Robust standard errors in parentheses. Demographic controls are the same as in Table 3. The treatment variable is discrete, i.e., it is a dummy for getting the close poll (versus getting the not close poll). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Beliefs About the Closeness of the Election and Voter Turnout, OLS Results (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	-0.03	0.01	0.02									
	(0.03)	(0.04)	(0.04)									
Pred vote margin, pre-treat		-0.06*	-0.03									
		(0.03)	(0.03)									
Pr(Marg <100 votes), post				-0.05**	0.01	0.03						
				(0.02)	(0.04)	(0.04)						
Pr(Marg <100 votes), pre					-0.07	-0.06						
					(0.04)	(0.04)						
Pr(Marg <1,000 votes), post							0.00	0.01	0.03			
							(0.02)	(0.04)	(0.04)			
Pr(Marg <1,000 votes), pre								-0.00	-0.00			
								(0.04)	(0.04)			
<100 or 1,000 votes, post										-0.02	0.01	0.03
										(0.01)	(0.03)	(0.03)
<100 or 1,000 votes, pre											-0.03	-0.03
											(0.03)	(0.03)
Mean DV	72.14	72.19	72.19	72.25	72.33	72.33	71.94	71.93	71.93	72.09	72.13	72.13
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	6,650	6,612	6,612	3,286	3,282	3,282	3,407	3,406	3,406	6,693	6,688	6,688
R-squared	0.45	0.45	0.46	0.45	0.45	0.46	0.45	0.45	0.46	0.45	0.45	0.46

Notes: The dependent variable is turnout (0-1) from administrative voting records, with coefficients multiplied by 100 for ease of readability. Robust standard errors in parentheses. All specifications are OLS regressions. All regressions include state fixed effects and past voting controls (5 dummies for having voted in the general elections in 2000, 2002, 2004, 2006, and 2008). The voting rate based on administrative data is 72% (71.9% for close poll, 72.1% for not close poll). Demographic controls are as listed in Table 3. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Beliefs About the Closeness of the Election and Voter Turnout, IV Results (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
IV Results:												
Pred vote margin, post-treat	-0.12	-0.15	-0.16									
	(0.29)	(0.30)	(0.30)									
Pred vote margin, pre-treat		0.03	0.06									
		(0.17)	(0.17)									
Pr(Marg <100 votes), post				-0.52	-0.23	-0.19						
				(1.47)	(0.46)	(0.45)						
Pr(Marg <100 votes), pre					0.13	0.13						
					(0.40)	(0.38)						
Pr(Marg<1,000 votes), post							0.27	0.30	0.38			
							(0.43)	(0.47)	(0.49)			
Pr(Marg<1,000 votes), pre								-0.27	-0.30			
								(0.42)	(0.42)			
<100 or 1,000 votes, post										0.09	0.05	0.08
										(0.51)	(0.33)	(0.33)
<100 or 1,000 votes, pre											-0.07	-0.07
											(0.29)	(0.29)
F-stat on excl instrument	57.52	86.45	85.96	0.717	23.17	23.68	6.914	21.63	20.04	4.888	43.09	42.93
Mean DV	72.14	72.19	72.19	72.25	72.33	72.33	71.94	71.93	71.93	72.09	72.13	72.13
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	6,650	6,612	6,612	3,286	3,282	3,282	3,407	3,406	3,406	6,693	6,688	6,688
First Stage Results:												
Close poll treatment	-2.80***	-2.69***	-2.67***	0.85	2.54***	2.55***	2.72***	2.44***	2.33***	1.61**	2.45***	2.42***
	(0.37)	(0.29)	(0.29)	(1.01)	(0.53)	(0.52)	(1.03)	(0.53)	(0.52)	(0.73)	(0.37)	(0.37)

Notes: The dependent variable is turnout (0-1) from administrative voting records, with coefficients multiplied by 100 for ease of readability. Robust standard errors in parentheses. In all specifications, post-treatment beliefs are instrumented with a dummy variable for receiving the close poll treatment. All regressions include state fixed effects and past voting controls (5 dummies for having voted in the general elections in 2000, 2002, 2004, 2006, and 2008). Demographic controls are as listed in Table 3. The voting rate based on administrative data is 72% (71.9% for close poll, 72.1% for not close poll). After showing the IV results, we also present the exact first stage results, where a belief variable is regressed on a dummy for the close poll. These results are slightly different from those in Table 3-4 because we include past voting controls. For reduced form results, see Appendix Table C24. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Impact of Close/Not Close Postcard Treatments on Turnout (2014 Experiment)

	(1)	(2)	(3)	(4)
Close poll (vs. not close poll)	0.29	0.29		0.29
	(0.25)	(0.25)		(0.25)
Close poll (vs. control)			0.34*	
			(0.18)	
Not close poll (vs. control)			0.05	
			(0.18)	
Small electorate likely				-0.17
				(0.25)
F(Close vs. NotClose)			0.242	
Mean DV if not close poll=1	53.45	53.45		53.45
Mean DV if control=1			53.43	
Additional controls	No	Yes	Yes	Yes
Observations	126,126	126,126	1,385,318	126,126

Notes: The dependent variable is turnout (0-1) from administrative voting records, with coefficients multiplied by 100 for ease of readability. Turnout is defined at the individual level, and is based on merging by date of birth. An observation is a person. Standard errors clustered by household are in parentheses. Each regression includes dummy for the 8 randomization strata; separate dummies for voting in the 2008, 2010, and 2012 elections; and state dummies. The additional controls are controls for gender, race (dummies for Black, Hispanic, or other), age (dummies for 25-34, 35-44, 45-54, 55-64, 65-74, 75+), and party registration (dummies for Democrat and Republican, as well as a dummy for missing party registration). The sample size is much larger in column 3 than columns 1, 2, and 4 because column 3 includes control households that did not receive a postcard. In contrast, columns 1, 2, and 4 are restricted to individuals in households that received a postcard. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: The Relevance of Perceived Closeness for the Observational Relationship between Actual Closeness and Voter Turnout

Belief variable used:	Δ beliefs from 10pp drop in actual turnout	95% CI for impact of beliefs on voting	95% CI for impact of beliefs channel	Point estimate on s	95% CI for s
Panel A: 2010 Experiment	(1)	(2)	(3)	(4)	(5)
Predicted vote margin	-4.8pp	[-0.76, 0.43]	[-2.1, 3.7]	0.23	[-0.62, 1.08]
Pr(Marg <100 votes)	+1.4pp	[-1.08, 0.69]	[-1.5, 0.9]	-0.08	[-0.44, 0.28]
Pr(Marg <1,000 votes)	+4.1pp	[-0.58, 1.35]	[-2.4, 5.5]	0.46	[-0.69, 1.61]
<100 or 1,000 votes	+2.8pp	[-0.58, 0.73]	[-1.6, 2]	0.06	[-0.47, 0.59]
Overall for 2010				0.005	[-0.31, 0.32]
Panel B: 2014 Experiment	(1)	(2)	(3)	(4)	(5)
Predicted vote margin	-4.8pp	[-0.28, 0.07]	[-0.3, 1.4]	0.15	[-0.10, 0.40]
Pr(Marg <100 votes)	+1.4pp	[-0.08, 0.31]	[-0.1, 0.4]	0.05	[-0.03, 0.13]
Pr(Marg <1,000 votes)	+4.1pp	[-0.09, 0.34]	[-0.4, 1.4]	0.15	[-0.11, 0.40]
<100 or 1,000 votes	+2.8pp	[-0.08, 0.32]	[-0.2, 0.9]	0.10	[-0.07, 0.26]
Overall for 2014				0.06	[-0.01, 0.14]
Panel C: Pooled Data	(1)	(2)	(3)	(4)	(5)
Overall for pooled data				0.05	[-0.02, 0.13]
Reduced form regression	coef on close poll treatment: 0.25 (se=0.24)			N=132,831	

Notes: This table estimates s, which is the share of the observational relationship between actual closeness and voter turnout that can be attributed to individual perceptions of closeness. For Panels A and B, column 1 is based on the coefficient estimates in columns 1, 3, 5, and 7 of Appendix Table C4. For Panel A, column 2 is based on the 95% confidence intervals for post-treatment beliefs in columns 3, 6, 9, and 12 of Table 6. For Panel B, column 2 is based on the confidence intervals from Appendix Table C25. For Panels A and B, column 3 equals the confidence interval in column 2 multiplied by column 1. For Panels A and B, column 4 provides a point estimate of s, and it is equal to the midpoint of the column 3 confidence interval divided by 3.4pp (10pp*$B = 0.34$). For Panels A and B, column 5 equals the column 3 confidence intervals divided by 3.4pp. Thus, the column 5 confidence intervals for s include estimation error from IV estimation, but ignore estimation error in estimating how perceived closeness responds to actual closeness and in how turnout responds to actual closeness. Panel C estimates s while pooling data together from the 2010 and 2014 experiments. First, we perform a reduced form regression of turnout on the close poll dummy, the share of times voted in the past, controls for race and gender, 10-year age bins, a year dummy, and state fixed effects, while clustering standard errors by household; this regression is shown in the final line of the table. Then, we estimate s using the different belief measures, and combine those estimates together to create an overall s for the pooled data. See Section 6.2 for further information on this exercise, and see Appendix A.3 for further details.