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. In a field experiment during the 2010 US gubernatorial elections, 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. 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. Our results suggest, with 95% confidence, that no more than 13% of the cross-state relationship between actual closeness and turnout is due to the impact of perceived closeness.

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1 Introduction

Why people vote is a core question in political economy. 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. 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, lab studies abstract from the context of real-life elections and so may fail to account for factors that are salient outside the lab. Perhaps in part due to these challenges, recent empirical work on 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), politician race (Washington, 2006), habit (Fujiwara et al., 2016), or the media (Gentzkow et al., 2011; Drago et al., 2014; Falck et al., 2014; 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 aspects of both 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 US 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 polls 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 if 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 shown polls, most subjects overestimate the chance of a very close election. The median probabilities that the gubernatorial race would be decided by less than 100 or less than 1,000 votes were 10% and 20%, respectively, much higher than historical averages. While 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, belief changes 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—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 (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. (Going forward, we abbreviate percentage points by "pp.") A cross-randomized treatment that randomly provided an expert prediction of whether the electorate size would be smaller or larger also had no impact on turnout.

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 (e.g., people who don't always vote; or people who report having strong political ideologies and thus likely care a lot about the election outcome). Combining data from the 2010 and 2014 experiments, in our preferred specification, we estimate that only (a statistically insignificant) 5% of the observed relationship between actual closeness and turnout is driven by perceptions of closeness. The top of our 95% confidence interval is 13%, meaning that no 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 way to test 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 sports, 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 people believe they have a higher chance of influencing the election or because of other reasons correlated with the election being close (Cox, 1999, 2015).

[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.

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 referenda, 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.[3]

A complementary approach to examine whether closeness affects turnout is to use lab experiments. Though samples are generally small, lab experiments offer unparalleled control, and 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.[4]

[3]We caveat by noting that individually-assigned treatments such as ours could also have the potential to activate social pressure or peer effects if telling people the election is close makes them think that others will vote, which makes them anticipate greater social pressure or peer effects. However, this seems much less immediate than social pressure or peer effects arising from an aggregate treatment, where individuals may be influenced by others due to the treatment actually making the others more likely to vote.

[4]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. By controlling the distribution of induced preferences, lab

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.[5] In addition, we are aware of very few studies that seek to measure or influence voter beliefs about electoral closeness.[6] 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.[7] 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,

studies are ideally suited for testing multiple (and sometimes subtle) predictions of pivotal voter models. 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).

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

[6] 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.

[7] 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.

thereby providing greater external validity. Fourth, we consider how our results relate to a broad range of voting theories.[8]

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

[8] 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 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.

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 for the voter, and we consider only people who support the minority candidate, then P1 continues to hold.[9]

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.[10]

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.[11]

[9]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.

[10]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.

[11]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.

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 distribution 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.[12]

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

[12]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.

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 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.[13] 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

[13] 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).

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 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 to predict 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.[14] 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

[14] 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.

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 may be roughly equivalent to a 1 in 100 chance of being pivotal. All belief questions were administered without any incentives for accuracy.[15]

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.[16]

We asked the same questions immediately after treatment to detect if there was any immediate impact on voting intentions, given the possibility (discussed more 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).

[15]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 ages and schooling 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 uncertainty (see Kendall et al. (2015) for an example that does), doing this for simplicity and time/financial constraints from the survey company. We address potential measurement error in stated beliefs by instrumenting beliefs with the randomized treatment. Appendix A.3 discusses further.

[16]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).

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 focus on beliefs about electoral closeness, we perform most analyses restricting to these 6,705 individuals who did the survey, as belief data are only observed for those taking the survey.[17]

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%[18]

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

[17] Beyond those assigned to the Close and Not Close treatments, there are roughly 5,000 voters in the Control group (the group that did not get invited to take the survey). Control voters are excluded from most analyses (as we have no belief data for them), but they are included in the reduced form in column 3 of Table C27.

[18] 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.

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.[19]

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.3 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

[19]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.

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 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 a Master's or PhD. Even for these well-educated voters, the median perceived chances 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.3, 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). Consistent with theory, voters who are less informed update more. We measure how informed voters are using self-reported 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 with very low interest in politics updates by 4.7pp, whereas a voter with a very high interest in politics updates by only 1.8pp.[20]

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.[21] 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.

[20] 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.

[21] 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.

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 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 is highly persistent (e.g., 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.[22]

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.

[22] 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).

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 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.[23]

Beyond the factors discussed in the Introduction, OLS may be biased for further reasons.[24] 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) the election is close. Third, additional unobserved factors could affect 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.[25]

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

[23]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.

[24]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).

[25]These results are slightly different from those in Table 3-4 because we include past voting controls. For reduced form results, see Appendix Table C27.

earlier discussed initial imbalance between the Close and Not Close groups in terms of initial 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 chance of a very close election is anywhere near as important as that of other voting predictors like age, education, and income, where relations on the order of 10-40pp. Even though IV has standard errors that are roughly 10 times larger (or more) than OLS, our estimated "zeros" are still reasonably precise. Section 6.1 discusses how IV precision further improves when restricting to subjects who update beliefs after treatment, e.g., the top of the 95% CI for column 12 drops from 0.73 to 0.42 (Table C19). Section 6.2 returns to the question of precision for our 2010 study. It is hard to know the exact source of the difference between OLS and IV, but we suspect that measurement error is quite important for OLS.[26]

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 related forms or constructs of a person's underlying perception of election closeness. To the extent that the different closeness variables represent

[26]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.

different underlying concepts, 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.

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 showed 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 (e.g., 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.[27] 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,

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

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.[28] We implemented a 2x2 design of 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).[29] 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

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

[29] 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.

readership of our postcard.[30]

The randomization was conducted using voter file records provided by an anonymous vote validation company.[31] We stratified by state, and attempted to stratify by whether someone voted in the general elections of 2008, 2010, and 2012.[32] 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 C26).

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"

[30]We do not claim that everyone in our study read the postcard, as there was likely a sizable share that did not. Rather, we argue that people not reading the postcard seems very unlikely to be an explanation for our estimated 0 effect, given that the average impact on turnout of a single mailer across studies is 0.75 pp (Gerber and Green, 2016), as discussed below, and such mailers are likely quite similar to ours in whether they get read. Further, the "message" of closeness in our mailer did not increase turnout, whereas other messages such as gratitude or social pressure on mailers substantially boost turnout (Gerber and Green, 2016). Appendix A.4 discusses how assumptions about the magnitude of belief impacts in 2014 affect our conclusions.

[31]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).

[32]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.

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 we have different states across the two years.[33]

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

[33] 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.

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. Further, column 5 shows there is no significant interaction between the two treatments.

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 5-8pp. 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.[34]

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 C27 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).

[34] 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 impact on turnout (Gerber and Green, 2016)—without these studies, the average effect would be higher than 0.75pp.

6 Alternative Explanations and Further 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 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 for the 2010 experiment, 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.[35] 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

[35] 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, screen time data show that few people lingered excessively after seeing the polls we provided (e.g., only 3% of people spent more than 1 minute looking at the poll screen, while the median person spent 15 seconds), so it is unlikely this occurred.

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 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 their intended probability of voting after receiving poll information.[36]

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

[36]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. Also, while we caveat by noting that intended probability of voting is over-reported and potentially subject to ceiling effects (given the high base rate), other field experiments finds significant impacts of treatments on intention to vote even when intention to vote is very high (e.g., Doherty and Adler, 2014). In Doherty and Adler (2014), a single mailer increases intended chance of turnout even though the average intended chance of turnout in the control group is 3.8 on a 1 (definitely not) to 4 (definitely will) turnout scale.

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 (Table C15), though we caveat by noting that day of survey is chosen by respondents. Our conclusions are also unchanged if we restrict to the half of the sample that took the survey closest to the election (Table C16).

Last, while we could not measure beliefs a 2nd 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 50-100% two months after a one-time treatment.[37] Especially given we sent an email reminding voters of the polls that we provided them, we hypothesize that most of 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

[37]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 the overall return to college in the US, 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. On political attitudes, Broockman and Kalla (2016) find 3-month persistence of over 100% of a US information intervention on transgender tolerance. While these studies do not concern non-attitudinal political beliefs (we are not aware of any field experiments regarding persistence of non-attitudinal political beliefs), we have little reason to believe that there would be less persistence in our domain compared to these others. Furthermore, the time between information provision and voting in our study is substantially less than two months, making it easier to achieve high levels of persistence. Additionally, we can measure the extent to which polls changed in the days before the election after our information arrived. If true closeness were changing a lot, this might make differences in perceived closeness induced by our experiment less likely to persist. However, the average absolute difference in two-party Democrat poll share during the days before the election after our information first arrived vs. two-party Democrat poll share during the prior 4 weeks is only 1pp, and our results are robust to excluding the 2 of 20 elections where the difference is more than 2pp.

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).[38]

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 (Delli 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).[39] 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 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 C17 (2010 experiment) and C18 (2014 experiment), our zero result is robust to restricting to individuals who don't always vote.[40]

Given that many people do not update beliefs, one question is whether our results are robust to restricting to cases where a person updates their beliefs. Appendix Table C19 shows our results are robust to this restriction, and become more tightly estimated. In column 12,

[38] Levitt and List (2007, 2011) indicate that Hawthorne Effects result from a desire to please the experimenter. Such a desire would seem more driven by preferences instead of information.

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

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

the top of the 95% CI falls to 0.42 (compared 0.73 in Table 6), so increasing the perceived chance of a very close election by 5pp would increase turnout by no more than 2.1pp.[41]

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 C21 shows no impact of beliefs on turnout when restricting to voters who rate themselves as having strong political ideologies. This test in Table C21 is also useful for exploiting voters who likely care a lot about the election outcome, and therefore might respond most to perceived closeness.[42]

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 C23 (2010 experiment) and C24 (2014 experiment).

Closeness information may be most relevant when the election is close to 50/50. Our 2010 and 2014 conclusions are also robust to restricting to states where we use a 50/50 close poll (though we have much less power here for the 2010 analysis).

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

[41] If we use beliefs in logs instead of levels here, the zero results also seem precise (see Appendix A.2).

[42] 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 C21) and obtained the same conclusions. Likewise, impacts of turnout might be larger among voters with low interest in politics (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 politics (because these people would care about the polls). In either subpopulation, there is no relation between closeness beliefs and turnout. Appendix Table C22 covers the case of high interest in politics.

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.3. 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 hard to answer because it depends on the particular model of voting (recall that several classes of models predict that perceived closeness should increase turnout), as well as numerous assumptions about key parameters (e.g., distributions of voting costs and 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 C27. 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.[43] Put differently, if voters were motivated by instrumental considerations, biased beliefs of the level observed in our experiment seem sufficient to rationalize significant turnout.

Were the gubernatorial races eclipsed in importance by senate races? One

[43] A different question besides whether the experiment was strong enough to matter theoretically is, under the beliefs that we observe, could the level of turnout have been rationalized by an instrumental model? In Appendix D.7, we show that observed turnout can be rationalized if the ratio of voting benefits to costs for the marginal voter is 800:1. This is smaller than other ratios that have been considered reasonable in the literature. For example, Myatt (2015) discusses a ratio of 2500:1.

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.4 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:

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

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, to estimate 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.4. Then, by the logic of the two sample IV estimator (Angrist and Krueger, 1992), we 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.[44]

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)

[44] We calculate two sample IV standard errors using the Delta Method (e.g., Perez-Truglia and Cruces, 2017). See Appendix A.4 for details. Appendix A.4 also discusses the question for two sample IV of whether the 2010 and 2014 samples should be thought of as drawn from the same population.

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.[45] We 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

[45]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 (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.

evidence of bandwagon effects with respect to actual voting (Appendix Table C29). 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 C25). For brevity, these findings are detailed in Appendix A.5, 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 were 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, as mentioned previously, the size of any predicted relationship varies depending on a variety of unobservables (e.g., voting benefits and costs, aggregate uncertainty). While we cannot rule out that our treatments have any effect (e.g., we cannot reject that the close poll treatment in the 2014 experiment could boost 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.

Although we cannot measure the share of individuals reading the mailers in our 2014 experiment, our results suggest that a mailer "message" indicating the election is close is not an effective way of boosting turnout in large US elections, particularly so compared to various alternative mailer messages (e.g., emphasizing gratitude or social pressure) that have been shown to substantially boost turnout (Gerber and Green, 2016).

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.

Thus, it is quite possible that instrumental voting models may be highly relevant for other types of elections, even if our results suggest that such models may be less relevant for large US elections such as gubernatorial 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, parties 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 closeness.

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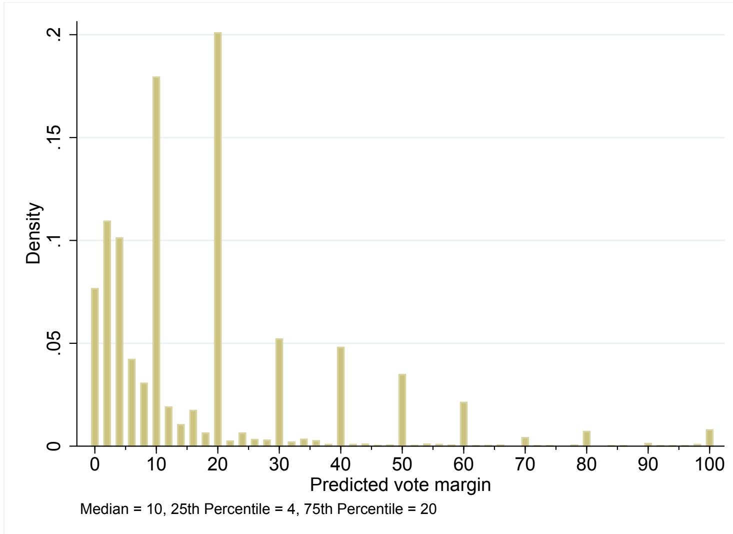
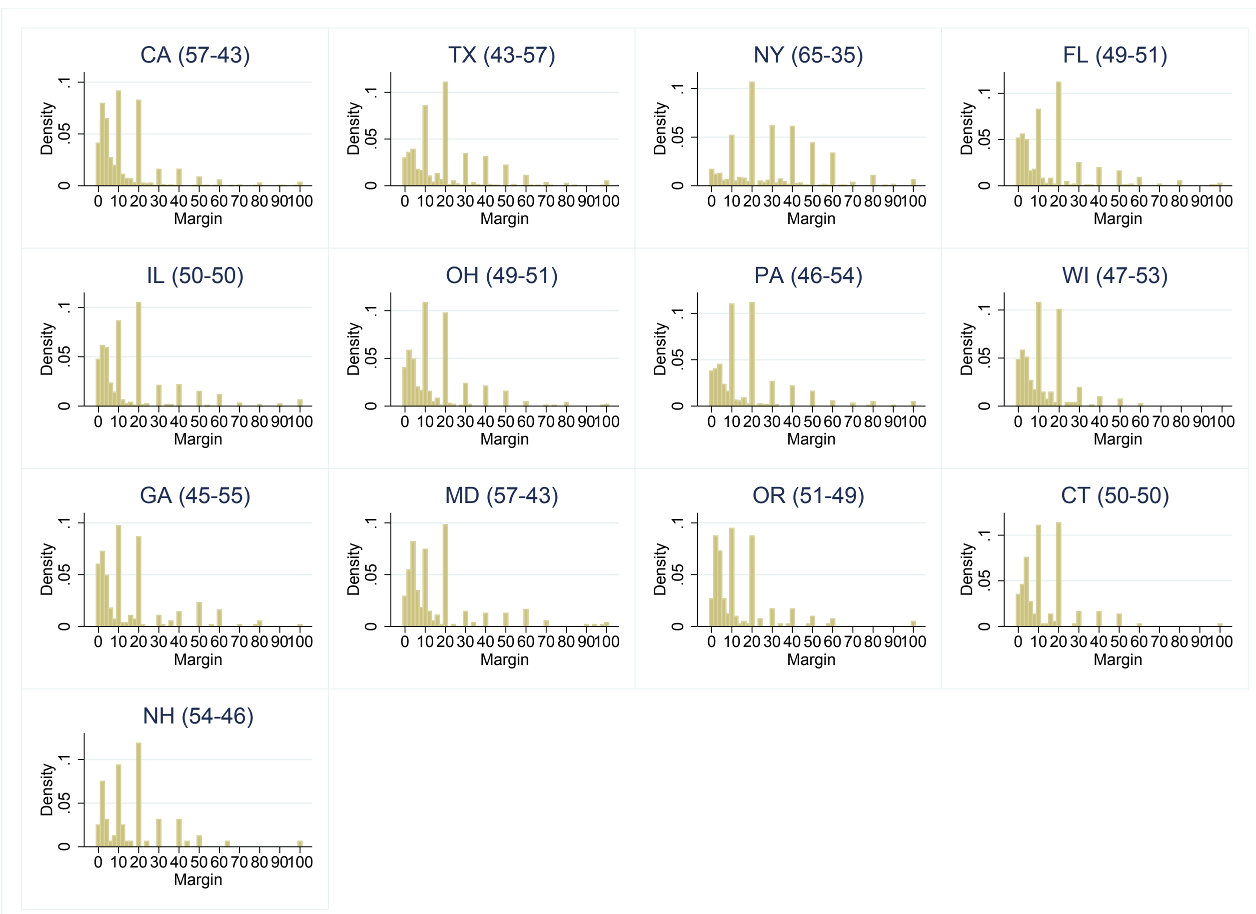
Figure 1: Distribution of Predicted Margin of Victory, Before Treatment (2010 experiment)



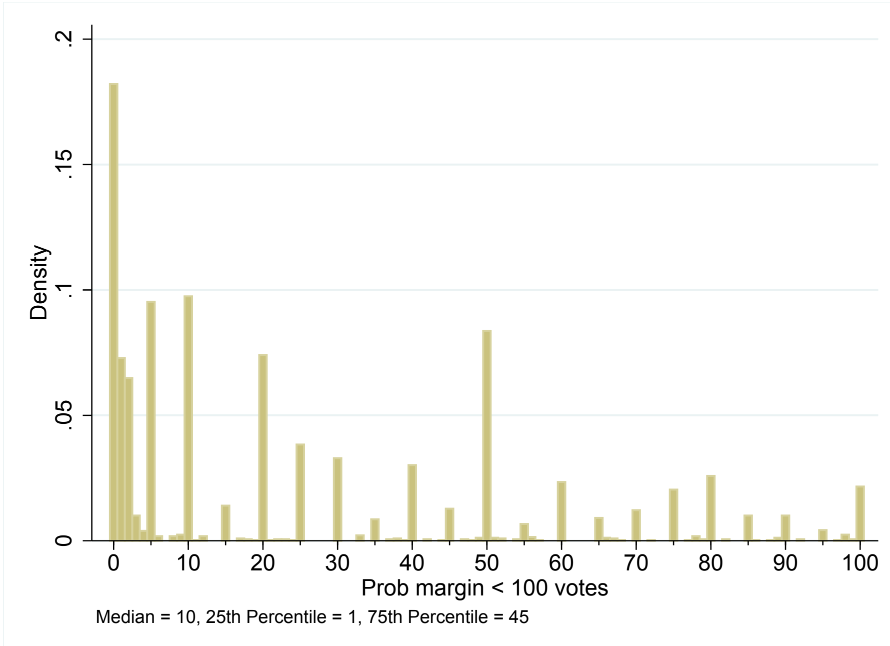
(a) Overall



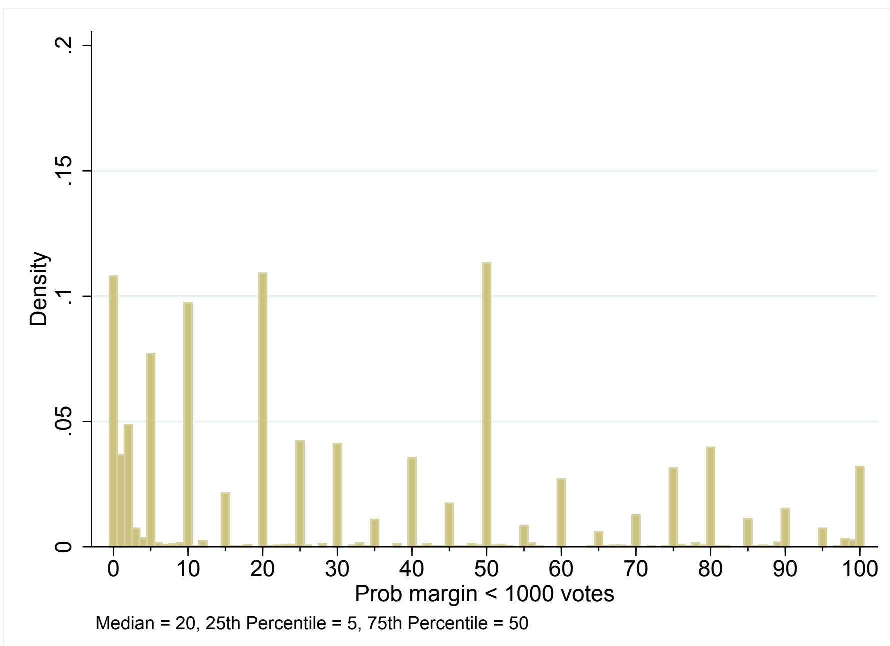
(b) Across States

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)



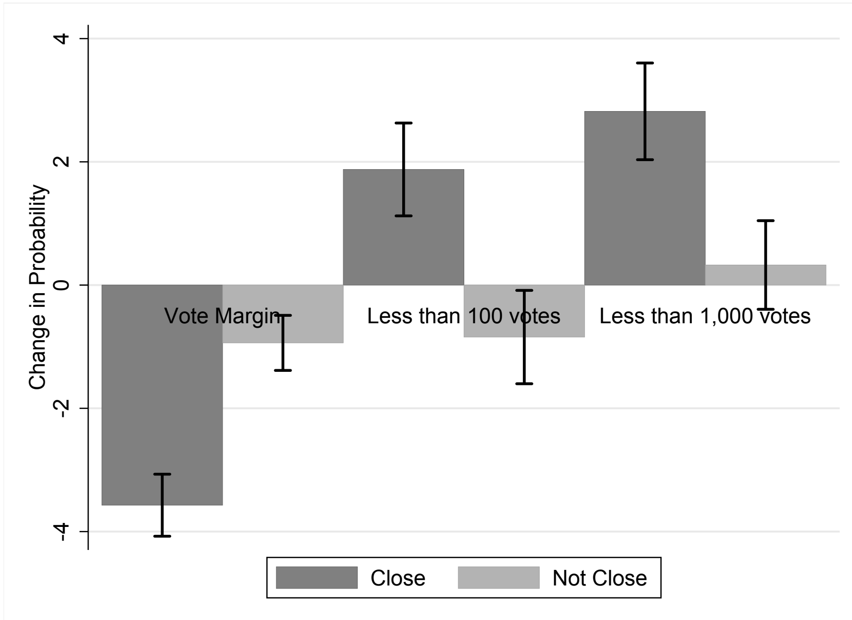
(a) Less than 100 Votes



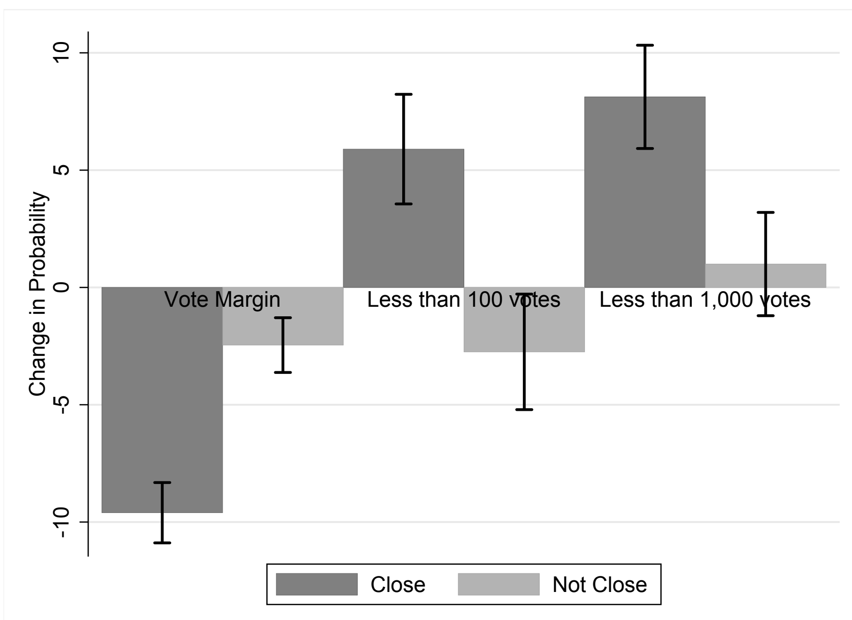
(b) Less than 1,000 Votes

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. For "interest in politics," 1 is "not interested at all" and 5 is "extremely interested."

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 C27. For results where we restrict to voters who update their beliefs, see Appendix Table C19. * 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)	(5)
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)	
Close poll X Small electorate					0.12
					(0.35)
Close poll X Large electorate					0.08
					(0.35)
Not close poll X Small electorate					-0.39
					(0.35)
F(Close vs. NotClose)			0.242		
Mean DV if not close poll=1	53.45	53.45		53.45	53.45
Mean DV if control=1			53.43		
Additional controls	No	Yes	Yes	Yes	Yes
Observations	126,126	126,126	1,385,318	126,126	126,126
Clusters (households)	79,551	79,551	875,476	79,551	79,551

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. In column 5, the excluded category is Not close poll X Large electorate. * 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 C28. 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.4 for further details.

"One in a Million: Field Experiments on Perceived Closeness of the Election and Voter Turnout": Online Appendix

Alan Gerber, Mitchell Hoffman, John Morgan, and Collin Raymond

The Online Appendix is organized as follows. Appendix A provides additional discussion related to Sections 4.3 and 6. Appendix B gives more details on the data. Appendix C provides additional figures and tables. Appendix D provides a formal model to accompany the discussion from Section 2. Appendix E provides documents used in the experiments.

A Additional Discussion

A.1 Discussion on IV Estimates (Section 4.3)

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 related forms or constructs of a person's underlying perception of election closeness. To the extent that the different closeness variables represent different underlying concepts, we show here 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 (under the assumption that the different closeness variables do not affect turnout in the unexpected direction, if they have any affect at all).[1]

To see this, consider an IV model of the form in Table 6:

$$\begin{aligned} T &= b_0 + b_1 x_1 + u \\ x_1 &= c_0 + c_1 z + \epsilon \end{aligned}$$

where T is a dummy for turnout; x_1 a person's predicted vote margin; x_2 is a person's subjective chance of the election being decided by less than 100 or 1,000 votes; u is an error; z is a dummy for receiving the close poll; and ϵ is an error. We assume that $u = b_2 x_2 + \tilde{u}$, where $cov(\tilde{u}, z) = 0$. We work with a simple bivariate model with no covariates, but the same intuition can also be extended to a model with covariates. We have that:

$$\begin{aligned} plim(\hat{b_1} - b_1) &= \frac{cov(z,u)}{cov(z,x_1)} = \frac{cov(z, b_2 x_2 + \tilde{u})}{cov(z,x_1)} \\ &= \frac{b_2 cov(z,x_2)}{cov(z,x_1)} = \frac{(+)*(+)}{(-)} = - \end{aligned}$$

In instrumental voting models, the impact of x_1 is negative (i.e., greater predicted vote margin leads to less turnout) and the impact of x_2 is positive (i.e., greater predicted probability of a

[1] For theoretical arguments in favor of the assumption that the different closeness variables would not affect turnout in the expected direction, see Propositions 3 and 4 in Appendix D.

very close election leads to more turnout). Above, we've shown that if the instrument affects both x_1 and x_2, and x_2 affects y in the expected direction, then the estimate of x_1 on y is biased downward, i.e., biased upward in magnitude, provided that x_1 affects y in the expected direction. Intuitively, suppose an instrument separately affects two endogenous variables. Then, if one runs an IV regression using one variable at a time, some of the impact of the second variable will be attributed to the first.[2]

Note also that $plim(\hat{b_1} - b_1) = 0$ if $b_2 = 0$. That is, if the perceived chance of a very close election has no impact on turnout, then running the IV analysis one regressor at a time yields no bias.

Last, it is unsurprising that the IV estimates are statistical 0's, given that the reduced form relationship between getting the close poll and turnout is also zero (Appendix Table C27).

A.2 Using Beliefs in Logs instead of Levels (Section 6.1)

Our results analyze beliefs in levels instead of logs, as this seemed the simplest way to proceed (particularly for the decomposition in Table 6.2). However, our IV results are robust to analyzing beliefs in logs instead of levels, which we believe is a useful robustness check, given the dispersion in stated beliefs. For example, Appendix Table C19 performs our IV analysis on an important subsample, namely, the set of people who update their beliefs after seeing the poll we provided. In Appendix Table C20, we re-do this analysis but using log(1+beliefs) instead of beliefs in levels. Based on the 95% CIs, decreasing the perceived margin by 10% (0.1 log points) increases turnout by no more than 0.79pp in column 3. In column 12, increasing the perceived chance of a very close election by 10% increases turnout also by no more than 0.79pp. Our main IV results in Table 6 are also robust to beliefs in logs.

A.3 Discussion on Are Belief Levels Sensible? (Section 6.1)

Further discussion on eliciting beliefs without incentives. As noted by footnote 15 in the main text, we did not use incentives for eliciting beliefs for two reasons: (i) Legal concerns about payments constituting gambling on elections or paying people to vote and (ii) Concerns that a quadratic scoring rule would be confusing for subjects of various ages and educational backgrounds. Still, some readers, particularly those from a lab experimental background, may be concerned about whether we successfully elicited beliefs. For example, the chapter of Laury and Holt (2008) in the *Handbook of Experimental Economics Results* provides examples of some situations where economic behavior by laboratory subjects is different based on whether financial incentives are used. For example, choices in risky gambles over large stakes seem to be affected by whether questions are hypothetical or not. However, Laury and Holt (2008) also acknowledge that there may be lab situations where using incentives may not matter.

We have four responses to this concern. First, our randomized treatment (close or not close polls) provides a natural way of addressing measurement/elicitation error, including potential error from not using incentives. Second, while not standard in lab experiments,

[2] Similarly, if we estimate an IV regression of T on x_2 while excluding x_1, $plim(\hat{b_2} - b_2) = \frac{b_1 cov(z,x_1)}{cov(z,x_2)} = \frac{(-)*(-)}{(+)} = +$ if b_1 is negative. That is, $\hat{b_2}$ would also be biased upward in magnitude.

eliciting beliefs without incentives is standard practice in most field data (Manski, 2004), including in leading studies published in top journals (Wiswall and Zafar, 2014; Delavande and Kohler, 2015; Kendall et al., 2015). Third, Hoffman and Burks (2017) randomized whether field beliefs were incentivized and found no impact. Fourth, as discussed in the main text, and as discussed further below, the belief data appear highly sensible in many ways.

Consistency of our beliefs data with evidence in behavioral economics. One way to examine whether beliefs are sensible is to examine whether subjects' beliefs are consistent with evidence and theory in behavioral economics. In fact, a long-line of papers in psychology and economics have documented (and modeled) individuals' over-estimation of small probabilities; the work of Kahneman and Tversky (1979) on prospect theory is a notable early effort. Probability over-weighting can help explain anomalies such as the Allais (1953) paradox. Recent work using at field data (e.g., Snowberg and Wolfers, 2010; Andrikogiannopoulou and Papakonstantinou, 2016; Chiappori et al., 2012; Gandhi and Serrano-Padial, 2014; Barseghyan et al., 2013) have, in line with our results, found evidence for overestimating events with negligible probabilities. In fact, our elicited probabilities regarding an almost zero-probability event—i.e., a "close election"— are roughly similar to estimates that Barseghyan et al. (2013) find in an entirely different environment. Structurally estimating a model of probability weighting using insurance choice data, Barseghyan et al. (2013) find that individuals act as if they place weights of approximately 6-8% on almost zero-probability events.

A tied election is an event that results from the combined actions of many thousands or millions of individuals. In fact, there is an extensive literature in both psychology and economics that discusses how individuals tend to overestimate unlikely events, particularly when samples are large. Benjamin et al. (2016), drawing on evidence such as Kahneman and Tversky (1972) and Benjamin et al. (2013), model how individuals tend to predict considerably greater dispersion of outcomes than that implied by the Law of Large Numbers, describing this as non-belief in the Law of Large Numbers (NBLLN).

To see whether this model can help explain our belief levels, we examined whether individuals with more NBLLN are more likely to over-estimate the probability of a close election. In particular, our coin experiment tests each individual's views about the aggregate result of a sample consisting of a large number (1,000) of coin flips. We suppose that individuals who exhibit greater NBLLN systematically over-estimate the probability of "extreme" samples with a large number of observations. In our case, with a fair coin, the probability of getting between 481 and 519 heads is 78% (Benjamin et al., 2013).[3] Given the high true probability of 481-519 heads, we conceptualize an extreme sample as one outside this range.

Consistent with (Benjamin et al., 2013), we find that subjects substantially underestimate the probability of 481-519. In our data, the average probability assigned to 481-519 heads was 44% instead of 78%. However, there is substantial heterogeneity and it is correlated with perceived chance of a very close election. Measuring NBLLN using the probability that a person puts outside of 481-519 heads, Table C4 shows that voters with greater NBLLN assign higher probability to the election being decided by less than 100 votes (column 3), less than 1,000 votes (column 5), or less than 100/1,000 votes. This holds controlling for education, income, and other controls. Thus, individuals who overestimate the probability of extreme

[3]Recall from Section 3 that subjects were asked to place subjective probabilities on the following 7 bins: 0-200 heads, 201-400 heads, 401-480 heads, 481-519 heads, 520-599 heads, 600-799 heads, 800-1,000 heads.

events in the coin-flipping domain, an easily understood stochastic process, tend to produce the highest estimates of a very close election.[4]

Time in belief questions. A further reason to take seriously the beliefs data is that most people took time to consider the belief questions (and did not answer overly quickly). We know this because we have each subject's time on each question throughout the survey. For the pre-treatment vote margin question, people took a median time of 35 seconds to answer the question (p10=19 seconds, p90=78 seconds). In addition, for the pre-treatment less than 100 or 1,000 votes question, people took a median of 16 seconds (p10=9 seconds, p90=36 seconds).

What if reported beliefs differ from true beliefs? While subject beliefs seem very sensible in the ways described above and are consistent with work in behavioral economics, it is worth considering how our results would be affected if stated beliefs differed from true underlying beliefs. If subjects exaggerated their beliefs about closeness by a fixed amount (e.g., they stated subjective probabilities by taking true probabilities and adding 20pp), this would have no impact on our results. However, our IV and OLS results on how closeness beliefs affect turnout would be biased downward if subjects exaggerated changes in beliefs. Still, even in this circumstance, our reduced form estimates would be unaffected, and our analysis would still be qualitatively valid. Furthermore, the analysis in Table 8 would be unaffected because exaggerations in belief change would show up positively in the reaction of believed closeness to actual closeness and inversely in our IV estimates. Thus, our evidence on the importance of perceived closeness for explaining the relationship between actual margin and turnout seems that it would not be directly affected by people exaggerating changes in their beliefs.

A.4 Additional Discussion on Section 6.2

Section 6.2 analyzes the importance of perceived closeness for the cross-state relationship between actual margin and voter turnout. Two key assumptions underlie the analysis in Section 6.2:

1. What measure of beliefs should we be using? And how can we combine together the estimates of s based on different belief measures?

2. What should be assumed about how beliefs were affected in the 2014 experiment?

Which measure of beliefs. It is not clear to us which measure of beliefs should be preferred (as perceived margin and the perceived probabilities of a very close election are related variables for how a voter might perceive closeness), but it seems like there are strong reasons for focusing on perceived chance of a margin of less than 100 or less than 1,000 votes. Consider a hypothetical experiment that randomized the actual margin in different states. We would like to know how much of the effect of actual closeness on turnout comes through the "true perceived closeness" channel versus elites responding. If the way that the perceived

[4]Interestingly, higher NBLLN is positively correlated with margin of victory. Thus, greater NBLLN only predicts higher perceived closeness for the belief variables associated with a very close election (instead of general electoral closeness).

closeness channel actually operates is by changing peoples perceived chance of an almost tie, then that would be a reason for using the perceived chance of margin less than 100 votes (or the less than 100/1,000 combined measure) as the main belief measure.

While there are strong reasons focusing on perceived chance of a very close race, a perhaps more disciplined approach (and one that uses all the data) is to combine the different estimates of s together. To do this, we weight the estimates of s according to the precision of their estimates.[5] Specifically, let $\hat{s}_{marg}$, $\hat{s}_{100}$, and $\hat{s}_{1{,}000}$ be our estimates of s based on the three belief measures predicted vote margin, Pr(Marg <100 votes), and Pr(Marg <1,000 votes), respectively. Then, our overall estimate of s is given by:

$$\hat{s}_{overall} = \frac{h_{marg}\hat{s}_{marg} + h_{100}\hat{s}_{100} + h_{1{,}000}\hat{s}_{1{,}000}}{h_{marg} + h_{100} + h_{1{,}000}}$$

where h_{marg}, h_{100}, and $h_{1{,}000}$ represent the precisions. To calculate a standard error for the overall estimate of s, we use the Delta Method, combined with the assumptions that $cov(\hat{s}_{marg}, \hat{s}_{100}) = cov(\hat{s}_{marg}, \hat{s}_{1{,}000}) = cov(\hat{s}_{100}, \hat{s}_{1{,}000}) = 0$, leading to:[6]

$$se(\hat{s}_{overall}) = \sqrt{\frac{1}{h_{marg} + h_{100} + h_{1{,}000}}}.$$

In forming our overall estimate of s, we choose to use the estimates of s based on the three belief measures of predicted vote margin, Pr(Marg <100 votes), and Pr(Marg <1,000 votes), as they are all based on separate data. An alternative approach is to use estimates of s based on only two belief measures, namely predicted vote margin the predicted of a margin of less than 100 or 1,000 votes. As seen in Appendix Table C31, combining these two measures leads to slightly less precision for the overall estimates than in Table 8, but precision is still very high: we can reject an s value of no more than 0.23 in our preferred pooled specification.

Assumption on belief impacts in 2014 experiment. It is also not obvious what differential impact on beliefs might arise from a postcard versus an online survey. Some people quickly throw out postcards (leading to smaller effects on beliefs), but a postcard is a more physical and tangible medium, potentially leading to larger effects. The 2014 study had similar wording to the 2010 study. The distance between close and not close polls was smaller in 2014 (potentially leading to smaller changes in beliefs), but we also had a greater share of close polls in 2014 that were 50/50 (potentially leading to larger changes in beliefs), as seen in Appendix Table C2.

One thing we can do is to ask how small would the effect on beliefs need to be for us not to be able to reject $s = 1$. For our preferred specification using the pooled data, the effects on beliefs would need to be about 8 times smaller to fail to reject $s = 1$. It seems very unlikely

[5] This approach parallels optimal GMM in the weights it assigns to each $\hat{s}$ (under the assumption that the moments based on the $\hat{s}$ values are uncorrelated with one another).

[6] Our conclusions are robust to relaxing the assumption of 0 covariance. For a general variance-covariance matrix, we have that $var(\hat{s}_{overall}) = \frac{1}{h_1+h_2+h_3} + \frac{2\sum_{i \neq j} \rho_{ij} h_i^{.5} h_j^{.5}}{(h_1+h_2+h_3)^2}$ by the Delta Method, where $\rho_{ij} = corr(\hat{s}_i, \hat{s}_j)$. Suppose that $\rho(\hat{s}_{marg}, \hat{s}_{100}) = \rho(\hat{s}_{marg}, \hat{s}_{1{,}000}) = \rho(\hat{s}_{100}, \hat{s}_{1{,}000}) = 0.5$. In this case, if we re-do the 95% confidence intervals for $\hat{s}_{overall}$, we obtain $[-0.40, 0.41]$ for 2010, $[-0.03, 0.15]$ for 2014, and $[-0.03, 0.14]$ for the pooled data.

to us that our 2014 postcard's effect on beliefs would be 8 times smaller than the effect of the 2010 online survey. If we assume that the impact on beliefs in the 2014 experiment was only half as large as in 2010, we obtain an estimate of $\hat{s}_{overall} = 0.11$, with a 95% confidence interval of $[-0.03, 0.25]$. This evidence indicates that our conclusions are qualitatively robust to more conservative assumptions about how beliefs were affected during the 2014 experiment.

Two-Sample IV (TSIV) estimation. For the 2014 data (as well as the pooled 2010/2014 data), we cannot run an IV regression of turnout on post-treatment beliefs, instrumenting with receiving the close poll treatment. Instead, in estimating s, we perform a reduced form regression of turnout on whether someone received the close poll treatment, and divide the estimate by a first stage estimate using the 2010 data. In the just identified case, the TSIV estimator is given by:

$$\hat{\theta}_{TSIV} = \frac{\hat{\theta}_R}{\hat{\theta}_F}$$

where $\hat{\theta}_R$ is the reduced form estimate and $\hat{\theta}_F$ is the first stage estimate. If we assume that $cov\left(\hat{\theta}_R, \hat{\theta}_F\right) = 0$ (which we think is particularly reasonable when the reduced form and first stage are from separate samples), then by the Delta Method, it can be shown that:

$$se\left(\hat{\theta}_{TSIV}\right) = \frac{1}{\hat{\theta}_F}\sqrt{var\left(\hat{\theta}_R\right) + \frac{\hat{\theta}_R^2}{\hat{\theta}_F^2} var\left(\hat{\theta}_F\right)}$$

We use this formula for calculating TSIV standard errors. Note that if there is no first stage estimation error (i.e., $var\left(\hat{\theta}_F\right) = 0$), then we have that $se\left(\hat{\theta}_{TSIV}\right) = \frac{se\left(\hat{\theta}_R\right)}{\hat{\theta}_F}$.

Note that it is not possible for us to include the same control variables for the first-stage (from 2010 experiment) and reduced-form (from 2014 experiment). The two experiments are based on different states, so the state effects would be different. Furthermore, our past voting controls are for 2000, 2002, 2004, 2006, and 2008 for the 2010 experiment, whereas the past voting controls are for 2008, 2010, and 2012 for the 2014 experiment.

Two sample IV requires that both samples are drawn from the same overall population. While there are some differences between the 2010 and 2014 populations in observable demographics (compare Tables 1 and C11), the differences are relatively small. As discussed in Section 5, one noticeable difference between the 2010 and 2014 experiments is the voting rate, where the rate was 72% in 2010 and 53% in 2014. As argued in footnote 33, this seems likely due to the internet sample having a relatively high voting rate. Still, we believe that the 2010 and 2014 populations are broadly similar.

Another way of evidencing that the 2010 and 2014 samples are broadly from the same overall population is to compare the reduced form estimates. As noted in Section 5 of the paper, the reduced form estimates are quite similar. With full controls, the estimate is 0.29 for 2014 (Table 7) compared to 0.23 for 2010 (Table C27).[7]

Standard errors for s. In Table 8, the column 5 confidence intervals for s include estimation error from our main IV estimation (as well as from first stage estimation error for

[7]This test is not possible in most instances of TSIV. However, the 2010 data includes the outcome, the endogenous regressor, and the instrument (instead of just the endogenous regressor and the instrument).

Panels B and C), but ignore estimation error in estimating how perceived closeness responds to actual closeness and in how turnout responds to actual closeness. We do this to focus on understanding the precision of our experimental estimates (as opposed to combining the precision of our experimental and non-experimental estimates).

A.5 Additional Discussion on Bandwagon Effects (Section 6.3)

Bandwagon effects could stem from multiple sources. First, individuals may simply prefer to conform to the actions of others (Callander, 2007; Hung and Plott, 2001; Goeree and Yariv, 2015) either due to intrinsic preferences for conformity, or a sense of duty. Thus, individuals receive a payoff not just from having their favored candidate win, but also from voting in a way that conforms to the median voter. A second potential mechanism is the strategic considerations at play when there is a common values component to the candidate qualities, as discussed in Section 2. If the conditions outlined in that section fail to hold in the common values setting, then Prediction 1 is no longer valid.[8] However, as summarized by Prediction A1 in Appendix D, if we look at the set of individuals whose beliefs do not shift with the poll results, then we would still expect Prediction 1 to hold on this sub-sample. A third mechanism is the signaling motivations also discussed in Section 2. Even if the signaling value is of a vote (or abstention) is higher with a more extreme electoral outcome (and so Prediction 1 will not hold) we can still test if different polls induced voters to send different signals, and so whether Prediction A2 in Appendix D holds.

Table C29 investigates these effects. In the first stage, column 1 shows that the randomly assigned poll-shown Democrat vote share causes an increase in a person's predicted Democratic vote share, which is unsurprising given the earlier evidence that people update beliefs. For every 1pp of the Democrat being ahead in the poll shown, people update 0.27pp in their belief. In columns 2-5, we examine the relation between a person believing the Democrat is ahead and their likelihood of voting Democrat.[9] The OLS result in column 2 suggests a positive relation, with a 1pp increase in Democrat vote share associated with a 0.16pp higher chance of voting Democrat. In the IV results in columns 3-5, there is no statistically significant relation (though standard errors are larger). The OLS estimates may be biased by a number of factors, including unobserved variables (e.g., whether a person watches Fox News could affect how they vote (DellaVigna and Kaplan, 2007) and their perception of who's ahead), self-justifying beliefs (i.e., deciding to vote Democrat for another reason and then justifying the belief to themselves that the candidate is popular), and measurement error in beliefs.[10]

[8] As discussed in Appendix D, in this case, observing a poll that informs an individual that candidate A is very likely to win reduces the probability of being decisive, but increases the payoff from voting for A. Therefore, the reduction in pivotality may cancel out (or even dominate) in the computation of the benefits of voting with the increase in the payoff differential between voting for A and voting for B.

[9] It is worth re-iterating that information about *for whom* a person voted is self-reported. While we have limited reason to think that people would misreport for whom they voted (in contrast to a likely social desirability bias of saying whether a person voted), some readers may wish to view these results here as less definitive (given that they are not based on administrative data like our main results).

[10] Appendix Table C30 shows that poll-shown Democrat vote share does lead individuals to express a greater intention of voting Democrat in our IV regression. We think that greater attention should be paid to the behavior of voting Democrat as opposed to a mere intention, as it is the behavior which is most consequential. Still, studying intentions may still be useful for us in the event that the poll information we showed was overcome by another source of information. Combining the positive insignificant impact of Democrat vote

As discussed earlier, some theories of voting (such as common value instrumental models) predict that increased closeness beliefs should increase turnout conditional on people not changing their preferences. Thus, besides testing whether people's preferences were affected, we can also restrict to the sample of people whose preferences did not change. As seen in Appendix Table C25, our main IV results are qualitatively robust to restricting to this sample.[11]

Further Comparison of Our Results to the Literature. As noted in footnote 5 in the main text, the earlier field experiment of Ansolabehere and Iyengar (1994) found evidence of bandwagon effects as a result of randomly assigning one of two polls to around 400 voters. Given that we fail to find causal evidence of bandwagon effects with respect to actual voting, why might our results differ? One possibility is that Ansolabehere and Iyengar (1994) analyze intended vote choice, whereas we analyze actual (self-reported) vote choice. Indeed, as noted in footnote 10 in the Appendix, we do find bandwagon effects with respect to intended Democrat vote share. A prominent more recent paper finding evidence of bandwagon effects is Knight and Schiff (2010), who use a structural approach to find strong evidence of bandwagon effects in presidential primaries. One possibility for difference in results concerns primary vs. general elections. In primary elections, one is comparing among options within one's party. Because the ideological differences among candidates is presumably smaller than in a general election, voters may be more susceptible to social influences.

B Data Appendix

B.1 2010 Experiment

Beyond the restrictions mentioned in the text, subjects for the 2010 study were required to be English-language survey takers, and only one participant per household (thereby avoiding situations where there are multiple Knowledge Panel respondents in a household).

The randomization for the 2010 experiment was carried out by the statistics team at Knowledge Networks, the firm administering the experiment. Knowledge Networks conducted the randomization (as opposed to the researchers) to protect the confidential information of subjects. The randomization was conducted in SAS by sorting individuals by state, education, whether the person voted in the 2008 general election (self-reported), gender, race (white, black, hispanic, other, or 2+ race), age (breaking age into 4 categories: 18-29, 30-44, 45-59, 60+), and a random number.[12] After sorting, individuals were given a number "count" corresponding to their row number (i.e., a person in the 7th row was given the number 7).

beliefs on actual voting Democrat, combined with a positive significant impact on intention to vote Democrat, we would interpret the results as limited or inconclusive support for bandwagon effects.

[11] Further corroborating evidence is also provided by an earlier considered robustness check, where we re-did our main IV results restricting to voters with a strong ideology (Table C21). Such voters seem more likely to view voting as a private values endeavor than non-ideological voters.

[12] More precisely, the 5 race categories were: "white, non-hispanic," "black, non-hispanic," "other, non-hispanic", "hispanic", and "2+ races, non-hispanic." The education categories were: "1st, 2nd, 3rd, or 4th grade," "5th or 6th grade," "7th or 8th grade," "9th grade," "10th grade," "11th grade," "12th grade no diploma," "high school graduate - high school dipl," "some college, no degree," "associate degree," "bachelors degree," "masters degree," and "professional or doctorate degree." Over 97% of individuals who responded to our survey have "high school graduate - high school dipl" or above.

People with mod("count",3)=0 were assigned to Close Poll. People with mod("count",3)=1 were assigned to Not Close Poll. People with mod("count",3)=2 were assigned to Control. The sample was selected in the week of October 11, 2010 and assigned in the week of October 18, 2010.

A common approach in voting experiments (as well as field experiments in general) is to control for randomization strata (e.g., Pons, 2016). In our case, there are many small strata, such that controlling for every single strata strains the regression. However, we gradually add control variables. In our full specifications in columns 3, 6, 9, and 12 of Table 6 in the main text, we control for state, education, gender, race, and age. We also control for actual voting in 2008 instead of self-reported voting. Thus, we are (approximately) controlling for all the stratification variables (even though we do not include fixed effects for every strata).

As mentioned in footnote 22 in the main text, our past voting controls measure whether a person voted in past general elections in 2000, 2002, 2004, 2006, and 2008. However, young voters in 2010 may not have been eligible to vote in some of these past elections. This is not driving our results because the results are qualitatively similar (though less precise) without past voting controls. We have also repeated 6 while additionally including a control for being age 27 or younger, and the results were very similar.

Our analysis of the experiment is focused on comparing individuals receiving either the Close Poll or Not Close Poll treatments. In addition, there are individuals who were assigned to the Close or Not Close treatments (but who didn't respond to our survey), as well as individuals assigned to Control (who received no survey from us). Though we have fewer variables covering all 3 groups (the 3 groups being assigned to Close, assigned to Not Close, and Control), we also made summary statistics comparing across the 3 groups. Those assigned to the Close and Not Close treatments are well balanced. Among the 3 groups, the Control condition had a lower voting rate in the past 5 elections than those assigned to the Close or Not Close groups, as well as a slightly higher chance of being registered Democrats instead of Republicans.[13] On further investigation, we discovered that this was entirely driven by the state of California. Removing California, the 3 groups are well balanced. In Appendix Table C27, the only table that uses the Control individuals, we address the imbalance by controlling for past voting rate. Our main 2010 results are also qualitatively similar to removing California.

In terms of timing, we were informed by Knowledge Networks that the pre-election survey was being launched shortly before 9pm on Tuesday, October 19th, 2010. However, the first responses in our data are time stamped as occurring shortly after midnight on Wednesday, Oct. 20th, 2010. We believe that this includes people who took the survey after midnight on the East Coast, as well as those who took it before midnight in the Central and Pacific time zones.

There is very little item non-response to the election closeness belief questions, and whether post-treatment beliefs are missing is uncorrelated with treatment status. This holds also conditional on pre-treatment beliefs being non-missing. Thus, there is no concern about differential attrition during the experiment.

[13] The randomization was performed by Knowledge Networks before these variables were obtained from the vote validation company.

B.2 2014 Experiment

As mentioned in footnote 31 in the main text, the anonymous vote validation company imposed a number of sample restrictions to create the voter lists for the experiment. These were:

- Is not a bad address (defined by USPS delivery point codes)
- Is not a foreign mailing address
- Is not considered undeliverable (again defined by USPS codes)
- Is not an out-of-state mailing address
- Is not a permanent absentee voter
- Is not deceased
- Has not had an NCOA flag applied
- Age is between 18 and 90
- Has not yet requested a ballot in the 2014 election
- Has not yet voted in the 2014 election

The data from the 2014 experiment were merged to voting records with the assistance of the anonymous vote validation company. To ensure the quality of the merge, we require a match in exact date of birth between individuals in the initial data set and individuals in the voting records. Doing this excludes 2.0% of the individuals in our data.

Selection of 2014 Polls. As mentioned in the main text, poll information was obtained from RealClearPolitics.com (whereas in 2010, we had poll data both from RealClearPolitics.com and FiveThirtyEight.com). When we looked at the FiveThirtyEight website in 2014, the website appeared to have been re-vamped and did not seem to provide the same easy-to-access gubernatorial polls.

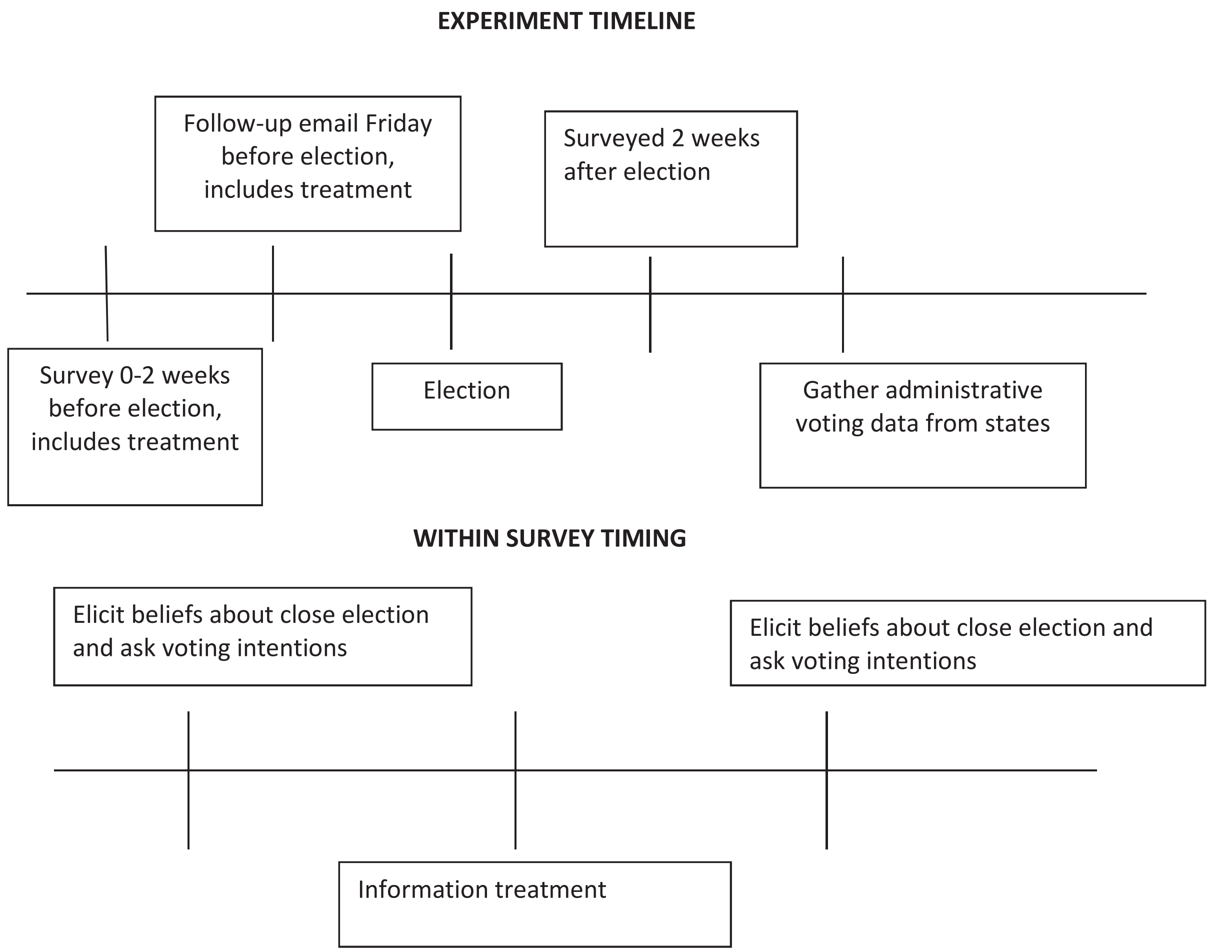
As described in the main text, in choosing polls, we first selected the most close and least close polls within the last 30 days. Because Fox News is often considered a contentious news source, we limited ourselves to non-Fox News polls (this caused us to exclude only two polls). The polls are a collection of polls conducted by national organizations (e.g., CBS News) and local news organizations (e.g., a local television station). In the event of a tie, we chose polls to promote congruence regarding whether both polls were from national organizations or from local organizations. In the further event of a tie, we chose the more recent poll.

B.3 Additional Data

Historical Data. Section 4.1 discusses data on historical gubernatorial elections in the US. These data were kindly provided by James Snyder in Sept. 2010. After some light data cleaning, we are left with a sample of 835 contested gubernatorial general elections in 1950-2009.

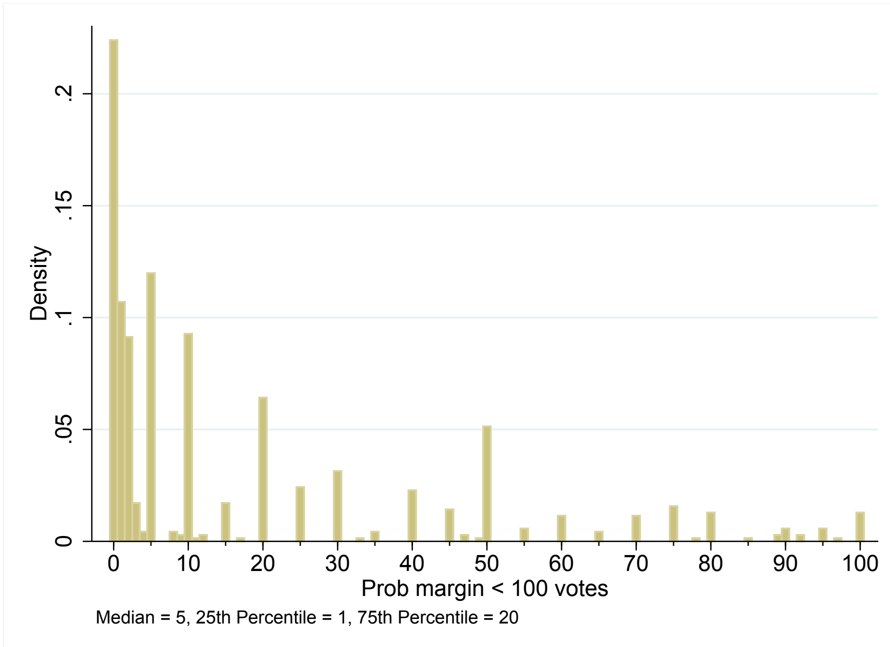
C Additional Figures and Tables

Figure C1: Timeline for the 2010 Experiment

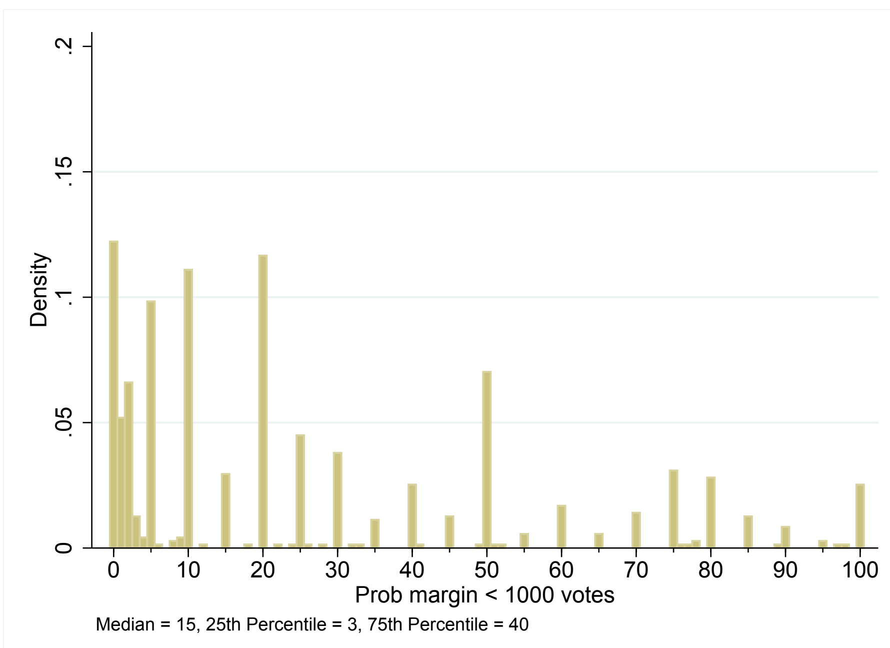


Notes: This is a timeline for the 2010 experiment.

Figure C2: Subjective Probabilities that Gubernatorial Election will be Decided by Less than 100 Votes or 1,000 Votes–Voters with Master's or PhD (2010 Experiment)



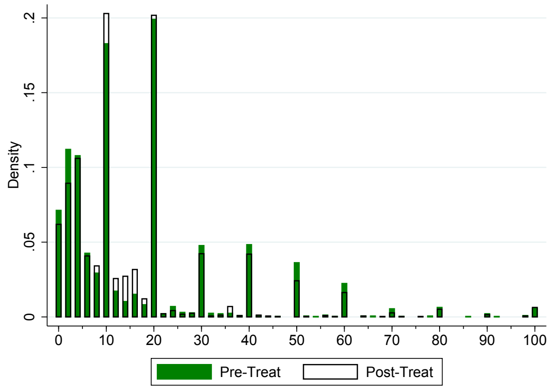
(a) Less than 100 Votes



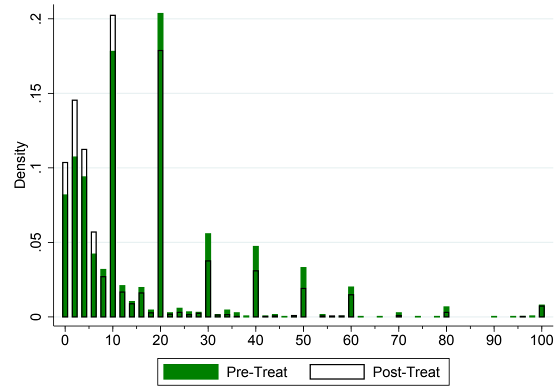
(b) Less than 1,000 Votes

Notes: This is a robustness check to Figure 2 in the main text. The difference is we restrict to voters with an education level of master's or PhD.

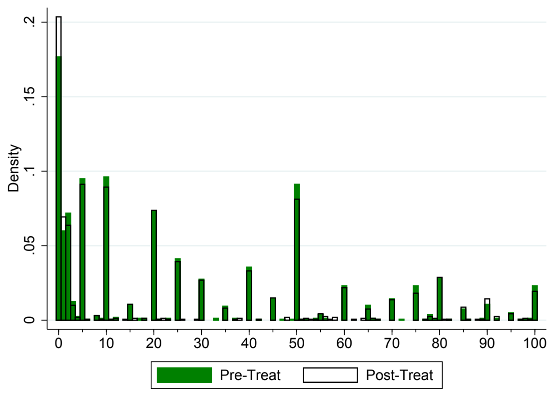
Figure C3: Distribution of Closeness Beliefs Before and After the Close and Not Close Treatments (2010 Experiment)



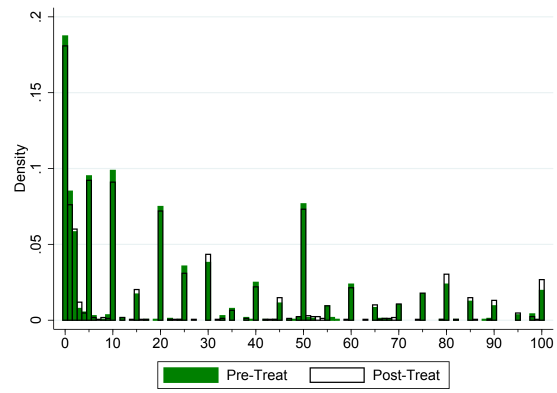
(a) Predicted Margin, Not Close Poll



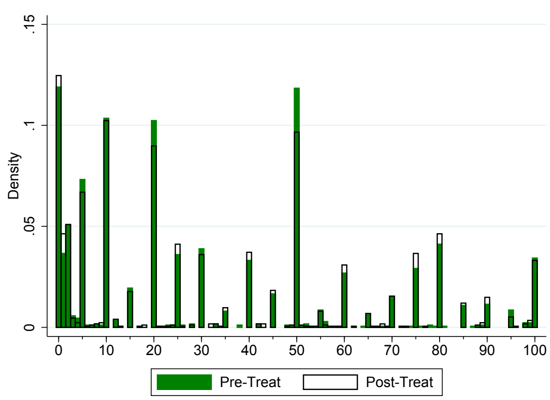
(b) Predicted Margin, Close Poll



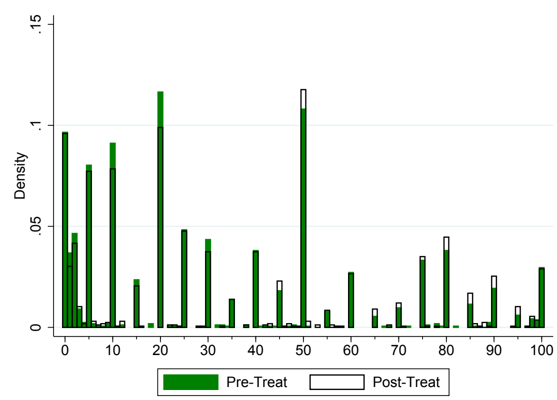
(c) Probability of margin less than 100 votes, Not Close Poll



(d) Probability of margin less than 100 votes, Close Poll



(e) Probability of margin less than 1,000 votes, Not Close Poll



(f) Probability of margin less than 1,000 votes, Close Poll

Notes: These graphs analyze the distribution of subjective electoral closeness beliefs. It shows them before and after the two treatments (not close poll and close poll). Increases in post-treatment beliefs (relative to pre-treatment beliefs) can be found by looking for white bar space in the graphs. For example, for probability of margin less than 100 votes, there was an increase in the number of responses of "0" post-treatment relative to pre-treatment. We restrict to individuals for whom the pre-treatment and post-treatment belief is non-missing.

Table C1: Selected Papers using Instrumental Voting Models (2000-2015)

Journal	Article Name	Authors	Year
AER	Information aggregation and strategic abstention...	M Battaglini, RB Morton, TR Palfrey	2008
AER	Costly voting	T Borgers	2004
AER	Information aggregation in standing and ad hoc committees	SN Ali, JK Goeree, N Kartik, TR Palfrey	2008
AER	Decision making in committees: Transparency...	G Levy	2007
AER	Legislative bargaining under weighted voting	JM Snyder, MM Ting	2005
AER	Two-class voting: a mechanism for conflict resolution	E Maug, B Yilmaz	2002
AER	Self-enforcing voting in international organizations	G Maggi, M Morelli	2006
AER	Inferring strategic voting	K Kawai, Y Watanabe	2013
AER	A theory of strategic voting in runoff elections	L Bouton	2013
AER	Decision-making procedures for committees of careerist experts	G Levy	2007
AER	The value of information in the court: Get it right...	M Iaryczower, M Shum	2012
AER	Choice shifts in groups: A decision-theoretic basis	K Eliaz, D Ray, R Razin	2006
AER	Consensus building: how to persuade a group	B Caillaud, J Tirole	2007
AER	International unions	A Alesina, I Angeloni, F Etro	2005
ECMA	The power of the last word in legislative policy making	BD Bernheim, A Rangel, L Rayo	2006
ECMA	Combinatorial voting	DS Ahn, S Oliveros	2012
ECMA	Learning while voting: Determinants of collective...	B Strulovici	2010
ECMA	An experimental study of collective deliberation	JK Goeree, L Yariv	2011
ECMA	Preference monotonicity and information aggregation...	S Bhattacharya	2013
ECMA	One person, many votes: Divided majority...	L Bouton, M Castanheira	2012
ECMA	Choosing choices: Agenda selection with uncertain issues	R Godefroy, E Perez-Richet	2013
ECMA	Signaling and election motivations in a voting model...	R Razin	2003
JPE	Overcoming ideological bias in elections	V Krishna, J Morgan	2011
JPE	Sequential voting procedures in symmetric binary elections	E Dekel, M Piccione	2000
JPE	Mixed motives and the optimal size of voting bodies	J Morgan, F Vardy	2012
JPE	Bargaining and majority rules: A collective search perspective	O Compte, P Jehiel	2010
JPE	Cost benefit analyses versus referenda	MJ Osborne and MA Turner	2010
JPE	Delegating decisions to experts	H Li, W Suen	2004
QJE	Strategic extremism: Why Republicans and Democrats divide...	EL Glaeser, GAM Ponzetto, JM Shapiro	2005
QJE	On committees of experts	B Visser, O Swank	2007
QJE	Elections, governments, and parliaments...	DP Baron, D Diermeier	2001
ReStud	Aggregating information by voting...	JC McMurray	2012
ReStud	Voting as communicating	T Piketty	2000
ReStud	The swing voter's curse in the laboratory	M Battaglini, RB Morton	2010
ReStud	On the theory of strategic voting	D Myatt	2007
ReStud	Committee design with endogenous information	N Persico	2004
ReStud	Strategic voting over strategic proposals	P Bond, H Eraslan	2010
ReStud	Bandwagons and momentum in sequential voting	S Callander	2007
ReStud	Coalition formation in non-democracies	D Acemoglu, G Egorov, K Sonin	2008
ReStud	On the faustian dynamics of policy and political power	JH Bai and G Lagunoff	2011
ReStud	Bargaining in standing committees with an endogenous default	V Anesi, DJ Seidmann	2015

Notes: The table lists selected papers using instrumental voting models. "AER" is *American Economic Review*, "ECMA" is *Econometrica*, "JPE" is *Journal of Political Economy*, "QJE" is *Quarterly Journal of Economics*, and "ReStud" is *Review of Economic Studies*.

Table C2: Experimental Information Provided: Close and Not-close Poll Figures, as well as Small and Large Electorate Numbers, by State

Panel A: Provided polls and poll averages in 2010 expt

State	Close poll		Not-close poll		Average poll	
	Dem. Share	Rep. Share	Dem. Share	Rep. Share	Dem. Share	Rep. Share
CA	50%	50%	57%	43%	52%	48%
CT	52%	48%	57%	43%	54%	46%
FL	51%	49%	54%	46%	50%	50%
GA	50%	50%	44%	56%	46%	54%
IL	50%	50%	43%	57%	47%	53%
MD	52%	48%	58%	42%	55%	45%
NH	51%	49%	60%	40%	55%	45%
NY	53%	47%	68%	32%	62%	38%
OH	49%	51%	41%	59%	48%	52%
OR	51%	49%	47%	53%	50%	50%
PA	49%	51%	42%	58%	45%	55%
TX	47%	53%	42%	58%	45%	55%
WI	49%	51%	44%	56%	46%	54%

Panel B: Provided polls and poll averages in 2014 expt

State	Close poll		Not-close poll		Average poll	
	Dem. Share	Rep. Share	Dem. Share	Rep. Share	Dem. Share	Rep. Share
AR	49%	51%	44%	56%	46%	54%
FL	50%	50%	53%	47%	51%	49%
GA	50%	50%	47%	53%	49%	51%
KS	50%	50%	53%	47%	51%	49%
MA	50%	50%	46%	54%	49%	51%
MI	50%	50%	45%	55%	48%	52%
WI	50%	50%	47%	53%	49%	51%

Panel C: Provided electorate size predictions in 2014 expt

State	Small electorate	Large electorate
AR	800,000	1,000,000
FL	6,000,000	7,700,000
GA	2,900,000	3,800,000
KS	1,100,000	1,200,000
MA	2,100,000	2,900,000
MI	3,900,000	4,800,000
WI	2,000,000	2,400,000

Notes: Panels A-B lists the polls that were used in the 2010 and 2014 experiments (as well as the poll averages at the time of the experiment). For example, for California in the 2010 experiment, the close poll was "50-50," whereas the not close poll was 57% Democrat vs. 43% Republican. For the 2010 poll averages, we report the average of state polls during Sept. 10-Oct. 20. For the 2014 poll averages, we report the average of state polls during Sep 18 - Oct 18. These dates roughly correspond to the periods over which we searched polls to select "close" and "not-close" polls. The sample over which the poll averages are calculated may not correspond exactly to the sample from which polls were selected for the experiment, as the averages taken here are based on poll lists collected after the elections. Panel C lists the predicted electorate sizes that were provided in the 2014 experiment. As mentioned in footnote 28 in Section 5 of the main text, these are based on the predictions of 7 election experts. The numbers here represent the most extreme predictions.

For the 2014 experiment (but not for the 2010 experiment), we provided the source of the polls along with the numbers. For AR, the close and not close polls were from Rasmussen Reports and CBS News/NYT/YouGov, respectively. For FL, from TB Times/Bay News 9/News 13/UF and UNF. For GA, from SurveyUSA and Rasmussen Reports. For KS, from CNN Opinion Research and SurveyUSA. For MA, from Boston Globe and WGBH/Emerson. For MI, from WeAskAmerica and Detroit News. For WI, from Marquette University and Marquette University (i.e., from polls administered by Marquette University on different dates). In all cases, the source of the close poll is listed first, followed by the source of the not close poll.

Table C3: Summary Statistics for 2010 Experiment

Variable	Mean	Std. Dev.	Min.	Max.	N
Panel A: Demographics					
Male	0.39	0.49	0	1	6705
Black	0.08	0.27	0	1	6705
Hispanic	0.06	0.24	0	1	6705
Other	0.03	0.18	0	1	6705
Mixed race	0.02	0.15	0	1	6705
Age	53.33	14.2	18	93	6705
Less than high school	0.03	0.16	0	1	6705
High school degree	0.13	0.34	0	1	6705
Some college or associate degree	0.34	0.47	0	1	6705
Bachelor's degree	0.29	0.45	0	1	6705
Master's or PhD	0.21	0.41	0	1	6705
Household income 25k-50k	0.23	0.42	0	1	6705
Household income 50k-75k	0.23	0.42	0	1	6705
Household income 75k-100k	0.18	0.38	0	1	6705
Household income 100k +	0.24	0.43	0	1	6705
Panel B: Politics					
Registered Democrat	0.48	0.5	0	1	3823
Registered Republican	0.36	0.48	0	1	3823
No party affil/decline to state/indep	0.14	0.34	0	1	3823
Other party registration	0.02	0.16	0	1	3823
Identify Nancy Pelosi as Speaker	0.82	0.38	0	1	6595
Interest in politics (1-5 scale)	3.71	1.06	1	5	6684
Affiliate w/ Democrat party (1-7)	4.24	2.14	1	7	6673
Ideology (1=Extremely Conserv, 7=Extremely Liberal)	3.88	1.51	1	7	6624
Panel C: Beliefs					
Pred vote margin, pre-treat	17.08	17.78	0	100	6652
Pred vote margin, post-treat	14.76	15.83	0	100	6650
Pr(Marg $<$ 100 votes), pre	24.42	28.3	0	100	3284
Pr(Marg $<$ 100 votes), post	24.95	28.97	0	100	3286
Pr(Marg $<$ 1,000 votes), pre	31.69	29.7	0	100	3409
Pr(Marg $<$ 1,000 votes), post	33.22	30.51	0	100	3407
Prob voting, pre-treatment	87.06	27.79	0	100	6698
Prob voting, post-treatment	87.91	27.08	0	100	6700
Prob vote Dem, pre-treatment	49.94	43.77	0	100	6705
Prob vote Dem, post-treatment	50.14	43.68	0	100	6705
Prob vote Republican, pre-treatment	41.5	43.08	0	100	6705
Prob vote Republican, post-treatment	41.72	43.03	0	100	6705
Panel D: Voting					
Voted (self-reported)	0.84	0.36	0	1	5867
Voted (administrative)	0.72	0.45	0	1	6705
Share voted previous 5 elections (administrative)	0.65	0.37	0	1	6705

Notes: This table presents summary statistics. The sample is the 6,705 individuals who who completed the 2010 pre-election survey. "Share voted previous 5 elections" refers to the share of time a person is recorded as voting in the general elections of 2000, 2002, 2004, 2006, and 2008.

Table C4: Predicting Pre-treatment Beliefs (2010 Experiment)

Dep. var.:	Margin of victory		Prob < 100 votes		Prob < 1,000 votes		Prob < 100 or 1,000 votes	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Actual vote margin in state	0.48*		-0.14		-0.41**		-0.28**	
	(0.25)		(0.11)		(0.20)		(0.13)	
Subj prob that number of heads in 1000 flips would be outside of 481-519 (measure of NBLLN)	0.04***	0.04***	0.08***	0.08***	0.04**	0.04**	0.06***	0.06***
	(0.003)	(0.003)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Log size of electorate	-1.78		-0.54		0.26		-0.13	
	(2.40)		(1.34)		(2.00)		(1.27)	
Affiliate w/ Democrat party (1-7)	-0.18	-0.11	0.18	0.16	0.65	0.60	0.38	0.33
	(0.23)	(0.24)	(0.29)	(0.30)	(0.54)	(0.54)	(0.35)	(0.36)
Interest in politics (1-5 scale)	-0.05	-0.01	-1.46***	-1.50***	-0.35	-0.33	-0.96**	-0.97***
	(0.25)	(0.24)	(0.33)	(0.33)	(0.63)	(0.60)	(0.37)	(0.38)
Male	-2.91***	-2.89***	-11.38***	-11.36***	-14.26***	-14.31***	-12.90***	-12.93***
	(0.29)	(0.30)	(0.87)	(0.89)	(1.46)	(1.48)	(1.01)	(1.02)
Black	4.23***	4.46***	14.58***	14.45***	3.72**	3.27**	9.23***	9.10***
	(1.42)	(1.17)	(2.15)	(2.22)	(1.81)	(1.63)	(1.64)	(1.65)
Hispanic	2.09*	2.05*	10.17***	9.71***	6.69**	6.84**	8.62***	8.54***
	(1.08)	(1.10)	(3.27)	(3.26)	(2.95)	(2.84)	(2.40)	(2.40)
Other	0.73	1.57	8.29**	7.94**	0.56	0.36	4.39*	4.09*
	(1.56)	(1.52)	(3.44)	(3.42)	(2.28)	(2.10)	(2.40)	(2.30)
Mixed race	0.16	0.47	6.21	6.60	1.16	0.76	3.79	3.91
	(1.15)	(1.17)	(4.29)	(4.30)	(4.17)	(4.09)	(2.88)	(2.93)
Age 25-34	-4.30*	-4.60*	4.15	4.29*	-0.35	-0.24	1.66	1.84
	(2.52)	(2.46)	(2.57)	(2.58)	(4.80)	(4.79)	(2.83)	(2.80)
Age 35-44	-4.86*	-5.06**	2.33	2.43	1.93	2.18	1.67	1.79
	(2.57)	(2.50)	(2.62)	(2.66)	(3.66)	(3.63)	(2.30)	(2.23)
Age 45-54	-5.01*	-5.26**	3.22	3.26	-0.16	0.05	0.99	1.13
	(2.59)	(2.53)	(2.97)	(3.01)	(3.67)	(3.65)	(2.54)	(2.52)
Age 55-64	-6.28**	-6.71***	2.26	2.32	0.97	1.35	1.35	1.56
	(2.59)	(2.49)	(2.25)	(2.27)	(3.28)	(3.30)	(1.76)	(1.71)
Age 65-74	-7.83***	-8.05***	1.20	1.02	-0.23	-0.09	0.25	0.29
	(2.81)	(2.67)	(2.28)	(2.32)	(3.64)	(3.70)	(2.10)	(2.07)
Age 75 or more	-9.06***	-9.43***	8.10**	7.96**	2.40	2.90	5.26**	5.42***
	(2.78)	(2.61)	(3.44)	(3.50)	(2.62)	(2.81)	(2.07)	(2.01)
Income $25k-$50k	-0.73	-0.83	0.96	1.10	0.53	0.23	0.98	0.95
	(0.68)	(0.73)	(2.31)	(2.32)	(2.53)	(2.44)	(2.00)	(1.95)
Income $50k-$75k	-1.34**	-1.32**	-2.15	-2.19	-1.25	-1.63	-1.70	-1.80
	(0.67)	(0.64)	(2.43)	(2.48)	(1.76)	(1.72)	(1.61)	(1.60)
Income $75k-$100k	-2.10***	-2.15***	-2.62	-2.44	-2.87	-3.45	-2.75	-2.84
	(0.57)	(0.62)	(2.55)	(2.61)	(2.58)	(2.48)	(1.96)	(1.97)
Income $100k +	-1.40***	-1.10**	-5.16***	-5.20***	-8.60***	-9.38***	-6.92***	-7.26***
	(0.48)	(0.51)	(1.83)	(1.85)	(2.71)	(2.63)	(1.89)	(1.88)
Less than high school	-1.06	-1.10	8.36**	8.42**	-5.08	-5.00	1.30	1.32
	(1.73)	(1.68)	(3.88)	(3.90)	(4.50)	(4.51)	(3.54)	(3.53)
Some college or associate degree	-2.84***	-2.34***	-1.81	-2.04	-3.87**	-4.13**	-2.99***	-3.27***
	(0.54)	(0.57)	(1.72)	(1.78)	(1.65)	(1.66)	(1.15)	(1.15)
Bachelor's degree	-5.35***	-4.80***	-7.09***	-7.33***	-7.07***	-7.36***	-7.14***	-7.42***
	(0.83)	(0.81)	(1.75)	(1.76)	(1.89)	(1.89)	(1.21)	(1.18)
Master's or PhD	-6.28***	-5.94***	-9.12***	-9.22***	-9.10***	-9.41***	-9.18***	-9.39***
	(0.84)	(0.86)	(1.99)	(2.02)	(1.93)	(1.93)	(1.44)	(1.43)
State FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	5,462	5,462	2,717	2,717	2,773	2,773	5,490	5,490

Notes: This table presents OLS regressions of voters' pre-treatment beliefs on various covariates. It covers voters' perception the election is decided by less than 100 or 1,000 votes, as well as voters' predictions of the vote margin and vote share for the Democrat. Standard errors are in parentheses, and account for clustering by state using a block bootstrap (500 replications). We account for clustering by state because actual margin and electorate size vary at the state level, and we use a block bootstrap because we only have 13 states. The block bootstrap is executed using "vce(bootstrap, cluster(state))" in Stata 14. The vote margin is the difference in percentage points between the winner and loser among the Democrat and Republican shares of the two-party vote. The subjective prob that the number of heads in 1000 flips would be outside of 481-519 is our measure of non-belief in the law of large numbers (NBLLN), and is discussed further in Appendix A.3. This number is calculated as 100 minus the probability expressed for 481-519. This number is defined as long as someone gives a non-missing answer for 481-519 heads. The correlation here becomes stronger if we restrict attention to people giving non-missing answers on all 7 bins. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C5: The Effect of the Close Poll Treatment on Vote Margin Predictions: Robustness Check where Main Regressor is Continuous (2010 Experiment)

Dep. var = Predicted vote margin, post-treat	b_{post} (1)	b_{post} (2)	b_{post} (3)	Δb (4)	b_{post} (5)	b_{post} (6)	b_{post} (7)	b_{post} (8)
Margin in viewed poll	0.42***	0.22***	0.22***	0.21***	0.22***	0.35***	0.24***	0.30***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.09)	(0.06)	(0.05)
Pred vote margin, pre-treat			0.54***					
			(0.02)					
Viewed margin*Interest in politics (1-5 scale)						-0.03		
						(0.02)		
Viewed margin*Identify Nancy Pelosi as Speaker							-0.02	
							(0.06)	
Viewed margin*Share voted previous 5 elections								-0.13*
								(0.06)
Interest in politics (1-5 scale)					-0.02	0.30	-0.02	-0.01
					(0.21)	(0.28)	(0.21)	(0.21)
Identify Nancy Pelosi as Speaker					-1.53***	-1.53***	-1.33*	-1.53***
					(0.54)	(0.54)	(0.77)	(0.54)
Share voted previous 5 elections (administrative)					-1.13**	-1.13**	-1.14**	0.05
					(0.56)	(0.56)	(0.56)	(0.77)
State FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demog 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

Notes: This is a robustness check to Table 3. The difference is that the main regressor is continuous instead of discrete. That is, instead of looking at whether a person received the close poll (instead of the not close poll), we examine the vote margin they observed in the poll. For example, if the voter was shown a 55-45 poll, the margin in viewed poll is equal to 10. * significant at 10%; ** significant at 5%; *** significant at 1%

Table C6: 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: Robustness Check where Main Regressor is Continuous (2010 Experiment)

	Prob < 100 votes			Prob < 1,000 votes			< 100 or 1,000 votes		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Margin in viewed poll	-0.10	-0.13	-0.14	-0.39	-0.19	-0.14	-0.24	-0.16	-0.14
	(0.06)*	(0.03)***	(0.04)***	(0.05)***	(0.03)***	(0.04)***	(0.04)***	(0.02)***	(0.02)***
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)***
Demog Controls	No	No	Yes	No	No	Yes	No	No	Yes
State FE	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

Notes: This is a robustness check to Table 4. The difference is that the main regressor is continuous instead of discrete. That is, instead of looking at whether a person received the close poll (instead of the not close poll), we examine the vote margin they observed in the poll. For example, if the voter was shown a 55-45 poll, the margin in viewed poll is equal to 10. * significant at 10%; ** significant at 5%; *** significant at 1%

Table C7: The Effect of the Close Poll Treatment on Beliefs: Robustness Check where Restrict to Cases where Beliefs Change (2010 Experiment)

Dep. var.:	Predicted vote margin			Prob < 100 votes			Prob < 1,000 votes			< 100 or 1,000 votes		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Close poll treatment	-3.15***	-4.24***	-4.04***	5.13***	7.25***	6.57***	6.37***	6.86***	6.05***	5.67***	6.98***	6.26***
	(0.71)	(0.68)	(0.67)	(1.92)	(1.57)	(1.57)	(1.79)	(1.44)	(1.43)	(1.31)	(1.07)	(1.05)
Pred vote margin, pre-treat		0.30***	0.24***									
		(0.02)	(0.02)									
Pr(Marg <100 votes), pre					0.61***	0.57***						
					(0.03)	(0.03)						
Pr(Marg <1,000 votes), pre								0.61***	0.57***			
								(0.03)	(0.03)			
<100 or 1,000 votes, pre											0.62***	0.58***
											(0.02)	(0.02)
Mean DV if not close poll=1	17.82	17.57	17.57	33.20	33.27	33.27	38.44	38.44	38.44	36.01	36.05	36.05
Observations	2,530	2,492	2,492	1,031	1,027	1,027	1,148	1,147	1,147	2,179	2,174	2,174
R-squared	0.01	0.13	0.19	0.01	0.34	0.38	0.01	0.36	0.41	0.01	0.35	0.39

Notes: This is a robustness check to Tables 3 and 4. Columns 4-12 here are analogous to columns 1-9 of Table 4. The difference is that we restrict attention to individuals who change their beliefs. For IV results of turnout on beliefs, while also restricting to cases where beliefs change, see Table C19. * significant at 10%; ** significant at 5%; *** significant at 1%

Table C8: Replicating the Literature: Correlation between Actual Ex-post Vote Margin and Turnout (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)
Actual vote margin in state	-0.34*	-0.39**	-0.26**	-0.29**	-0.13**
Clustered SE by state	(0.17)	(0.17)	(0.12)	(0.10)	(0.05)
Block bootstrap SE	(0.33)	(0.33)	(0.28)	(0.24)	(0.11)
Wild bootstrap p value	[0.0710]	[0.168]	[0.0235]	[0.0445]	[0.297]
What is an observation?	State	State	Person	Person	Person
Demographic Controls	No	No	No	Yes	Yes
Control for past voting?	No	Yes	No	No	Yes
Observations	13	13	6,705	6,705	6,705
R-squared	0.14	0.39	0.00	0.10	0.46

Notes: The dependent variable is turnout (0-1) from administrative voting records, with coefficients multiplied by 100 for ease of readability. The regressor of interest is the actual final vote margin in the state, where the actual margin is presented in percentage terms. Columns 1-2 are cross-state regressions where each observation is a state (i.e., a gubernatorial election). In columns 1-2, the sample from columns 3-5 is collapsed by state. In contrast, in columns 3-5, an observation is a person in the 2010 experiment. See Table 5 for more on the Demographic Controls. In column 2, "Control for past voting?" means that we control for a person's average voting rate over the general elections in 2000, 2002, 2004, 2006, and 2008, whereas in column 4, we control for the 5 past voting dummy variables. There are 13 states (clusters). The first row of standard errors presents standard errors clustered by state. The block bootstrap is executed using "vce(bootstrap, cluster(state))" in Stata 14 and using 500 replications. The wild bootstrap is executed using "bootwildct" in Stata 14 and using 2,000 replications. In columns 1-2, clustering by state is the same as robust standard errors (because an observation is a state). The non-robust standard errors are larger for both columns, and equal to 0.25 in column 1 and 0.22 in column 2. Thus, with regular / non-robust standard errors, the column 1 and 2 coefficients lose statistical significance. Stars of statistical significance are calculated based on standard errors clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

Table C9: Demographics and Turnout (2010 Experiment)

	(1)	(2)
Pred vote margin, post-treat	-0.039	-0.001
	(0.04)	(0.03)
Male	2.027*	2.107**
	(1.07)	(0.83)
Black	0.278	1.214
	(2.15)	(1.56)
Hispanic	-3.928	-1.856
	(2.46)	(1.91)
Other	-2.462	-1.089
	(2.99)	(2.54)
Mixed race	5.172	6.827**
	(3.43)	(3.17)
Age 25-34	2.469	-7.700*
	(4.38)	(4.06)
Age 35-44	21.368***	-0.316
	(4.14)	(3.88)
Age 45-54	27.372***	-0.168
	(4.06)	(3.83)
Age 55-64	32.368***	1.432
	(4.03)	(3.81)
Age 65-74	39.524***	4.632
	(4.07)	(3.82)
Age 75 or more	42.827***	4.312
	(4.29)	(3.98)
Household income $25k-$50k	9.106***	2.619
	(2.04)	(1.59)
Household income $50k-$75k	12.444***	2.658*
	(2.03)	(1.60)
Household income $75k-$100k	13.341***	3.002*
	(2.15)	(1.71)
Household income $100k +	14.610***	3.649**
	(2.10)	(1.68)
Less than high school	-9.878**	-8.374***
	(4.07)	(3.22)
Some college or associate degree	1.746	-1.140
	(1.79)	(1.40)
Bachelor's degree	8.769***	2.917**
	(1.84)	(1.44)
Master's or PhD	10.481***	3.326**
	(1.95)	(1.52)
Past Voting Controls	No	Yes
Observations	6,650	6,650
R-squared	0.12	0.46

Notes: The dependent variable is turnout (0-1) from administrative voting records, with coefficients multiplied by 100 for ease of readability. State effects are also included. * significant at 10%; ** significant at 5%; *** significant at 1%

Table C10: Robustness: Beliefs about the Closeness of the Election and Voter Turnout, IV Results (2010 Experiment), No Past Voting Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	0.13	0.13	0.05									
	(0.39)	(0.40)	(0.39)									
Pred vote margin, pre-treat		-0.30	-0.12									
		(0.22)	(0.21)									
Pr(Marg $<$100 votes), post				-1.30	-0.51	-0.49						
				(2.26)	(0.62)	(0.59)						
Pr(Marg $<$100 votes), pre					0.31	0.36						
					(0.54)	(0.50)						
Pr(Marg $<$1,000 votes), post							0.03	0.03	0.35			
							(0.54)	(0.61)	(0.63)			
Pr(Marg $<$1,000 votes), pre								-0.06	-0.26			
								(0.54)	(0.54)			
$<$100 or 1,000 votes, post										-0.28	-0.20	-0.08
										(0.66)	(0.44)	(0.43)
$<$100 or 1,000 votes, pre											0.10	0.06
											(0.39)	(0.37)
F-stat on excl instrument	56.33	86.85	86.65	0.726	22.76	23.25	7.384	21.97	19.86	5.199	43.42	42.89
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

Notes: This table is similar to Table 6. The difference is that we do not include Past Voting Controls (i.e., dummies for whether someone voted in the 2000, 2002, 2004, 2006, and 2008 general elections). * significant at 10%; ** significant at 5%; *** significant at 1%

Table C11: Comparison of Means for 2014 Follow-up Experiment: Balance Test

Closeness:	control	close	close	notclose	notclose	close	not	big	small
Electorate size:		big	small	big	small		close		
Male	.467	.469	.47	.468	.468	.47	.468	.469	.469
Black	.132	.132	.135	.134	.132	.133	.133	.133	.133
Hispanic	.049	.047	.048	.047	.047	.047	.047	.047	.047
Other race	.023	.02	.023	.023	.023	.022	.023	.021	.023
Age	49.81	49.86	49.69	49.89	49.67	49.77	49.78	49.88	49.68
Democrat	.258	.253	.257	.258	.258	.255	.258	.256	.258
Republican	.233	.234	.234	.231	.238	.234	.234	.232	.236
Other party	.509	.513	.509	.511	.504	.511	.508	.512	.506
vote2008?	.659	.661	.657	.657	.657	.659	.657	.659	.657
vote2010?	.489	.489	.49	.489	.488	.489	.488	.489	.489
vote2012?	.712	.713	.713	.712	.71	.713	.711	.713	.711

Notes: This table compares means across the various treatment groups. Because we have a 2x2 design, we provide means for each of the two treatment dimensions (Close/Not Close vs. Big/Small Electorate) separately, as well as for the four different interactions. Gender and race have a small amount of missingness (less than 1%), whereas party registration is unknown/missing (partyaffiliation==“UNK”) for 42% of individuals. Having party affiliation of “Other party” corresponds with having no party affiliation or any other non-Democrat/Republican party affiliation in our data. The high rate of missingness for party affiliation reflects that party affiliation is scant or missing for particular states such as Arkansas and Georgia.

Table C12: Comparison of Means for 2014 Follow-up Experiment: Balance Test, p-values

	close/notclose	close/control	control/notclose
Male	.496	.205	.745
Black	.801	.179	.321
Hispanic	.636	.211	.058
Other race	.083	.082	.525
Age	.946	.556	.621
Democrat	.364	.285	.855
Republican	.99	.695	.708
Other party	.434	.546	.634
vote2008?	.6	.753	.299
vote2010?	.761	.954	.633
vote2012?	.428	.621	.549

Notes: This table compares means across the various treatment groups. p-values are presented in the table.

Table C13: Robustness: Beliefs about the Closeness of the Election and Immediate Intended Probability of Voting, IV Results (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	-0.05	-0.04	-0.05									
	(0.22)	(0.23)	(0.22)									
Pred vote margin, pre-treat		-0.01	0.04									
		(0.13)	(0.12)									
Pr(Marg <100 votes), post				-1.21	-0.41	-0.42						
				(1.68)	(0.34)	(0.33)						
Pr(Marg <100 votes), pre					0.32	0.35						
					(0.30)	(0.28)						
Pr(Marg <1,000 votes), post							0.44	0.49	0.59			
							(0.36)	(0.37)	(0.38)			
Pr(Marg <1,000 votes), pre								-0.42	-0.47			
								(0.33)	(0.33)			
<100 or 1,000 votes, post										0.03	0.03	0.06
										(0.38)	(0.25)	(0.24)
<100 or 1,000 votes, pre											-0.04	-0.03
											(0.22)	(0.21)
F-stat on excl instrument	57.07	85.89	85.44	0.740	23.46	23.94	6.883	21.59	20.00	4.903	43.27	43.10
Mean DV	87.95	88.00	88.00	88.10	88.18	88.18	87.70	87.70	87.70	87.90	87.94	87.94
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	6,645	6,607	6,607	3,285	3,281	3,281	3,406	3,405	3,405	6,691	6,686	6,686

Notes: This table is similar to Table 6. The difference is that the dependent variable here is the post-treatment intended probability of voting (ranging from 0%-100%). * significant at 10%; ** significant at 5%; *** significant at 1%

Table C14: Robustness: Beliefs about the Closeness of the Election and Information Acquisition, IV Results (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	-0.62	-0.58	-0.57									
	(0.43)	(0.42)	(0.42)									
Pred vote margin, pre-treat		0.28	0.28									
		(0.23)	(0.23)									
Pr(Marg <100 votes), post				1.12	0.44	0.39						
				(1.81)	(0.58)	(0.59)						
Pr(Marg <100 votes), pre					-0.35	-0.31						
					(0.51)	(0.50)						
Pr(Marg <1,000 votes), post							0.75	0.90	0.93			
							(0.64)	(0.72)	(0.75)			
Pr(Marg <1,000 votes), pre								-0.78	-0.79			
								(0.64)	(0.64)			
<100 or 1,000 votes, post										0.92	0.67	0.68
										(0.71)	(0.46)	(0.46)
<100 or 1,000 votes, pre											-0.56	-0.57
											(0.40)	(0.40)
F-stat on excl instrument	48.47	85.37	85.52	1.056	26.49	25.52	6.762	18.40	17.51	5.973	43.69	43.03
Mean DV	4.698	4.759	4.759	4.175	4.145	4.145	5.005	5.007	5.007	4.596	4.582	4.582
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	5,790	5,758	5,758	2,874	2,871	2,871	2,957	2,956	2,956	5,831	5,827	5,827

Notes: This table is similar to Table 6. The difference is that the dependent variable is whether an agent started to pay less attention to the campaigns (coded as -1), more attention to the campaigns (coded as +1), or an unchanged amount of attention to the campaigns (coded as 0) after being exposed to a poll, as reported in the post-election survey. As in Table 6, coefficients are multiplied by 100 for ease of readability. * significant at 10%; ** significant at 5%; *** significant at 1%

Table C15: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Weight by Day of Survey (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	-0.29	-0.34	-0.38									
	(0.38)	(0.42)	(0.42)									
Pred vote margin, pre-treat		0.13	0.17									
		(0.23)	(0.22)									
Pr(Marg <100 votes), post				0.04	0.10	0.21						
				(0.61)	(0.68)	(0.69)						
Pr(Marg <100 votes), pre					-0.18	-0.23						
					(0.59)	(0.58)						
Pr(Marg <1,000 votes), post							0.65	0.59	0.67			
							(1.05)	(0.85)	(0.92)			
Pr(Marg <1,000 votes), pre								-0.49	-0.52			
								(0.75)	(0.80)			
<100 or 1,000 votes, post										0.33	0.33	0.42
										(0.64)	(0.57)	(0.59)
<100 or 1,000 votes, pre											-0.32	-0.36
											(0.50)	(0.51)
F-stat on excl instrument	34.82	45.12	44.72	3.395	9.664	9.545	1.444	7.328	5.992	3.255	14.48	13.24
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

Notes: The table is similar to Table 6 in the main text, but we weight each observation by the day of survey response. The idea is that any washing away of beliefs would be lessened for those taking the survey last. The first day of survey response (day 14) is Wednesday, October 20, 2010. The last day of survey response is Election Day, or Tuesday, November 2, 2010. The weighting is done using "aweights" in Stata.

Table C16: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Restrict to half of the sample that took the survey closest to the election (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	-0.04	-0.09	-0.11									
	(0.41)	(0.41)	(0.41)									
Pred vote margin, pre-treat		0.01	0.04									
		(0.22)	(0.22)									
Pr(Marg <100 votes), post				-0.06	-0.01	-0.00						
				(0.45)	(0.66)	(0.65)						
Pr(Marg <100 votes), pre					-0.06	-0.03						
					(0.58)	(0.56)						
Pr(Marg <1,000 votes), post							-0.10	-0.14	-0.07			
							(0.77)	(1.08)	(1.15)			
Pr(Marg <1,000 votes), pre								0.13	0.08			
								(0.94)	(0.98)			
<100 or 1,000 votes, post										-0.06	-0.05	-0.02
										(0.45)	(0.61)	(0.61)
<100 or 1,000 votes, pre											0.02	0.02
											(0.53)	(0.52)
F-stat on excl instrument	30.40	46.48	47.48	6.404	10.79	10.86	2.089	3.970	3.425	6.120	12.61	12.46
Mean DV	70.32	70.41	70.41	70.92	71.04	71.04	69.54	69.52	69.52	70.23	70.28	70.28
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	3,373	3,349	3,349	1,702	1,699	1,699	1,694	1,693	1,693	3,396	3,392	3,392

Notes: The table is similar to Table 6 in the main text, but we restrict to half of the sample that took the survey closest to the election. The idea is that any washing away of beliefs would be lessened for those taking the survey later. The first day of survey response (day 14) is Wednesday, October 20, 2010. The last day of survey response is Election Day, or Tuesday, November 2, 2010.

Table C17: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to People Who Don't Always Vote (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	-0.41	-0.46	-0.47									
	(0.38)	(0.41)	(0.41)									
Pred vote margin, pre-treat		0.19	0.24									
		(0.22)	(0.22)									
Pr(Marg <100 votes), post				-1.22	0.03	0.15						
				(9.95)	(0.73)	(0.70)						
Pr(Marg <100 votes), pre					-0.09	-0.15						
					(0.63)	(0.59)						
Pr(Marg <1,000 votes), post							0.89	0.69	0.72			
							(0.97)	(0.64)	(0.64)			
Pr(Marg <1,000 votes), pre								-0.59	-0.56			
								(0.54)	(0.53)			
<100 or 1,000 votes, post										1.10	0.41	0.45
										(1.64)	(0.49)	(0.48)
<100 or 1,000 votes, pre											-0.38	-0.37
											(0.42)	(0.40)
F-stat on excl instrument	43.41	58.41	58.29	0.0309	10.85	11.50	2.566	13.95	13.89	1.070	23.90	24.18
Mean DV	58.03	58.09	58.09	57.76	57.88	57.88	58.16	58.14	58.14	57.97	58.01	58.01
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	4,086	4,061	4,061	1,991	1,987	1,987	2,120	2,119	2,119	4,111	4,106	4,106

Notes: The table is similar to Table 6 in the main text, but the sample is restricted to voters who don't always vote. That is, we drop people who voted in all 5 general elections in 2000, 2002, 2004, 2006, and 2008.

Table C18: Robustness: Impact of Close/Not Close Postcard Treatments on Turnout, Sample Restricted to People Who Don't Always Vote (2014 Experiment)

	(1)	(2)	(3)	(4)	(5)
Close poll (vs. not close poll)	0.43	0.42		0.42	
	(0.35)	(0.34)		(0.34)	
Close poll (vs. control)			0.38		
			(0.25)		
Not close poll (vs. control)			-0.03		
			(0.25)		
Small electorate likely				-0.21	
				(0.34)	
Close poll X Small electorate					0.21
					(0.49)
Close poll X Large electorate					0.31
					(0.48)
Not close poll X Small electorate					-0.32
					(0.48)
F(Close vs. NotClose)			0.228		
Mean DV if not close poll=1	29.43	29.43		29.43	29.43
Mean DV if control=1			29.42		
Additional controls	No	Yes	Yes	Yes	Yes
Observations	71,385	71,385	782,677	71,385	71,385

Notes: This table is similar to Table 7 in the main text, but the sample is restricted to voters who don't always vote. That is, we drop people who voted in all 3 general elections in 2008, 2010, and 2012. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C19: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to People Who Change their Closeness Beliefs (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	-0.09	-0.16	-0.13									
	(0.42)	(0.34)	(0.35)									
Pred vote margin, pre-treat		0.01	0.02									
		(0.09)	(0.09)									
Pr(Marg <100 votes), post				-0.29	-0.22	-0.30						
				(0.51)	(0.32)	(0.34)						
Pr(Marg <100 votes), pre					0.08	0.11						
					(0.20)	(0.19)						
Pr(Marg <1,000 votes), post							0.14	0.13	0.13			
							(0.37)	(0.33)	(0.34)			
Pr(Marg <1,000 votes), pre								-0.06	-0.04			
								(0.20)	(0.20)			
<100 or 1,000 votes, post										-0.06	-0.05	-0.06
										(0.30)	(0.23)	(0.24)
<100 or 1,000 votes, pre											0.02	0.03
											(0.14)	(0.14)
F-stat on excl instrument	22.68	40.08	35.94	5.482	20.88	18.75	9.674	18.73	17.83	14.45	37.49	35.38
Mean DV	67.23	67.30	67.30	66.93	67.19	67.19	69.34	69.31	69.31	68.20	68.31	68.31
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	2,530	2,492	2,492	1,031	1,027	1,027	1,148	1,147	1,147	2,179	2,174	2,174

Notes: The table is similar to Table 6 in the main text, but the sample is restricted in each regression to people who change their beliefs about the closeness of the election on that belief variable. For example, columns 1-3 restrict to people with a change in predicted vote margin, whereas columns 4-6 restrict to people with a change in perceived chance of the election being decided by less than 100 votes.

Table C20: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Using Log Beliefs and Sample Restricted to People Who Change their Closeness Beliefs (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
IV Results:												
log Pred vote margin, post-treat	-0.78	-1.28	-1.07									
	(3.75)	(3.41)	(3.48)									
log Pred vote margin, pre-treat		0.22	0.29									
		(0.83)	(0.82)									
log Pr(Marg <100 votes), post				-4.58	-3.88	-5.25						
				(8.01)	(5.43)	(5.66)						
log Pr(Marg <100 votes), pre					1.12	1.58						
					(3.37)	(3.23)						
log Pr(Marg <1,000 votes), post							3.23	2.82	2.77			
							(8.72)	(7.52)	(7.62)			
log Pr(Marg <1,000 votes), pre								-1.80	-1.29			
								(4.33)	(4.06)			
log <100 or 1,000 votes, post										-1.07	-1.12	-1.23
										(5.89)	(4.51)	(4.67)
log <100 or 1,000 votes, pre											0.03	0.33
											(2.72)	(2.61)
F-stat on excl instrument	82.74	104.5	99.87	10.58	34.23	31.65	10.25	19.62	19.47	20.58	51.18	48.77
Mean DV	67.23	67.30	67.30	66.93	67.19	67.19	69.34	69.31	69.31	68.20	68.31	68.31
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	2,530	2,492	2,492	1,031	1,027	1,027	1,148	1,147	1,147	2,179	2,174	2,174
First Stage Results:												
Close poll treatment	-0.37***	-0.42***	-0.41***	0.28***	0.42***	0.40***	0.24***	0.28***	0.27***	0.26***	0.34***	0.32***
	(0.04)	(0.04)	(0.04)	(0.09)	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)

Notes: The table is similar to Table C19, but we the logarithm of all the belief measures (instead of beliefs in levels). The log is calculated with 1 added inside. That is, for column 1, the regressor of interest is log(1+ post-treatment predicted vote margin). We use a 1 inside because beliefs can be 0. Recall that IV coefficients are multiplied by 100 for ease of readability. To interpret the first stage, for example, the coefficient in column 1 means that the close poll decreased believed vote margin by roughly 37% (among those in our sample of people who change their beliefs).

We also made Table 6 using log beliefs instead of beliefs in levels. The conclusions were unchanged. In terms of precision, we found that increasing the perceived vote margin by 10% is predicted to increase turnout by no more than 0.78pp. Increasing the perceived chance of a very close election by 10% is predicted to increase turnout by no more than 1.37pp. The column 1 first stage coefficient was -0.27, meaning that the treatment decreased the predicted vote margin by roughly 27%.

Table C21: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to Voters with Strong Ideology (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	-0.34	-0.41	-0.43									
	(0.44)	(0.44)	(0.44)									
Pred vote margin, pre-treat		0.15	0.18									
		(0.24)	(0.24)									
Pr(Marg <100 votes), post				0.99	-0.08	-0.12						
				(31.54)	(0.83)	(0.81)						
Pr(Marg <100 votes), pre					0.01	0.05						
					(0.72)	(0.70)						
Pr(Marg <1,000 votes), post							0.48	0.45	0.58			
							(0.62)	(0.55)	(0.58)			
Pr(Marg <1,000 votes), pre								-0.37	-0.46			
								(0.49)	(0.50)			
<100 or 1,000 votes, post										0.56	0.27	0.31
										(0.97)	(0.44)	(0.44)
<100 or 1,000 votes, pre											-0.24	-0.26
											(0.39)	(0.38)
F-stat on excl instrument	21.77	35.98	35.92	0.00221	7.137	7.211	3.384	15.05	13.45	1.525	23.25	23.22
Mean DV	76.22	76.29	76.29	75.60	75.71	75.71	76.56	76.55	76.55	76.09	76.14	76.14
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	2,796	2,780	2,780	1,377	1,375	1,375	1,438	1,437	1,437	2,815	2,812	2,812

Notes: The table is similar to Table 6 in the main text, but the sample is restricted to individuals with a "strong ideology." Strong ideology is defined as having a 1, 2, 6, or 7 on a 1-7 scale of conservatism/liberalism, where 1="extremely liberal", 2="liberal," 3="slightly liberal," 4="moderate, middle of the road," 5="slightly conservative," 6="conservative," and 7="extremely conservative."

Table C22: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Sample Restricted to Voters with Higher Interest in Politics (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	0.19	0.17	0.16									
	(0.40)	(0.41)	(0.41)									
Pred vote margin, pre-treat		-0.15	-0.12									
		(0.24)	(0.23)									
Pr(Marg <100 votes), post				-0.05	-0.06	-0.02						
				(1.50)	(0.57)	(0.60)						
Pr(Marg <100 votes), pre					-0.01	-0.04						
					(0.51)	(0.52)						
Pr(Marg <1,000 votes), post							-0.21	-0.31	-0.22			
							(0.38)	(0.57)	(0.59)			
Pr(Marg <1,000 votes), pre								0.27	0.19			
								(0.51)	(0.51)			
<100 or 1,000 votes, post										-0.20	-0.18	-0.16
										(0.47)	(0.41)	(0.42)
<100 or 1,000 votes, pre											0.13	0.12
											(0.37)	(0.37)
F-stat on excl instrument	26.81	44.04	44.46	0.439	14.24	12.87	7.292	13.78	12.73	4.550	27.03	25.94
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	4,037	4,019	4,019	1,993	1,992	1,992	2,071	2,071	2,071	4,064	4,063	4,063

Notes: The table is similar to Table 6 in the main text, but the sample is restricted to voters who indicated a high interest in politics. Subjects are asked about their interest in what's going on in politics and government. We define "high interest" as choosing "extremely interested" or "very interested." Those selecting "moderately interested," "slightly interested," or "not interested at all" are defined as not having high interest. For the analysis in Table 3 in the main text, we convert these 5 categories into a 5-point scale.

Table C23: Beliefs about the Closeness of the Election and Voter Turnout, IV Results: Drop Larger States (2010 Experiment)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	0.05	-0.00	-0.02									
	(0.34)	(0.34)	(0.34)									
Pred vote margin, pre-treat		-0.07	-0.03									
		(0.19)	(0.19)									
Pr(Marg <100 votes), post				0.91	-0.53	-0.44						
				(1.77)	(0.62)	(0.59)						
Pr(Marg <100 votes), pre					0.39	0.34						
					(0.54)	(0.50)						
Pr(Marg <1,000 votes), post							0.11	0.16	0.16			
							(0.38)	(0.50)	(0.53)			
Pr(Marg <1,000 votes), pre								-0.16	-0.14			
								(0.44)	(0.44)			
<100 or 1,000 votes, post										-0.22	-0.13	-0.09
										(0.89)	(0.40)	(0.39)
<100 or 1,000 votes, pre											0.08	0.06
											(0.35)	(0.34)
F-stat on excl instrument	39.99	65.56	65.59	0.686	11.93	12.77	7.873	16.86	15.42	1.552	26.72	27.64
Mean DV	74.25	74.31	74.31	74.24	74.35	74.35	74.19	74.17	74.17	74.21	74.26	74.26
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	3,965	3,943	3,943	1,968	1,965	1,965	2,026	2,025	2,025	3,994	3,990	3,990

Notes: The table is similar to Table 6 in the main text, but we drop individuals from larger states. To define a large state, we calculate the median electorate size in our sample. Then we drop individuals from states where the electorate is above the median. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C24: Robustness: Impact of Close/Not Close Postcard Treatments on Turnout, Drop Larger States (2014 Experiment)

	(1)	(2)	(3)	(4)	(5)
Close poll (vs. not close poll)	-0.00	0.02		0.02	
	(0.33)	(0.32)		(0.32)	
Close poll (vs. control)			0.30		
			(0.24)		
Not close poll (vs. control)			0.28		
			(0.23)		
Small electorate likely				0.26	
				(0.32)	
Close poll X Small electorate					0.28
					(0.46)
Close poll X Large electorate					-0.05
					(0.46)
Not close poll X Small electorate					0.19
					(0.46)
F(Close_vs_NotClose)			0.959		
Mean DV if not close poll=1	60.37	60.37		60.37	60.37
Mean DV if control=1			60.28		
Additional controls	No	Yes	Yes	Yes	Yes
Observations	73,418	73,418	804,537	73,418	73,418

Notes: This table is similar to Table 7 in the main text, but we drop individuals from larger states. To define a large state, we calculate the median electorate size in our sample. Then we drop individuals from states where the electorate is above the median. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C25: Robustness: Beliefs about the Closeness of the Election and Voter Turnout, IV Results (2010 Experiment), Restrict Attention to People who Don't Change their Intended Probability of Voting Democrat

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pred vote margin, post-treat	-0.28	-0.33	-0.36									
	(0.34)	(0.35)	(0.34)									
Pred vote margin, pre-treat		0.15	0.20									
		(0.21)	(0.20)									
Pr(Marg <100 votes), post				-0.05	-0.06	-0.05						
				(0.82)	(0.50)	(0.48)						
Pr(Marg <100 votes), pre					0.00	0.02						
					(0.45)	(0.42)						
Pr(Marg <1,000 votes), post							0.38	0.43	0.58			
							(0.42)	(0.46)	(0.49)			
Pr(Marg <1,000 votes), pre								-0.38	-0.48			
								(0.41)	(0.43)			
<100 or 1,000 votes, post										0.25	0.20	0.25
										(0.42)	(0.34)	(0.34)
<100 or 1,000 votes, pre											-0.20	-0.22
											(0.31)	(0.30)
F-stat on excl instrument	44.02	76.48	77.42	2.011	23.26	24.89	7.651	26.28	23.77	7.629	48.76	47.99
Mean DV	74.01	74.08	74.08	74.26	74.36	74.36	73.62	73.62	73.62	73.94	73.98	73.98
Demographic Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	5,834	5,799	5,799	2,875	2,871	2,871	2,999	2,998	2,998	5,874	5,869	5,869

Notes: This table is similar to Table 6 in the main text, but the sample is restricted to individuals who do not change their intended probability of voting for the Democratic candidate after observing the poll information in the pre-election survey. * significant at 10%; ** significant at 5%; *** significant at 1%

Table C26: Robustness: Impact of Close/Not Close Postcard Treatments on Turnout, Restrict to People with Name on Postcard or whose Name Would Have been on Postcard (2014 Experiment)

	(1)	(2)	(3)	(4)	(5)
Close poll (vs. not close poll)	0.39	0.40		0.40	
	(0.28)	(0.28)		(0.28)	
Close poll (vs. control)			0.43**		
			(0.20)		
Not close poll (vs. control)			0.04		
			(0.20)		
Small electorate likely				-0.16	
				(0.28)	
Close poll X Small electorate					0.24
					(0.39)
Close poll X Large electorate					0.15
					(0.39)
Not close poll X Small electorate					-0.40
					(0.39)
F(Close_vs_NotClose)			0.155		
Mean DV if not close poll=1	51.51	51.51		51.51	51.51
Mean DV if control=1			51.45		
Additional controls	No	Yes	Yes	Yes	Yes
Observations	78,838	78,838	868,112	78,838	78,838

Notes: This table is similar to Table 7 in the main text. The difference is we restrict attention to the person to whom the postcard is addressed (or to whom the postcard would have been addressed in cases where the household did not receive a postcards). In contrast, in our main results, we include all voters in the household as being treated, both the person to whom the postcard as addressed and the potential others to whom the postcard is not addressed. In column 3, we include individuals who would have received a postcard had they been randomly assigned to receive either the close or not close treatment arms. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C27: Reduced Form: Impact of Close/Not Close Treatments on Turnout (2010 Expt)

	(1)	(2)	(3)
Received close poll treatment	0.19	0.23	
	(0.81)	(0.81)	
Assigned to Close Poll Treatment			-0.07
			(0.68)
Assigned to Not Close Poll Treatment			-0.41
			(0.68)
Additional controls	No	Yes	No
Mean DV if received not close poll=1	72.18	72.18	
Mean DV if assigned to control=1			70.42
Observations	6,705	6,705	15,460
R-squared	0.45	0.46	0.40

Notes: This table shows reduced-form results from the 2010 experiment. In columns 1 and 2, the main regressor is a dummy equal to 1 if someone received the close poll treatment (i.e., they took the survey and saw the close poll) and 0 (i.e., they took the survey and saw the not close poll). This is our main regressor for most of the paper. In contrast, in column 3, the main regressors are dummies for being assigned to get the close poll and for being assigned to get the not close poll (the excluded group is people who were assigned to receive no survey invitation). All regressions include state fixed effects and past voting controls. The additional controls are the demographic controls listed in Table 3. Observations are excluded from column 3 if the state identifier is missing in the administrative voting data. (In columns 1-2, the state identifier is from data from Knowledge Networks and has no missingness.) * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C28: Beliefs About the Closeness of the Election and Voter Turnout, TSIV Estimates

	(1)	(2)	(3)	(4)
Predicted vote margin, post-treat	-0.10			
	(0.09)			
Pr(Marg <100 votes), post		0.11		
		(0.10)		
Pr(Marg <1,000 votes), post			0.12	
			(0.11)	
<100 or 1,000 votes, post				0.12
				(0.10)
Observations	126,126	126,126	126,126	126,126

Notes: This table shows two-sample IV (TSIV) estimates of how beliefs about the closeness of the election affect turnout. 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. The reduced form (estimated using the 2014 experiment) is from column 4 of Table 7 and is based on the coefficient "Close poll (vs. not close poll)". The first stage (estimated using the 2010 experiment) is based on column 2 of Table 3, as well as columns 3, 6, and 9 of Table 4. Standard errors are calculated by the Delta Method (see Appendix A.4). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C29: Testing for the Bandwagon Effect: The Effect of Beliefs about Democrat Likely Vote Share on Voting for the Democratic Candidate, IV Results (2010 Experiment)

Specification:	1st Stage	OLS	IV	IV	IV
Dep. var.:	Predicted Dem share, Post-treatment	Vote Dem	Vote Dem	Vote Dem	Vote Dem
	(1)	(2)	(3)	(4)	(5)
Dem vote share in viewed poll	0.27***				
	(0.03)				
Predicted dem share, post-treatment		0.16***	0.48	0.50	0.49
		(0.05)	(0.41)	(0.41)	(0.41)
Predicted dem share, pre-treatment				-0.16	-0.18
				(0.23)	(0.22)
Demographic Controls	No	No	No	No	Yes
Observations	6,684	4,594	4,594	4,582	4,582
F-stat on excl instrument			48.56	69.69	68.98

Notes: Coefficients are multiplied by 100 for ease of readability. Robust standard errors in parentheses. Column 1 is an OLS regression of the post-treatment predicted Democrat vote share on the Democrat vote share shown in the viewed poll. Column 2 is an OLS regression of whether someone voted for the Democratic candidate (self-reported). Columns 3-5 are IV regressions similar to the column 2 regression; in these columns, the voters' beliefs about the likely Democratic vote share are instrumented with the Democratic vote share in the poll they were shown. All regressions control for a person's pre-treatment intended probability of voting Democrat. Demographic controls are as listed in Table 3. The sample size is smaller in columns 2-5 than column 1 because some individuals do not take the post-election survey where the vote choice question is asked, and some people also refuse to answer the vote choice question. The coefficient is 0.23(0.03) if one re-does column 1 while restricting to the sample in column 2. * significant at 10%; ** significant at 5%; *** significant at 1%

D Appendix: Theory

In the body of the paper we highlight several classes of voting models, and discuss to what extent they can generate Prediction 1. In this Appendix we present theoretical results that formalize the discussion in the body of the paper and link common voting models to our experimental treatment.

This Appendix has several parts. In sub-section D.1 we develop a model of how potential voters may update their beliefs from polls. We then turn to considering how shifts in beliefs, caused by observing different poll results, will change behavior. Sub-section D.2 considers a standard private values instrumental model, while sub-sections D.3, D.5, and D.4 discuss the predictions of the prediction of common-values models, duty-voting models, and signaling models, respectively. Our formalization allows us to capture both the standard, Bayesian case,

Table C30: Robustness: Testing for Bandwagon Effects using Intended Probability of Voting Democrat (2010 Experiment)

	OLS (1)	IV (2)	IV (3)	IV (4)
Predicted dem share, post-treatment	0.06***	0.28**	0.29**	0.29**
	(0.02)	(0.12)	(0.13)	(0.13)
Predicted dem share, pre-treatment			-0.16**	-0.16**
			(0.07)	(0.07)
Observations	6,684	6,684	6,665	6,665
F-stat on excl instrument		80.27	113.3	112.2
Demographic Controls	No	No	No	Yes

Notes: The table is similar to Table C29. The difference is that we look at post-treatment intended probability of voting for the Democratic candidate as the dependent variable (as opposed to whether someone actually voted for the Democratic candidate).

Table C31: The Relevance of Perceived Closeness for the Observational Relationship between Actual Closeness and Voter Turnout: Robustness, where Combine Two Belief Measures (Predicted Margin and less than 100/1,000 combined measure)

Belief variable used:	Point estimate on s	95% CI for s
Panel A: 2010 Experiment	(4)	(5)
Overall for 2010	0.11	[-0.34, 0.56]
Panel B: 2014 Experiment	(4)	(5)
Overall for 2014	0.11	[-0.03, 0.25]
Panel C: Pooled Data	(4)	(5)
Overall for pooled data	0.10	[-0.04, 0.23]

Notes: This table presents a robustness check for columns 4-5 in Table 8 for the overall estimates of s. Table 8 used three belief measures to create the estimates there: Predicted vote margin, Pr(Marg <100 votes), and Pr(Marg <1,000 votes). In contrast, this table uses two belief measures: Predicted vote margin and the perceived probability of less than 100 or 1,000 votes (as people are only asked about 100 or 1,000 words).

where individuals' beliefs correspond to the true distributions, but also cases where subjective beliefs may not be correct, and individuals may not use Bayesian updating.

The experimental variation we are interested does not concern equilibrium outcomes, but rather the best response function of any individual voter. Hence, we focus on formal results regarding the comparative statics of this function.

We first present a few formal details that we use throughout the rest of this Appendix. As is true in our data, and the vast majority of the literature, we consider on majority rule elections where voters choose between two candidates A and B. As is typical in majority elections, we assume that the candidate receiving the most votes wins, and, in the event of a tie, a fair coin determines the winner.

We suppose that the realized number of eligible voters, m, is drawn from a distribution $H(\bullet; n)$ with support from $\{0, 1, ..., \infty\}$ and parameterized by n. We denote any given individual voter as i. We allow for i to have subjective (possibly incorrect) beliefs about H, which we denote $\hat{H}_i(\bullet; n)$). The parameter n represents the expected number of eligible voters. An individual voter knows the parameter n and the distribution H, but she does not know the realization m. Note that when H is degenerate, the model collapses to the familiar setting where there are a fixed number of voters. In contrast, if H is non-degenerate, then there is aggregate uncertainty as to the size of the electorate. We define "large" elections as the where $n \to \infty$. We make the mild assumption that in large elections the uncertainty regarding the electorate size is small. Formally, denoting the standard deviation of $H(\bullet; n)$ as $v(n)$ we assume that $\lim_{n\to\infty} \frac{v(n)}{n} = 0$ (we suppose the same assumption holds for $\hat{H}$). This assumption is satisfied by both of the most commonly used distributions in pivotal voting models: where H is either a degenerate distribution or a Poisson distribution.

D.1 Information and Beliefs

The first key linkage we want to explore is the connection between a voter's information and their beliefs about election outcomes (e.g., the margin of victory or the probability of being pivotal). We do so in the context of large elections, which fits our empirical application to state gubernatorial elections, where electorates are typically in the millions. We explore the effects of differential information exposure, in particular exposure to different polls, on beliefs. Key for our empirical strategy is that such experimental variation allows us to control for possible endogeneity of beliefs.

We suppose that a poll reports, among individuals sampled, the proportion of respondents who support candidate A, the proportion who support candidate B, and the number of respondents (or equivalently, a margin of error). Moreover, we suppose that these polls sample only individuals planning on voting already at any given point in time. Thus, polls represent information about the margin of support for the candidates among those planning on voting.

We will assume, as is true in our data, that both polls favor the same candidate, which we suppose is candidate A. Moreover, we also suppose that all polls are of the same size (this is not necessarily true in the data).[14] In order to link our theoretical results with the experimental design, we suppose that individuals treat the information we provide them in the

[14]So long as there is not too much variation across the size of polls, by continuity our results will continue to hold.

experiment "as if" only they received it. Thus, we suppose that receiving this new information does not cause people to believe that everyone has updated their beliefs. The comparative statics we examine in this sub-section reflect how beliefs about election outcomes will shift if a given individual is exposed to different information (i.e., polls).

Formally, while the voting model is static, in reality beliefs and behavior will vary across time as new information is obtained. Given any particular date in time, we denote σ_A as the number of individuals out of the entire population (of size m) who would go to the polls and cast their vote for A. σ_B is defined equivalently, and so the number of individuals who would abstain from voting is $1 - \sigma_A - \sigma_B$.

Polls only survey "likely" voters, those who would actually go to the poll. Thus, the inputs of the poll, σ_A and σ_B, represent the realized number of A and B voters were the election held at the date on which polling occurs. The pollster samples these individuals and reports the percentage of A voters, along with the number of respondents. Publicly releasing this information causes voters who observe the poll to update their beliefs about pivotality, and adjust their voting behavior accordingly. In line with our assumption that voters suppose only they are shown the poll information, we suppose the sets σ_A and σ_B are fixed after the poll is taken — i.e. all other voters will not switch their strategy between the polling date and the election.

Our identification strategy relies on this updating and adjustment process. In order to formalize our results, we will suppose the poll is of size N, which is a random sample of individuals who would vote for one of two candidates were the election held today (i.e., individuals who would not abstain). Out of the N individuals in the poll, k of them are A supporters and so $\frac{k}{N} \geq \frac{1}{2}$ is the fraction of A supporters in the poll. Similarly, $\frac{\sigma_A}{\sigma_A+\sigma_B}$ is the fraction of A supporters in the population of individuals who will actually vote in the election. For a given individual i let $\rho_i(\frac{k}{N}|\frac{\sigma_A}{\sigma_A+\sigma_B}, N, \sigma_A + \sigma_B)$ denote the conditional distribution of the level of support for A in the poll.

In most models of voting, individuals have a correct perception of probabilities and are Bayesian. We want to nest the standard model, but also allow for subjective, non-Bayesian beliefs and updating. Thus, we will specify an updating rule which generalizes Bayes' rule, but still allows for analytic tractability. Assumption A1 formalizes this structure.

A1: *Given a set of states Z, a set of events Ω, a (possibly subjective) prior belief over states $\rho(z)$ and a (possibly subjective) probability of any given event ω conditional on state z event $\phi(\omega|z)$ the posterior belief of state z_i, conditional on event ω_j, is:*

$$\frac{\phi(\omega_j|z_i)\gamma(\rho(z_i))}{\int_k \phi(\omega_j|z_k)\gamma(\rho(z_k))}$$

where γ is a monotone function that maps from the unit interval to the unit interval. We suppose that γ, ϕ and ρ are also continuous. Similarly, the ex-ante expected probability of ω_j is $\int_k \phi(\omega_j|z_k)\gamma(\rho(z_k))$.[15]

In particular, the standard Bayesian model occurs when ϕ is the true conditional probability, and γ is the identity function. More generally, A1 supposes that agents use a form of Bayes' rule, but where they are allowed to have subjective beliefs and possibly distort priors. Thus, we refer to $\gamma(\rho(z_k))$ as the subjective probability of state z_k.

[15] If we have a finite number of states or events then we can obtain continuity trivially for the latter two.

Our particular formulation is consistent with much of the literature on non-Bayesian updating, and nests the models of Benjamin et al. (2016), Barberis et al. (1998), Rabin (2002), Rabin and Vayanos (2010), Bodoh-Creed et al. (2014) and He and Xiao (2015). Thus it can accommodate phenomenon such as the gambler's fallacy, belief in hot-hands, non-belief in the Law of Large Numbers, and base rate neglect. Some non-Bayesian models do not satisfy assumption A1, such as the model of Mullainathan et al. (2008). This is not to say that our paper is inapplicable in these situations; other assumptions can be provided. A1 simply is a tractable generalization of Bayes' rule in order which gives expositional structure.

Intuition suggests that beliefs about the expected vote share of each candidate should positively vary with the poll results, i.e., a poll indicating that A is winning handily should produce posterior beliefs ascribing a greater vote share to A than would be the case if the poll showed a tight race. We next provide a sufficient condition (satisfied if individuals have correct beliefs), which in conjunction with A1, that generates such a result.

A2: *Fixing N and $\sigma_A + \sigma_B$, an individual's subjective beliefs $\rho(\frac{k}{N}|\frac{\sigma_A}{\sigma_A+\sigma_B}, N, \sigma_A + \sigma_B)$ exhibit the monotone likelihood ratio property in $\frac{\sigma_A}{\sigma_A+\sigma_B}$.*

A2 is naturally satisfied by true Bayesians with correct beliefs, as they recognize that poll outcomes are simply a binomial distribution featuring N i.i.d. draws from a population with parameter $\frac{\sigma_A}{\sigma_A+\sigma_B}$.

Of course, our primary interest in beliefs about the possible closeness of an election. In order to link beliefs about the margin of victory for A to the actual closeness of an election (in terms of margin of victory), we need an additional restriction on prior beliefs. This assumption, A3, also implicitly supposes that prior beliefs and polls agree about the likely winners. A3, in conjunction with A1 and A2, implies that observing a not-close poll leads to predictions of a larger margin of victory than observing a close poll.[16]

A3: *Suppose an individual's priors beliefs first-order stochastically dominate beliefs (in favor of A winning) that are symmetric around a tied outcome.*

Given the data we have, testing whether individual's updating processes and beliefs obey A1-A3 is not possible. But, their importance lies in the fact that they allow us to naturally link poll results to changes in beliefs, as the next proposition demonstrates.

Proposition 1: *Fix the sizes of polls, suppose the election is large and that A1 and A2 hold for all individuals. (i) Observing a poll with a smaller margin of victory for A leads to beliefs that the margin of victory for A in the election will be smaller. (ii) If individuals also satisfy A3 then observing a poll with a smaller margin of victory for A leads to beliefs that the margin of victory for A will be closer to 50%.*

Proof: We first prove (i). In doing so, recall that the size of both polls are of size N. We will denote the number of A supporters in the close poll as k and in the not-close poll as k'. Thus, $k' > k$. In the limit, as the expected size of the electorate goes to infinity, the revelation of the poll size N does not cause the individual to update about the distribution of the realized size of the electorate m. Thus, the information causes the individual to only update about

[16] If we do not suppose that polls agree with prior beliefs about the likely winner, one can easily construct counterexamples our result. For example, if an individual strongly believes that B is more likely to win, while poll results favor A, then observing the not-close poll may make them believe that the election outcome will be close, while observing the close poll leads them to believe that B will still win by a landslide.

$\frac{\sigma_A}{\sigma_A+\sigma_B}$. Denote $\frac{\sigma_A}{\sigma_A+\sigma_B}$ as ς_A, and prior beliefs over ς_A as $\zeta(\varsigma_A)$. By A1 the posterior beliefs after the close and not-close polls (respectively) are

$$\frac{\rho(\frac{k}{N}|\varsigma_A, N)g(\zeta(\varsigma_A))}{\int_{\varsigma'_A}\rho(\frac{k}{N}|\varsigma'_A, N)g(\zeta(\varsigma'_A))}$$

and

$$\frac{\rho(\frac{k'}{N}|\varsigma_A, N)g(\zeta(\varsigma_A))}{\int_{\varsigma'_A}\rho(\frac{k'}{N}|\varsigma'_A, N)g(\zeta(\varsigma'_A))}$$

Then, by A2, the posterior beliefs attached to ς_A after observing the not-close poll must first order stochastically dominate those attached to observing the close poll.[17] Thus, after observing the distant poll, the individual's mean belief about the winning margin of victory $E(\varsigma_A)$ must be larger than after observing the close poll.

We now turn to proving (ii). Because we consider large elections, we prove our result for the limit case, where if there is an idiosyncratic component to preferences, the realized distribution is equal to its expectation. Consider an individual i with a symmetric (around .5) prior distribution of ς_A. Then i's prior expected margin of victory for A is equal to 0. Note that if i observes a poll which gives equal support for A and B then i's posterior expected margin of victory is equal to 0 (because the prior distribution is symmetric). Now suppose i observes a close poll where $k > \frac{N}{2}$. Then i's posterior expected margin of victory for A is greater than 0 by A2. Now consider j, whose prior beliefs first order stochastically dominate i's prior. By Theorem 1 of Klemens (2007), j's posterior distribution after observing the close poll dominates i's posterior distribution after observing the close poll because of A2. Therefore, j also has a positive expected margin of victory for A after observing the close poll. In comparison, suppose j observes a not-close poll, with $k' > k$. Then by A2, then j has a larger posterior expected margin of victory for A after observing the not-close poll compared to the close poll (and so the expected margin of victory is farther from $\frac{1}{2}$).$\square$

The next proposition links observing different polls to different posterior beliefs about particular kinds of close elections: those decided by less than 100 or 1,000 votes. In order to simplify the statement of the proposition, we will provide several definitions.

We can first define the likelihood ratio of a close to a not-close poll, given any particular realization of $\varsigma_A = \frac{\sigma_A}{\sigma_A+\sigma_B}$. We can also, in a similar manner, define, given a prior ζ over ς_A, the likelihood ratio of the expected probability of a close poll to the expected probability of a not-close poll.

Definition: *Define $l(k, k'|N, \varsigma_A)$ as the likelihood ratio of seeing a given close poll with k A supporters out N respondents, to seeing a not-close poll with $k' > k$ A supporters out of N respondents, conditional on the state being ς_A:*

$$l(k, k'|N, \varsigma_A) = \frac{\rho(\frac{k}{N}|\frac{1}{2})}{\rho(\frac{k'}{N}|\frac{1}{2})}.$$

Similarly, define $E[l](k, k'|N, \zeta)$ as the likelihood ratio of the expected probability of a given

[17] Recall that the distortion function γ does not affect the monotone likelihood ratio ordering.

close poll with k A supporters out N respondents, to the expected probability of a given not-close poll with $k' > k$ A supporters out of N respondents:

$$E[l](k,k'|N,\zeta) = \frac{\int_{\varsigma'_A} \rho(\frac{k}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))}{\int_{\varsigma'_A} \rho(\frac{k'}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))}.$$

The next proposition shows that if the likelihood ratio of a close to a not-close poll, given a split electorate, is larger than the likelihood ratio of the expected probability of a close poll to the expected probability of a given not-close poll, then an individual will attach higher probability to a close election after the close poll (compared to the not-close poll). When individuals are Bayesian, this has a much simpler interpretation, which we discuss after providing the formal proposition and proof.

Proposition 2: *Suppose A1 holds and the election is large. If*

$$l(k,k'|N,\frac{1}{2}) > E[l](k,k'|N,\zeta)$$

then observing the close poll, compared to the not-close poll, leads to beliefs that the election is more likely to be decided by less than 1,000 (100) votes.

Proof: The proof proceeds in three steps.

Step 1: First, we prove the proposition for the situation where the beliefs are about the election being exactly tied. The posterior attached to A having 50% of the support after observing the close poll is

$$\frac{\rho(\frac{k}{N}|\frac{1}{2}, N)\gamma(\zeta(\frac{1}{2}))}{\int_{\varsigma'_A} \rho(\frac{k}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))}$$

while the posterior attached to A having 50% of the support after observing the not-close poll is

$$\frac{\rho(\frac{k'}{N}|\frac{1}{2}, N)\gamma(\zeta(\frac{1}{2}))}{\int_{\varsigma'_A} \rho(\frac{k'}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))}$$

Observe that

$$\frac{\rho(\frac{k}{N}|\frac{1}{2}, N)\gamma(\zeta(\frac{1}{2}))}{\int_{\varsigma'_A} \rho(\frac{k}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))} > \frac{\rho(\frac{k'}{N}|\frac{1}{2}, N)\gamma(\zeta(\frac{1}{2}))}{\int_{\varsigma'_A} \rho(\frac{k'}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))}$$

if and only if

$$\frac{\rho(\frac{k}{N}|\frac{1}{2}, N)}{\rho(\frac{k'}{N}|\frac{1}{2}, N)} > \frac{\int_{\varsigma'_A} \rho(\frac{k}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))}{\int_{\varsigma'_A} \rho(\frac{k'}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))}$$

which is the same as

$$l(k,k'|N,\frac{1}{2}) > E[l](k,k'|N,\zeta)$$

Step 2: Now we will prove this for beliefs about the election being decided by less than 100 or less than 1000 votes.

Observe that the posterior belief about the state of the world where the A receives a ς_A percentage of the votes after observing the close poll is

$$\frac{\rho(\frac{k}{N}|\varsigma_A, N)\gamma(\zeta(\varsigma_A))}{\int_{\varsigma'_A} \rho(\frac{k}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))}$$

and after the not-close poll is

$$\frac{\rho(\frac{k'}{N}|\varsigma_A, N)\gamma(\zeta(\varsigma_A))}{\int_{\varsigma'_A} \rho(\frac{k'}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))}$$

Recall, by assumption these posterior beliefs are continuous in ς_A. Fixing the size of the poll at N and letting the expected number of voters become arbitrarily large (i.e., a large election), it is the case that $\sigma_A + \sigma_B \to \infty$. Suppose that we care about states of the world where the election is decided in favor of A by exactly τ votes — so that $\sigma_A = \sigma_B + \tau$. Then as the elections become arbitrarily large $\frac{\sigma_A}{\sigma_B+\sigma_A} \to .5$. Denoting $\varsigma_{A,\tau} = \frac{\sigma_A}{\sigma_B+\sigma_A}$ where $\sigma_A = \sigma_B + \tau$, it is the case that

$$\frac{\rho(\frac{k}{N}|\varsigma_{A,\tau}, N)\gamma(\zeta(\varsigma_{A,\tau}))}{\int_{\varsigma'_A} \rho(\frac{k}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))} \to \frac{\rho(\frac{k}{N}|\frac{1}{2}, N)\gamma(\zeta(\frac{1}{2}))}{\int_{\varsigma'_A} \rho(\frac{k}{N}|\varsigma'_A, N)\gamma(\zeta(\varsigma'_A))}$$

and similarly for the posteriors after the not-close poll. Thus, the conditions that are sufficient in the case when for $\varsigma_A = \frac{1}{2}$ are also sufficient when considering beliefs that the election is more likely to be decided by exactly τ votes.

Step 3: Because for every $\tau \in [-1000, 1000]$ observing the close poll, compared to the not-close polls, leads to beliefs that the election is more likely to be decided by exactly τ votes then it must be the case that observing the close poll, compared to the not-close poll leads to beliefs that it is more likely that the election will be decided by less than 100 or 1000 votes. $\square$

For individuals are Bayesian and so who understand polls are binomial distributions, this proposition takes a simpler form, which we relay below. Moreover, if the poll is sufficiently large and individuals also believe that both the close and not-close polls could be actual realizations of the proportion of A voters in the population then it is *always* the case that observing the close poll, compared to the not-close poll, always leads to beliefs that the election is more likely to be decided by less than 1,000 or 100 votes.

Corollary 1: *Suppose individuals are true Bayesians and the election is large. (i) If*

$$\frac{k!(N-k)!}{k'!(N-k')!} < \frac{\textit{Prior probability of not-close poll}}{\textit{Prior probability of close poll}}$$

then observing the close poll, compared to the not-close poll, leads to beliefs that the election is more likely to be decided by less than 1,000 (100) votes. (ii) Moreover, suppose that $\zeta(\frac{k}{N})$ and $\zeta(\frac{k'}{N})$ are both strictly greater than 0. As N becomes large, observing the close poll, compared *to the not-close poll, always leads to beliefs that the election is more likely to be decided by less than 1,000 (100) votes.*

Proof: First we prove (i). The probability of a close poll (with k A supporters out of N respondents), conditional on a fraction of support ς_A for A amongst actual voters is $\frac{N!}{k!(N-k)!}\varsigma_A^k(1-\varsigma_A)^{N-k}$ (and similarly for a not-close poll).

If $\varsigma_A = .5$ then the above fraction becomes $\frac{N!}{k!(N-k)!}.5^k.5^{N-k} = \frac{N!}{k!(N-k)!}.5^N$, and similarly for k'. Therefore, the previously obtained condition,

$$\frac{\rho(\frac{k}{N}|\frac{1}{2},N)}{\rho(\frac{k'}{N}|\frac{1}{2},N)} > \frac{\int_{\varsigma'_A}\rho(\frac{k}{N}|\varsigma'_A,N)\gamma(\zeta(\varsigma'_A))}{\int_{\varsigma'_A}\rho(\frac{k'}{N}|\varsigma'_A,N)\gamma(\zeta(\varsigma'_A))},$$

becomes

$$\frac{\frac{N!}{k!(N-k)!}}{\frac{N!}{k'!(N-k')!}} > \frac{\text{Prior probability of close poll}}{\text{Prior probability of not-close poll}}$$

Equivalently, this is

$$\frac{k'!(N-k')!}{k!(N-k)!} > \frac{\text{Prior probability of close poll}}{\text{Prior probability of not-close poll}}$$

or

$$\frac{k!(N-k)!}{k'!(N-k')!} < \frac{\text{Prior probability of not-close poll}}{\text{Prior probability of close poll}}.$$

The rest of the proof follows as in the previous proposition. We now turn to (ii) Observe that as N becomes sufficiently large $\frac{k!(N-k)!}{k'!(N-k')!}$ must go to 0 since $k < k'$. Thus, we simply need to verify that $\frac{\text{Prior probability of not-close poll}}{\text{Prior probability of close poll}}$ does not go to 0 (or at least not as quickly). Observe that as N becomes sufficiently large, given any actual $\varsigma_A = \frac{\sigma_A}{\sigma_A+\sigma_B}$, the random variable that is the poll outcome converges to ς_A. Thus, the Prior probability of a close poll is simply the probability that an individual's distribution ζ ascribes to $\varsigma_A = \frac{k}{N}$. Similarly the Prior probability of a not-close poll is the probability that an individual's distribution ζ ascribes to $\varsigma_A = \frac{k'}{N}$. By assumption the ratio of these is a finite number. Again, the rest of the proof proceeds as in previous proposition. □

Thus, Corollary 1 points out that under relatively weak conditions and large polls, Bayesians should always ascribe higher beliefs to the election being decided by less than 100 or 1,000 votes after seeing the close poll, compared to the not-close poll.

Of course, this condition could fail, even when the polls and an individual's prior beliefs agree about the likely winner.[18] Imagine that the polls consist only of 100 people (a small sample size for political polls) and that the close poll is exactly a tie, but the not-close poll is 55 people in favor of A. In this case the left-hand side of the condition becomes approximately

[18]When they do not, counterexamples to our desired result are easy to find. Imagine that an individual's prior puts some weight on the election being a tie and some weight on B winning by a positive margin (and zero weight on all other outcomes). In addition, suppose that both polls predict A to win. Then observing the not-close poll will lead to higher beliefs about a close election.

.61. This means so long as the prior probability of the not-close poll is more than 61 percent as likely as the prior probability of the close poll, observing the close poll will lead to higher beliefs about a close election.

However, even for "normal" sized polls in political contexts our condition is likely to hold. For example, at a poll size of 1,000, that of a typical Gallup poll (http://www.gallup.com/178667/gallup-world-poll-work.aspx), the condition above becomes extremely mild. In this case the left hand side of the inequality falls to less than .01. Thus, only in the case where someone thinks that the probability of the close poll is over one hundred times more likely than that of the distant poll will they place a higher belief on a close election after observing the close poll.

D.2 Beliefs and Actions: Instrumental Private Values Models

We now turn to trying to understand how our treatment would affect observed behavior in the classic private values instrumental voting model. First discussed in voting model introduced by Downs (1957), it was later extended by Ledyard (1981), Ledyard (1984),Palfrey and Rosenthal (1983), and Palfrey and Rosenthal (1985), among others, and has become used in many applications. Our approach attempts to capture a very general form of this model. We want to highlight the fact that beliefs about how likely one is to be decisive affect the decision about whether to vote. In combination with the results in the previous sub-section, this implies that information about decisiveness should affect the decision about whether to vote. Thus we formalize the mapping between an instrumental model of voting with private values and our experiment, allowing us to elucidate the mechanisms that generate Prediction 1.

Formally, voters differ both in their preferred candidate and the strength of their preference. Let θ, drawn from a distribution G, having support $(0, 1)$, be the probability that a voter is an A supporter. Note that if G is non-degenerate then the exact realization of θ is unknown to all voters; thus, the model permits aggregate uncertainty in the sense of Myatt (2015) or Krishna and Morgan (2015). The strength of the preference for voter i, who is an A supporter, is $v_{A,i}$, drawn from a distribution F_A having support $(0, 1)$. Likewise, the strength of preference for voter j, who is a B supporter, is $v_{B,j}$, drawn from a distribution F_B having the same support. The strength of preference represents the difference in a voter's instrumental payoffs from comparing his more to less preferred outcome. In addition to their strength of preference, each voter also has a private cost of voting. Specifically, voter i's cost of coming to the polls is c_i, drawn from a distribution Ξ with support $[0, 1]$. We allow individuals to have incorrect beliefs about the distribution of outcomes — i's subjective prior distributions are denoted $\hat{G}_i$, $\hat{F}_i$ and $\hat{\Xi}_i$.

Voters in the model have two choices: whether to vote and for whom to vote. The latter choice is straightforward. It is a dominant strategy for A supporters coming to the polls to vote for A and likewise for B supporters to vote for B. Thus, conditional on going to the poll, any given individual always votes for one of the two candidates.

The determination of whether to vote is more involved. A voter will choose to participate if and only if her costs are smaller than her expected benefit from voting. The benefit of going to the polls is the utility difference between seeing the favored candidate elected and the other candidate elected, times the probability of the individual's vote actually being decisive. Of course, the probability of being decisive is endogenous, and depends on the turnout rates for both candidate's supporters (which in turn depends on the probability of being decisive).

Moreover, because of the private values setting, any given individual, fixing the actions of everyone else, is concerned only with the induced probability of being decisive. So, fixing a particular strength of preference for one candidate over another, and a cost of voting, an individual will attend the polls if and only if the probability of her vote being decisive in deciding the election is above some threshold.

Formally, in any equilibrium, the cost threshold for an A supporter with a value $v_{A,i}$ is

$$c^*_{A,i}(v_{A,i}) = \frac{1}{2} v_{A,i} E_{\theta|A} \left[\Pr\left[Piv_A|\theta\right]\right].$$

Here, the expression Piv_A denotes the set of events where an additional vote for A proves decisive; i.e., when the vote is either tied or candidate A is behind by one vote. The fraction $\frac{1}{2}$ in the above expression represents the fact that when the candidates are tied there is only a 50 percent probability that candidate A is chosen. Thus, if the election were tied and voter i cast the decisive vote, candidate A's chances of winning would rise from 50% to 100%. Similarly if candidate A is behind by one vote and voter i casts the decisive vote then A's chances of winning rise from 0 to 50 percent. The chance of casting a decisive vote depends on, among other things, the fraction of A supporters in the population, θ, which may be unknown. Thus an A supporter conditions on her preference, since this is informative about the realization of θ. Likewise, for voter j favoring B, we have

$$c^*_{B,j}(v_{B,j}) = \frac{1}{2} v_{B,j} E_{\theta|B} \left[\Pr\left[Piv_B|\theta\right]\right].$$

Notice that the participation rates for both types of voters determine the pivot probabilities. Thus, an equilibrium consists of a set of participation rates together with associated pivot probabilities such that the above equations are satisfied for all voters. Rather than solving for equilibrium threshold functions, $c^*_A(v_A)$ and $c^*_B(v_B)$, it is more convenient to express equilibrium in terms of average participation rates for A and B supporters. First, note that the participation rate for an A supporter with strength of preference v_A is $\Xi\left(c^*_A(v_A)\right) = \Xi\left(\frac{1}{2} v_{A,i} E_{\theta|A}\left[\Pr\left[Piv_A|\theta\right]\right]\right)$. Integrating over the distribution F_A we obtain an average participation rate for A supporters given by

$$P_A = \int \Xi\left(\frac{1}{2} v_{A,i} E_{\theta|A}\left[\Pr\left[Piv_A|\theta\right]\right]\right) f_A(v_A) dv_A. \tag{1}$$

Likewise, the average participation rate for B supporters is

$$P_B = \int \Xi\left(\frac{1}{2} v_{B,j} E_{\theta|B}\left[\Pr\left[Piv_B|\theta\right]\right]\right) f_B(v_B) dv_B. \tag{2}$$

Again, we allow subjective distributions for individual i to be denoted by $\hat{P}_{A,i}$ and $\hat{P}_{B,i}$ (consistent with $\hat{F}_{A,i}, \hat{F}_{B,i}$).

Of course, participation rates themselves do not determine an election outcome. What matters is the interaction between participation rates and the fraction of A and B supporters in the population. Let σ_A denote the realized number of A votes cast and let σ_B be defined likewise. Because abstention is allowed, it will typically be the case that $\sigma_A + \sigma_B < m$. Recall that individuals care about the probability of being pivotal. In other words, a supporter of candidate A cares about $\Pr[\sigma_A = \sigma_B|A] + \Pr[\sigma_A = \sigma_B - 1|A]$ (conditional on the fact that they exist).

For example, if an individual is a Bayesian, the probability of an election being exactly tied would be $\int_\theta \sum_m \sum_{t=0}^{\lceil \frac{m}{2} \rceil} (P_A\theta)^t (P_B(1-\theta))^t h(m) g(\theta|A) d\theta$. Similar calculations can be made for the election being decided by a single vote. Of course, we allow non-Bayesian beliefs, so individuals may have a different way to construct beliefs about pivotality.

If pivotal beliefs are formed using Bayes' Rule, and individuals have common (correct) priors, then it can be readily shown that an equilibrium exists; it reduces to the problem of finding P_A and P_B satisfying the above equations. The existence of the equilibrium depends on the following relationship: when voters obtain information they adjust their beliefs about the likelihood that they are pivotal. Precisely how this adjustment takes place depends on the nature of the information received, but, in general, information suggesting that the election will be closer tends to raise beliefs about the likelihood of being pivotal, while information suggesting the election is less competitive will tend to lower them. The decision as to whether to come to the polls hinges on a voter's belief about the chance that she is pivotal; thus, information that alters these beliefs in turn alters voters' choices. Information that produces higher probabilities of being pivotal in the mind of the voter will tend to raise participation, while information that lowers this chance will tend to reduce it.

As the above reasoning should make clear, Bayesian beliefs are not necessary for constructing an equilibrium. Voters may have non-Bayesian beliefs, for instance, a non-belief in the Law of Large Numbers, as in (Benjamin et al., 2016), and use them to determine pivot probabilities. So long as the resulting participation rates are consistent with equations (1) and (2) the model is also internally consistent.

Thus, equilibrium existence follows from standard fixed point reasoning so long as pivot probabilities move continuously with P_A, P_B and the distribution of θ. To guarantee positive participation rates it must be the case that when $P_A = 0$ (resp. $P_B = 0$), an A supporter (resp. B supporter) perceives the chance of being pivotal as non-negligible. For instance, if the voting population is Poisson distributed, this follows as a consequence of the fact that even when B supporters participate at positive rates, there is (small) chance that there will be only 0 or 1 voters; hence, an intervention by an A supporter would be decisive.

As mentioned, our experimental variation does not concern equilibrium outcomes, but rather the best response function of any individual voter, and so we focus on formal results regarding the comparative statics of this function. As Lemma 1 notes, all else equal, the higher the perceived pivot probability, the more likely an individual is to participate.

Lemma 1 *The more likely a voter believes she is pivotal, the more likely she is to vote.*

Proof. Observe that $\frac{\partial c^*_{A,i}(v_{A,i})}{\partial E_{\theta|A}[\Pr[Piv_A|\theta]]} = \frac{1}{2} v_{A,i} > 0$ for A supporters, and similarly for B supporters. ■

We cannot directly test Lemma 1, as we do not observe an individual's perceived probability of being pivotal, as mentioned in the body of the text. Instead, we elicited proxies for pivotality: the predicted margin of victory (i.e., $\frac{\int|\theta P_{A,i}-(1-\theta)P_{B,i}|g(\theta)d\theta}{\int\theta P_{A,i}+(1-\theta)P_{B,i}g(\theta)d\theta}$),[19] the probability of the election being decided by less than 1000 votes (i.e., $\Pr[|\sigma_{A,i}-\sigma_{B,i}|\leq 1000]$) and the probability of the election being decided by less than 100 votes (i.e., $\Pr[|\sigma_{A,i}-\sigma_{B,i}|\leq 100]$).[20] The following assumptions connect these observed measures to perceived pivot probabilities. Because the assumptions on the margin of victory being less than 100 or 1,000 votes are so similar, we state them as a single assumption.

A4: *A voter has a smaller predicted margin of victory (i.e.,* $\frac{\int|\theta P_{A,i}-(1-\theta)P_{B,i}|g(\theta)d\theta}{\int\theta P_{A,i}+(1-\theta)P_{B,i}g(\theta)d\theta}$*) if and only if she has a higher probability of being pivotal (i.e.,* $\Pr[Piv_{A,i}]$ *and* $\Pr[Piv_{B,i}]$ *).*

A5: *A voter has a larger probability of the election being decided by less than 1000 votes,* $\Pr[|\sigma_{A,i}-\sigma_{B,i}|\leq 1000]$ *(100 votes,* $\Pr[|\sigma_{A,i}-\sigma_{B,i}|\leq 100]$*), if and only if she has a higher probability of being pivotal (i.e.,* $\Pr[Piv_{A,i}]$ *and* $\Pr[Piv_{B,i}]$*).*

While these assumptions seem reasonable, they entail interpersonal comparisons, which will be colored by differences in the perceived distribution of θ, G, and so they are assumptions rather than results. To see how the assumptions might fail to hold, consider two voters i and j. Suppose that i views the distribution of θ as producing outcomes where candidate A enjoys either a 50% vote share or a 90% vote share. Voter j, on the other hand, sees θ as approximately degenerate, producing a 55% vote share for A. Voter j will report a smaller margin of victory for A but will have a lower perceived pivot probability.

Lemma 1 represents the central prediction of pivotal voting models — the link between beliefs about pivotality and consequent participation. Assumptions A4-5 allow us to connect observables in terms of beliefs to observables in terms of actions.

Proposition 3: *Suppose A4 holds. The smaller a voter's predicted margin of victory the more likely she is to vote.*

Proposition 4: *Suppose A5 holds. The larger a voter's belief about the election being decided by less than 1,000 (100) votes, the more likely she is to vote.*

Propositions 3 and 4 are true in any private values instrumental model of voting where A4 and A5 hold, as they rely only on those two assumptions and Lemma 1.

The propositions in this sub-section and the previous one have separately related beliefs to actions and information to beliefs. They now allow us to derive a proposition that directly leads to Prediction 1, allowing us to link information, action and beliefs to one another.

Proposition 5: *Suppose A1-A5 hold; then all else equal, observing a close poll (relative to a not-close poll) leads to a higher chance of turning out to vote.*

Proposition 5 is an immediate result of linking Propositions 1 and 2 to 3 and 4. Moreover, it goes to the heart of models of instrumental voting. Information, from polls and elsewhere, alters a voter's calculus of the value of voting by influencing her beliefs about the likelihood of close elections and hence the likelihood of her vote mattering. This, in turn, affects the decision to turn out. In other words, differences in information contained in the close and not-close polls affect the chance of voting.

[19] Participation rates and pivot probabilities depend on what type of individual is considering this, which we suppress for expositional ease.

[20] Again, for notational ease, we repress the conditioning on the voter's type.

D.3 Information and Common-Values Instrumental Voting

As discussed in the body of the paper, in models, such as Feddersen and Pesendorfer (1996, 1997), information may not only change the perceived probability of being pivotal, but also the perceived utility gap between the candidates, i.e. their valence. Although many of these models typically suppose individuals can costlessly vote, their implications extend to models with costly voting, as in Krishna and Morgan (2012).

We can modify our basic model to formalize and incorporate such considerations (in a reduced form manner). We denote the individual's (expected) value of voting for candidate A, instead of B, as $E[\hat{v}_{A,i} - \hat{v}_{B,i}|I, Piv_A]$, which is the expected utility difference between the candidates, conditional on an individual's information I, and being pivotal (recall we denote this event as Piv_A). Thus her estimated value of voting for candidate A over candidate B is

$$E[\hat{v}_{A,i} - \hat{v}_{B,i}|I, Piv_A]\frac{1}{2}E_{\theta|A}\left[\Pr\left[Piv_A|\theta\right]|I\right]$$

The first term represents the estimated benefit of candidate A over candidate B, but this is conditional on both the information conveyed by the poll, and the event that the voter is pivotal. As in the private values model, voters whose cost falls below this level will vote, and otherwise will abstain.

This class of models typically supposes there are partisans, who have purely private values and fixed preferences over the candidates, as well as independents. The latter have both a private values component to their payoff (i.e. they receive utility from seeing the candidate closer in ideology elected), and also have a common values component to their payoff. The common values component depends on two objects: the state of the world and the elected candidate. There are two potential states of the world, and the realized state is unknown by voters (who have a common prior over each state). Depending on the state, independents believe a different candidate should be elected — in other words they want to match the candidate to the state. Each independent voter receives a conditionally i.i.d. signal which is partially informative about the state of the world. Some independents receive stronger signals than others (in fact, some independent voters may receive an entirely uninformative signal).

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 differences, and, of course, the likelihood 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 1 vote is less likely to be decisive.

What information independents infer (as partisans will behave exactly as in the private values model) from observing different polls may depend on what they think is driving the differences in the poll results. If they believe the difference between the close and not-close polls is driven by informed independent voters, then they should exhibit a stronger preference for the candidate that is favored by both polls after observing the not-close poll (compared to the close poll). This is because observing that candidate A is farther ahead implies that more informed voters received a signal saying that A is the better candidate given the state. Thus, they should be more willing to support A after observing a not-close poll in favor of A.

In contrast, if the independents believe that the difference in the poll results is driven by partisans, then they should exhibit a shift preference towards the less favored candidate in the polls. This is because if there are more A partisans, then, conditional on being pivotal, there must have been many informed voters who voted for B. This indicates that it must be the case that B is the candidate that matches the realized state. Thus, an independent voters should exhibit a stronger preference for B, even though A is favored in the polls.

Observe that either effect could imply that individuals should have stronger preferences for one candidate or the other after observing a not-close poll (relative to a close poll). This increases the benefit to voting. Since observing the distant poll also reduces the chance of being pivotal, we could observe no net change in the benefit of voting after observing the not-close poll (relative to the close poll). Thus, although our treatment changed beliefs in the intended fashion, it could have also changed preferences in the opposite direction. The net effect on the decision of whether to turn out or not would then be zero.

In the body of the paper, we supposed that the private value (i.e., ideological) component, dominates the common values (i.e., valence) component. This implies that individuals will never change who they would vote for (based off their private signal or poll results), but may change whether they go to vote or not based on information. As discussed, suppose an individual supports the candidate with the minority of the overall support in the population, (call this is candidate B). 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, and so a B voter should be more likely to turn up and vote. However, for A voters, the two motives move in opposite directions, and so our prediction does not apply to them.

More generally, ideology may not dominate valence in many circumstances. However, the model still predicts that conditional on an individual's perceived valuation different between candidates being invariant to the poll result, they should behave as in a private values setting. Observed preferences may not shift for a variety of reasons: for example, the common values component is extremely small relative to the private values component (so that voters are essentially partisan), or because they are unsure of whether the poll results are drive by partisans or informed voters, and so do not adjust their preferences at all. Prediction A1 summarizes this intuition.

Prediction A1: *All else equal, if preferences do not change after observing the close poll, compared to the not-close polls, then observing the former (rather than the latter) leads to a higher chance of voting (versus abstaining).*

D.4 Information and Signaling

Another influential thread of the literature focuses on voting as a way of signaling private information. In some of these models, like Piketty (2000), individuals want to coordinate on future vote outcomes. In others, like Shotts (2006), Meirowitz and Shotts (2009) and Hummel (2011) they want to influence outcomes and candidate positions in future elections. In Razin

(2003) voters want to influence the ex-post policy decisions of officials. Thus, voting has value not only because it can serve to elect the right candidate, but also because it can convey private information, whether to politicians or to other voters about the correct (future or current) policy.

Thus, individuals may vote against their favored candidate to provide information about current or future policy positions. For example, voting for candidate B may not be an expression of support for B's position, but rather an expression that candidate A should moderate their position, conditional on winning. Thus. observing different polls can change beliefs about what the correct policy should be (whether for this election or a future election), and about the value of voting (because it may change the extent to which policy is altered). Thus observing different polls may change the signal any given voter will try to convey, or the value of conveying that signal. The signal that is observed post-vote consists of the number of individuals who voted for candidate A, candidate B, or abstained.

In the body of the paper, we discussed how given that policies are more sensitive to vote share in close elections than landslides, then Prediction 1 will hold: A voter observing a close poll recognizes that a vote for their preferred candidate has more impact on the desired candidate and policy than does a distant poll.

Of course, this may not happen. However, we can check to see whether voters signaled differently when they observed different polls. As mentioned, voters have only two mechanisms by which to convey information. Either they can change whether they actually vote or not (which shifts the number of abstainers), or they can change who they vote for conditional on actually voting (which shifts the number of A versus B votes). Moreover, as Hummel (2011) demonstrates, under reasonable assumptions, if individuals have both signaling and pivotality motives, in large elections the signaling motive dominates. Thus, as Prediction A2 summarizes, we would expect that in large elections if the optimal signal changes with beliefs about the closeness of the election, we would expect different behavior to occur when individuals observe different polls.

Prediction A2: *If the optimal signal shifts with beliefs about the closeness of the election, then observing the close poll, compared to the not-close polls, must either affect the probability of voting, or conditional on voting, the probability of voting for one candidate or the other.*

D.5 Information and Ethical Voting

We turn now to the approach to ethical voting developed in Feddersen and Sandroni (2006). Their model features "private values" preferences. Thus, there are individuals who have a preference for A and those who have a preference for B. They fix the utility gap between the candidates, making homogeneous across individuals and symmetric across supporters of either candidate. Moreover, as in the private values model, potential voters face an idiosyncratic cost of voting.

Voters also differ in a second dimension; they are either "abstainers" or "ethicals." Abstainers will never vote. Ethicals receive a payoff for following a rule-utilitarian strategy. The rule utilitarian strategy would be the optimal strategy for an individual to follow supposing that all ethical types who supported the same candidate also followed that strategy. This takes the form of a threshold strategy: ethicals should vote if their voting cost is below some threshold, and should otherwise abstain. If an ethical follows the optimal rule (for their type), they receive an additional payoff. When evaluating a pair of rules (one for each type), an agent's individual payoff is determined by the probability that their favored candidate wins, times the strength of preference (just as in the standard model), less the expected voting cost to all of society. A consistent pair of rules (the analogous outcome to an equilibrium) is a set of rule-utilitarian strategies that are best responses to one another.

Since ethical voters receive an exogenous payoff by following the rule-utilitarian strategy, they do not directly care about their own pivotality. However, they still may alter their behavior in response to information. If the poll conveys information about the distribution of voters' preferences in society, which Feddersen and Sandroni (2006) denote k, then Property 3 of Feddersen and Sandroni (2006) shows that more skewed distribution of preferences towards one candidate should lead to a higher margin of victory (and so a more skewed poll result). Moreover, as they point out, a more skewed distribution of preferences should also lead to a lower turnout. Thus, we would expect subjects shown a poll with a larger margin of victory to have a lower turnout.[21] Thus, Property 3 of Feddersen and Sandroni (2006) leads to Prediction 1, albeit via a different mechanism (margin of victory rather than pivotality).

D.6 Calibration

We calibrate a simple private values instrumental voting model in order to provide a sense of what size of an effect we might expect our treatment to have. Our main finding is that belief changes of the order observed in our study would generate turnout impacts on the order of roughly 5-7pp in the context of the model. We calibrate our model using the actual belief and turnout data we observe in our sample, rounded to improve clarity of the exposition.

As mentioned in the paper, the quantitative size of these effects is very dependent on the particular assumption of the model. However, we believe such an exercise, properly caveated, can provide a useful framework for understanding his effects.

We first discuss "baseline" calibrations using beliefs about the probability of the election being decided by less than 100 votes. We then discuss how an analogous exercise generates results using beliefs about the probability of the election being decided by less than 1,000 votes.

Both of these baseline calibrations include strong distributional assumptions, which although plausible for establishing a base case, are likely to be violated. We thus then turn to discussing to what extent our results will change as we vary these assumptions.

Key to our results is the our treatment should shift beliefs about pivotality by approximately 10%. Given our assumption about the uniform distribution about the ratio of costs relative to utility difference between candidates, we would expect a proportionate change in the turnout. More generally, that the base rate of turnout approximately 70%, so long as the distribution of costs relative to utility differences have reasonable mass around the 70th percentile, we should expect our change in beliefs to generate non-negligible shifts in turnout.

We also want to highlight that our calibration does not fly in the face of the intuition

[21] Feddersen and Sandroni (2006) have 4 other potential exogenous variables that also may affect margin of victory: the cost of voting, fraction of ethical voters, importance of election and the value of doing one's duty. However, we think that these are less plausible factors that individuals would perceive aggregate uncertainty about.

that private values instrumental models with perfectly rational voters have trouble generating reasonable turnout rates. This intuition relies on the fact that the probability of being pivotal is so small in large elections that the utility difference between candidates must be incredibly large relative to costs. In our calibration, we take as given individuals' reported beliefs about close elections, which as discussed previously, are orders of magnitude too large, and so require much more reasonable ratios than the standard instrumental voting models. Moreover, we do not actually identify the relative size of the costs and utility differences between candidates. We simply suppose that they take on a distribution that can rationalize the observed turnout given the reported beliefs. Given this, we then estimate the effect of our treatment. The calibration, by its nature, takes no stance on how reasonable the relative sizes of the costs and utility differences between candidates are.

Main Calibration. First, observe that in large elections, so long as the pdf of beliefs about election outcomes is reasonably smooth, the probability of an election being decided by 1 vote (i.e., a given vote is pivotal) is approximately $\frac{1}{100}$s of the probability of election being decided by less than 100 votes.[22]

We will suppose, in our baseline case, that individuals all have homogeneous beliefs. Although this is not actually true, it simplifies the calibration. We discuss what happens when we relax this assumption below. Recall, in our data, the mean belief about the probability of an election is 25%.[23] This implies the probability of being pivotal is approximately 0.0025.

Suppressing notation, recall that an individual will vote if and only if the cost of voting (e.g., c) is less than the utility gap between candidates (e.g. v), times .5, times the probability of being pivotal: $c \leq \frac{1}{2}vE[\Pr[Piv]]$; in other words $\frac{c}{v} \leq \frac{1}{2}E[\Pr[Piv]]$.

Of course, both costs and utility differences may be distributed in a variety of ways, leading to a variety of distributions regarding their ratios. Denote this distribution $F^{c,v}$. Observe that the turnout rate will then be equal to the cdf attached to $\frac{1}{2}E[\Pr[Piv]]$: $F^{c,v}(\frac{1}{2}E[\Pr[Piv]])$.

For simplicity, we will suppose that $F^{c,v}$ takes on a uniform distribution. Moreover, in line with the instrumental values literature we will suppose that no individuals have a negative cost of voting; in particular the lower bound of $F^{c,v}$ is 0. Thus, the CDF attached to any given number x is $\frac{x}{\bar{x}}$, where $\bar{x}$ is the upper bound of the distribution.

In the 2010 data, turnout is approximately 0.7; thus, $\frac{.00125}{\bar{x}} = .7$ or $\bar{x} = .001786$

In order to assess the predicted effect of our treatment on turnout, recall that our treatment shifted beliefs by about 2.5 pp. This shifts the right hand side of our turnout

[22]To see this, consider a continuous, differentiable function, that for each potential victory margin in $[-100\%, 100\%]$ tells us the probability assigned to that particular victory margin. For a large election, the region consisting of the election being decided by less than 100 votes is negligible, and thus, in this region, the function is approximately linear. Thus, so long as the actual discrete distribution is "close" to this function, a linear approximation of the pdf will be correct. Thus the pdf, in the region between the vote margin being -100 votes and +100 votes, takes on the form intercept + slope $*$ (vote margin + 100). Denoting the vote margin as vm, the total probability in this region is $201 *$ intercept + slope $\sum_v mvm + 100 * 201$; and the average probability is intercept + slope $*$ 100. Moreover, the probability of a tie election is also intercept + slope $*$ 100: i.e. $\frac{1}{201}$ of the probability of the election being decided by less than 100 votes. Moreover, so long as the slope is not too large the probability of the election being decided by exactly 1 vote is approximately the same as a tie election. This gives us approximately $\frac{2}{201}$, which we round to $\frac{1}{100}$.

[23]We calibrate here using the mean pre-treatment results. Using post-treatment average beliefs gives approximately the same results. Using median beliefs to calibrate the model, either regarding the election being decided by less than 100 or 1,000 votes, generates even larger estimates of the effect of turnout.

equation from .00125 to .001375. Turnout should then be $\frac{.001375}{.001786} = .7699$.

Using this particular belief data, we would then predict turnout should have increased by almost 7 pp from our treatment.

We can repeat the same exercise using the data on beliefs about the election being decided by less than 1,000 votes. Here the mean belief was about 30%; Thus, the right hand side of the turnout equation becomes .00015, and so $\bar{x} = \frac{.00015}{.7}$; or $\bar{x} = .000214$. The treatment shifted beliefs about 2.3 pp. This changes the right hand side of the turnout equation to .0001615, and so expected turnout should be $\frac{.0001615}{.000214} = .7547$. Therefore, this belief data predicts turnout should have increased by about 5 pp from our treatment.

Thus, using either belief data gives relatively consistent results: our treatment should have increased turnout by 5-7pp.

Relaxing assumptions. Of course, these estimations depend on two key distributional assumptions. First, we supposed that the distribution of the ratio of costs to utility differences, $F^{c,v}$, was uniform and had a lower bound of 0. The second is we supposed that all individuals had the same beliefs.[24] We discuss relaxing each of these in turn.

Our assumptions regarding the distribution of $F^{c,v}$ are not particularly restrictive. Many other assumptions regarding $F^{c,v}$ will generate similar results: for example, supposing that the distribution is normally distributed with 95% of the mass lying about 0 will generate very similar results.

The key intuition that drive our sizable responses is that our treatment causes a 7 to 10 percent change in beliefs about pivotality. Given that the initial beliefs caused a turnout rate of 70 percent, along with the assumption that almost all voting costs should be positive, implies that unless the distribution of the ratio of costs to utility differences features a negligible fraction of individuals around the 70th percentile, we should observe a reasonably large shift in behavior.

There are types of distributions of $F^{c,v}$ that could rationalize what we observe: for example if the $f^{c,v}$ was u-shaped, with the minimum around the 70th percentile, then our shifts in beliefs may have a much smaller effect.

We could also suppose that some individuals have negative costs of voting. In fact, one way of rationalizing our data would be that many individuals have negative voting costs; i.e. $F^{c,v}$ places significant weight on negative values. However, these voters would then have an "expressive" value from turning out. Thus, this would be tantamount to supposing that we would see small effects if we thought many people had expressive value to turning out, not a particularly surprising result.

The second assumption that we make is that individuals are homogeneous in their beliefs: they all have the same probabilities, and the treatment has the same affect on all individuals. In our actual data we observe both wide variation in the beliefs of individuals as well as the extent to which they update.

Thus, we could relax our baseline assumption and allow for individuals to have a distribution of initial beliefs, along with a distribution of responses to our treatment, subject

[24] Our assumptions are not without parallels in the existing empirical literature on voting. Coate and Conlin (2004) and Coate et al. (2008) suppose that costs are uniformly distributed and that all individuals who support a candidate experience have the same v, leading to our assumption that the ratio has a uniform distribution. DellaVigna et al. (2017) suppose that the difference between v and c is normally distributed.

to the restriction that the mean initial beliefs and mean response must have a mean equal to that observed in the data. We also suppose that these two variables are independently distributed. With (i) a large number of voters, (ii) a uniform distribution of $F^{c,v}$, and (iii) restricting initial beliefs, as well as post-treatment beliefs, to be bounded away from 0 and 100 percent, then allowing for heterogeneity will not change the responses. This is because with a uniform distribution, for example, shifting half of the individuals beliefs by 5% and half the individuals by 0% implies twice the behavioral response for the first group compared to a belief shift in 2.5%, and no behavioral response for the second group, leading to exactly the same behavioral response. More generally, since the CDF is linear the mean turnout rate will be the turnout rate of the mean. Of course, if we also suppose that the distribution is not uniform, then allowing for heterogeneity could change our results; although in order to generate the small behavioral shifts we observe the heterogeneity needs to impose that almost all individuals have negligible shifts in beliefs.

D.7 Is an Instrumental Model with Observed Beliefs Capable of Rationalizing the Observed Level of Treatment?

In Appendix D.6 above, we discussed the issue of whether our experiment was theoretically capable of generating significant treatment effects under an instrumental voting model with observed beliefs. We showed that the model predicts a treatment of 5-7pp.

A different question is whether the model is capable of rationalizing the observed level of turnout. Put differently, what is the ratio of voting costs to voting benefits that would be required to rationalize the level of turnout, and is that reasonable?

In Appendix D.6, we calculated $\bar{x} = 0.001786$, i.e., the upper end of the ratio of costs to benefits, which we assume is uniformly distributed. Multiplying this by the voting rate of 0.7 for the 2010 experiment, we obtain the voting cost to voting benefit ratio for the marginal voter. This is 0.7*0.001786=0.00125, or 1 to 800.

Whether or not this is a reasonable number is admittedly somewhat subjective. Still, we can look to the literature for guidance on whether this is a reasonable number or not. In the context of a recent, detailed, theoretical analysis of instrumental voting, Myatt (2015) discusses a benefit to cost ratio of 2500:1 as being reasonable.

Our model set-up assumes that voters are purely selfish. If voters were allowed to have a small amount of altruism as in Edlin et al. (2007) or Evren (2012), then the required benefit to cost ratio would be less than 800:1.

What if voters are risk-averse? Because our calibration uses the observed data to infer the size of the effect our treatment should have had under the null of the private value instrumental model being correct, we do not need to make any direct assumptions about the relative sizes of the costs and benefits, nor the risk attitudes of the subjects. Our model implicitly identifies the ratio of costs to benefits, in utils, from the data.

E Documents for the Experiments

E.1 Screenshots for the 2010 Experiment, Pre-Election Survey

We will now ask you questions about the upcoming November election for the governor of Oregon. The elections will be held on Tuesday, November 2nd, 2010.

As of today, have you already voted in the November elections, for example, by absentee ballot or early voting?

Select one answer only

- Yes
- No

Next

How interested are you in information about what's going on in government and politics? Extremely interested, very interested, moderately interested, slightly interested, or not interested at all?

Select one answer only

- Extremely interested
- Very interested
- Moderately interested
- Slightly interested
- Not interested at all

Next

How often would you say you vote? Seldom, part of the time, nearly always, or always?

Select one answer only

- ○ Seldom
- ○ Part of the time
- ○ Nearly always
- ○ Always

Next

What job or political office is held by Nancy Pelosi?

Select one answer only

- ○ U.S. Secretary of State
- ○ U.S. Secretary of Labor
- ○ U.S. Secretary of Homeland Security
- ○ Speaker of the U.S. House of Representatives
- ○ Majority Leader of the U.S. Senate

Next

In the election for governor, of the people voting for either the Democatic or Republican candidates, what share do you predict will vote for the Democratic candidate and what share do you predict will vote for the Republican candidate?

Type in the answer into each cell in the grid

	%
John Kitzhaber (Democrat)	
Chris Dudley (Republican)	
Total	0

Please make sure these numbers add up to 100%.

Next

Many of the next questions ask you to think about the **percent chance** that something will happen in the future.

The **percent chance** can be thought of as the number of chances out of 100. You can use any number between 0 and 100 (including 0 and 100).

For example, numbers like:
1 and 2 percent may be "almost no chance",
20 percent or so may mean "not much chance",
a 45 or 55 percent chance may be a "pretty even chance",
80 percent or so may mean a "very good chance",
and a 98 or 99 percent chance may be "almost certain"

Next

What do you think is the percent chance that you will vote in this year's election for governor?

Type in the number for the answer

[ ] %

Next

If you do vote in this year's election for governor, what do you think is the percent chance that you will vote for the following candidates:

Type in the answer into each cell in the grid

	%
John Kitzhaber (Democrat)	
Chris Dudley (Republican)	
Someone else	
Total	0

Note: This question asks about your chances of voting for the different candidates; it is not the same question as the previous one on predicting vote shares.

Next

What do you think is the percent chance the election for governor will be decided by 1000 or fewer votes?

Type in the number for the answer

[ ] %

Next

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:

John Kitzhaber (Democrat):	**51%**
Chris Dudley (Republican):	**49%**

Next

We would like to again ask you some of the same questions we did above:

Next

In the election for governor, of the people voting for either the Democatic or Republican candidates, what share do you predict will vote for the Democratic candidate and what share do you predict will vote for the Republican candidate?

Type in the answer into each cell in the grid

	%
John Kitzhaber (Democrat)	
Chris Dudley (Republican)	
Total	0

Recent Poll Results:

John Kitzhaber (Democrat):	**51%**
Chris Dudley (Republican):	**49%**

Next

What do you think is the percent chance that you will vote in this year's election for governor?

Type in the number for the answer

[ ] %

Recent Poll Results:

John Kitzhaber (Democrat): 51%
Chris Dudley (Republican): 49%

Next

If you do vote in this year's election for governor, what do you think is the percent chance that you will vote for the following candidates:

Type in the answer into each cell in the grid

	%
John Kitzhaber (Democrat)	
Chris Dudley (Republican)	
Someone else	
Total	0

Recent Poll Results:

John Kitzhaber (Democrat): 51%
Chris Dudley (Republican): 49%

Next

What do you think is the percent chance the election for governor will be decided by 1000 or fewer votes?

Type in the number for the answer

[ ] %

Recent Poll Results:

John Kitzhaber (Democrat):	**51%**
Chris Dudley (Republican):	**49%**

Next

E.2 Body of 2010 Experiment Follow-up / Reminder Email

Thank you for participating in our recent survey about the upcoming governor's election. Your participation is very important and helps us learn about what people are thinking. In case you wish to take a look again at the poll numbers we showed you last time, we included them below.

Poll Results:

John Kitzhaber (Democrat): 51
Chris Dudley (Republican): 49

E.3 Screenshots for the 2010 Experiment, Post-Election Survey

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:

Type in the answer into each cell in the grid

	%
Between 0 and 200 heads:	
Between 201 and 400 heads:	
Between 401 and 480 heads:	
Between 481 and 519 heads:	
Between 520 and 599 heads:	
Between 600 and 799 heads:	
Between 800 and 1,000 heads:	
Total	0

Please make sure your answers add up to 100 percent. Also, please try not to spend more than 1 minute on this question.

Next

Which one of the following best describes what you did in the recent elections that were held November 2nd, 2010?

Select one answer only

- I did not vote in the elections
- I voted in person at a polling place on election day.
- I voted in person at a polling place before election day
- I voted by mailing a ballot to elections officials before the election
- I voted in some other way

Next

Did you vote for governor in the November 2010 election?

Select one answer only

- ○ Yes
- ○ No

Next

Which candidate did you vote for?

Select one answer only

- ○ John Kitzhaber (Democrat)
- ○ Chris Dudley (Republican)
- ○ Someone else

Next

Did you vote for senator in the November 2010 election?

Select one answer only

- Yes
- No

Next

Which candidate did you vote for?

Select one answer only

- Ron Wyden (Democrat)
- Jim Huffman (Republican)
- Someone else

Next

After taking our pre-election survey, did you start to pay less, more, or the same attention to the campaigns? Which of the following best describes you?

Select one answer only

- I paid more attention to the campaigns.
- My attention to the campaigns did not change.
- I paid less attention to the campaigns.

Next

On the day that you voted or decided not to vote, would you have remembered the poll numbers we showed you in the pre-election survey, if someone had asked you about them?

Select one answer only

- Yes
- No

Next

Do you happen to remember the poll numbers we showed you in the pre-election survey about the race for governor. Please enter your best recollection:

Type in the answer into each cell in the grid

	%
John Kitzhaber (Democrat)	
Chris Dudley (Republican)	
Total	0

Please make sure your answers add up to 100 percent.

Next

E.4 Postcard for the 2014 Experiment

VOTING
2014
COUNTS

Voting Counts 2014
P.O. Box 310
Wallingford, CT 06492

PRESORTED
FIRST CLASS
US POSTAGE
PAID
XXXXXXXXX
XXXXXXXXX

<salutation>
<maddress>
<mcity>, <mstate> <mzip5>-<mzip4>

VCT14_010

THE ELECTION ON NOVEMBER 4 IS COMING UP

Below are the results of one recent poll about the race for <office> in <state>. The poll was conducted by a leading professional polling organization. People were interviewed from all over <state>, and the poll was designed to be both non-partisan and representative of the voting population. Please keep in mind that this is just one poll. Polls such as these are often used in forecasting election results.

Of people supporting either of the two leading candidates, the percent supporting each of the candidates was:

<cand1> - <party1>: <poll1>
<cand2> - <party2>: <poll2>*

It's never known for sure how many people will vote in any election. However, one election expert expects that roughly <TO> will vote in the upcoming election.

We hope you decide to participate and vote this November!

*Source: The calculation of the share of respondents that prefer each of the two leading candidates among those who prefer one of the two leading candidates is based on <pollcite>.

Appendix References

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